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"*To the solid ground*
Of Nature trusts the mind which builds for aye."—WORDSWORTH

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NATURE

A WEEKLY ILLUSTRATED JOURNAL OF SCIENCE.

*"To the solid ground
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THURSDAY, NOVEMBER 7, 1889.

TWENTY YEARS.

A REMINDER that to-day is the twentieth anniversary of the first issue of NATURE, will not, perhaps, be without interest to our readers, and certainly affords food for reflection to those who in various capacities have been more or less closely connected with this journal from the first.

"When another half-century has passed," said Prof. Huxley in our first number, "curious readers of the back numbers of NATURE will probably look on *our* best 'not without a smile.'"

It will probably be so, but though twenty years is hardly a sufficient interval to make our smiles at our earlier efforts supercilious, it is enough to test whether progress has been made, and whether the forward path is pursued with growing or with waning force.

As regards this journal itself, we may claim that it has not disappointed the hopes of its founders, nor failed in the task it undertook; and we make this claim all the more emphatically because we feel that what has been accomplished has not been due to our own efforts so much as to the unfailing help we have always received from the leaders in all branches of natural science. This help has not been limited to their contributions to our columns, but has consisted also of advice and suggestions which have been freely asked and as freely given. Not the least part of our duty, and even privilege, to-day is to state openly how small our own part has been, and to render grateful thanks to those to whom it is chiefly due that NATURE has a recognized place in the machinery of science, and has secured an audience in all parts of the civilized world.

We do not wish, however, to narrow our retrospect of the last twenty years by confining our attention to the measure of success which these pages have won. It has been attained, as we have shown, by the aid of nearly all the best-known scientific writers and workers, not in Britain only but in many countries old and new; and we cannot believe that they would thus have banded themselves together if evidence had not been given of an honest desire for the good of science and for the "promotion of natural knowledge," or if the attainment of these objects had not been regarded by us as of more importance than a journalistic success. Thus, on its twentieth birthday, we would think not so much of the growth of NATURE as of the advance which in the last twenty years it has chronicled.

A formal history of science for that period would be a formidable task, but it is already possible to discern what will probably appear to posterity to be the most salient characteristics of the last two decades.

In the physical sciences, the enormous development of the atomic theory, and the establishment of a connection between the theories of electricity and light, are perhaps the two main achievements of the years we are considering. Methods of accomplishing the at first sight impossible task of measuring atomic magnitudes have been devised. Our own volumes contain some of the most interesting papers of Sir William Thomson on this subject, and the close agreement in the results attained by very different methods is sufficient proof that, if only approximations, they are approximations we may trust. The brilliant vortex atom theory of Sir William Thomson has not as yet achieved the position of a proved hypothesis, but has stimulated mathematical inquiry. A number of very powerful researches have added to our knowledge of a most difficult branch of mathematics, which may yet furnish the basis of a theory which shall deduce the nature of matter and the phenomena of radiation from a single group of assumptions.

The theory of gases has been extended in both direc-

tions. The able attempt of Van der Waals to bring both vapour and liquid within the grasp of a single theory is complementary to the extension by Crookes, Hittorf, and Osborne Reynolds of our knowledge of phenomena which are best studied in gases of great tenuity.

The gradual expansion of thermodynamics, and in general of the domain of dynamics from molar to molecular phenomena, has been carried on by Willard Gibbs, J. J. Thomson, and others, until, in many cases, theory seems to have outrun not only our present experimental powers, but almost any conceivable extension which they may hereafter undergo.

The pregnant suggestion of Maxwell that light is an electro-magnetic phenomenon has borne good fruit. Gradually the theory is taking form and shape, and the epoch-making experiments of Hertz, together with the recent work of Lodge, J. J. Thomson, and Glazebrook, furnish a complete proof of its fundamental hypotheses. The great development of the technical applications of electricity has stimulated the public interest in this science, and has necessitated a more detailed study of magnetism and of the laws of periodic currents. The telephone and the microphone have eclipsed the wonders of the telegraph, and furnish new means of wresting fresh secrets from Nature.

Science has become more than ever cosmopolitan, owing chiefly to the imperative necessity for an early agreement as to the values of various units for a common nomenclature, and for simultaneous observations in widely separated localities. International Conferences are the order of the day, and the new units which they have defined are based upon experiments by many first-rate observers in many lands, amongst whom the name o Lord Rayleigh stands second to none.

On the side of chemistry the periodic law of Mendeleeff has become established as a generalization of the first importance, and the extraordinary feat of foretelling the physical properties of an as yet undiscovered element has attracted to it the attention of the whole scientific world.

The once permanent gases are permanent no more. Dulong and Petit's law has found a complement in the methods of Raoult. The old doctrine of valency is giving way to more elastic hypotheses. The extraordinary progress of organic chemistry, which originated in the work and influence of Liebig and the Giessen school, has continued at an accelerated rate. The practical value of even the most recondite investigations of pure science has again been exemplified by the enormous development of the coal-tar industry, and by the numerous syntheses of organic products which have added to the material resources of the community.

The increase of our knowledge of the sun by means of localized spectroscopic observation; the application of photography to astronomy, and more recently still the extension and generalization of the nebular hypothesis are perhaps the most remarkable developments of those branches of science which relate to astronomy. Stars which no human eye will ever see are now known to us as surely as those which are clearly visible. The efforts to reduce nebulæ, comets, and stars under one common law, as various cases of the collision or aggregation of meteoritic swarms, and the striking investigations of Prof. Darwin on the effects of tidal action, and on the application of the laws of gases to a meteoritic plenum, give promise of a fuller knowledge of the birth and death of worlds.

In the biological sciences, the progress during the last twenty years has consisted chiefly in the firm establishment of the Darwinian doctrine, and the application of it and its subordinate conceptions in a variety of fields of investigation. The progress of experimental physiology has been marked by increasing exactitude in the application of physical methods to the study of the properties of living bodies, but it has not as yet benefited, as have other branches of biology, from the fecundating influence of Darwin's writings: hence there is no very prominent physiological discovery to be recorded. The generation of scientific men which is now coming to middle age has been brought up in familiarity with Mr. Darwin's teaching, and is not affected by anything like hostility or *a priori* antagonism to such views. The result is seen in the vast number of embryological researches (stimulated by the theory that the development of the individual is an epitome of the development of the race) which these twenty years have produced, and in the daily increasing attention to that study of the organism as a living thing definitely related to its conditions which Darwin himself set on foot. The marine laboratories of Naples, Newport, Beaufort, and Plymouth, have come into existence (as in earlier years their forerunners on the coast of France), and served to organize and facilitate the study of living plants and animals. The *Challenger* and other deep-sea exploring expeditions have sailed forth and returned with their booty, which has been described with a detail and precision unknown in former times. The precise methods of microscopic study by means of section-cutting—due originally to Stricker, of Vienna—have within these twenty years made the study of cell-structure and cell-activity as essential a part of morphology as it had already become of physiology. These, and the frank adoption of the theory of descent, have swept away old ideas of classification and affinities, and have relegated the Ascidian "polyps" of old days to the group of Vertebrata, and the Sponges to the Cœlenterates. The nucleus of the protoplasmic cell—which twenty years ago had fallen from the high position of importance accorded to it by Schwann—has, through the researches of Bütschli, Flemming, and Van Beneden, been reinstated, and is now shown to be the seat of all-important activities in connection with cell-division and the fertilization of the egg. The discovery of

the phenomena of karyokinesis and their relation to fertilization will be reckoned hereafter as one of the most, if not the most, important of the biological discoveries of the past twenty years.

Apart from Darwinism, the most remarkable development of biological studies during these "twice ten tedious years" is undoubtedly the sudden rise and gigantic progress of our knowledge of the Bacteria. Though the foundations were laid fifty years ago by Schwann and Henle, and great advances were made by Pasteur and by Lister just before our period, yet it is within this span that the microscope and precise methods of culture have been applied to the study of the "vibrions," or "microbes," and the so-called "bacteriology" established. We now know, through the labours of Toussaint, Chauveau, Pasteur, and Koch, of a number of diseases which are definitely caused by Bacteria. We also have learnt from Pasteur how to control the attack of some of these dangerous parasites. Within these twenty years the antiseptic surgery founded by Sir Joseph Lister has received its full measure of trial and confirmation, whilst his opportunities and those of his fellow-countrymen for making further discovery of a like kind have been ignorantly destroyed by an Act of Parliament.

To particularize some of the more striking zoological discoveries which come within our twenty years, we may cite—the Dipnoous fish-like creature *Ceratodus* of the Queensland rivers, discovered by Krefft; the jumping wheel-animalcule *Pedalion*, of Hudson; the development and the anatomy of the archaic Arthropod Peripatus worked out by Moseley, Balfour, and Sedgwick; the Hydrocorallinæ of Moseley, an entirely new group of compound animals; the fresh-water jelly-fish *Limnocodium* of the Regent's Park lily-tank; the Silurian scorpion of Gotland and Lanarkshire; the protozoon Chlamydomyxa discovered by Archer in the Irish bogs; the Odontornithes and the Dinocerata of the American palæontologists; the intracellular digestion obtaining in animals higher than Protozoa, and the significance of the "diapedesis" of blood-corpuscles in inflammation, and the general theory of phagocytes due to Mecznikow; the establishment of the principle of degeneration as of equal generality with that of progressive development, by Anton Dohrn; the demonstration by Weismann and others that we have no right to mix our Darwinism with Larmarckism, since no one has been able to bring forward a single case of the transmission of acquired characters. Perhaps the attempt to purify the Darwinian doctrine from Lamarckian assumption will hereafter be regarded—whether it be successful or not—as the most characteristic feature of biological movement at the end of our double decade Its earlier portion was distinguished by the publication of some of Darwin's later works. Its greatest event was his death.

In botany, twenty years ago, the teaching in our Universities was practically sterile. In one of our earliest numbers, Prof. James Stewart defended with some vigour the propriety of intrusting botany to a lecturer at Cambridge who was also charged with the duty of lecturing on electricity and magnetism. It is startling to compare a past, in which botany was regarded as a subject which might be tacked on anywhere, with its present condition, in which there is scarcely a seat of learning in the three kingdoms which is not turning out serious work. The younger English school would be ungrateful if it did not acknowledge its debt to the eminent German teachers from whom it has derived so much in the tradition and method of investigation. Sachs and De Bary have left an indelible mark on our younger Professors. But it would be a mistake to suppose that English modern botany has simply derived from Germany. It has developed a character of its own, in which the indirect influence of Darwin's later work can be not indistinctly traced. There has been a gradual revolt in England, the ultimate consequences of which have still to be developed, against the too physical conception of the phenomena of plant life which has been prevalent on the Continent. Darwin, by his researches on insectivorous plants and plant movements from a purely biological point of view, prepared the way for this; Gardiner followed with a masterly demonstration of the physical continuity of protoplasm in plant tissues. This has thrown a new light on the phenomena studied by Darwin, and we need not, therefore, be surprised that his son, F. Darwin, has started what is virtually a new conception of the process of growth, by showing that its controlling element is to be sought in the living protoplasm of the cell, rather than in the investing cell-wall. On the whole, English botanists have shown a marked disposition to see in the study of protoplasm the real key to the interpretation of the phenomena of plant life. The complete analogy between the processes of secretion in animals and vegetables, established by Gardiner, and the essential part played by ferments in vegetable nutrition, illustrated by Green, are examples of the results of this line of inquiry. To Germany we owe a flood of information as to the function of the cell-nucleus, which it is singular has met with general acceptance but little detailed corroboration in this country.

In morphology a review would be ineffective which did not go somewhat deeply into detail. The splendid hypothesis of Schwendener, of the composite nature of lichens as a commensal union of Algæ and Fungi, has gradually won its way into acceptance. In England there is little of the first rank which calls for note except the researches of Bower on the production of sexual organs on the leafy plant in ferns without the intervention of an intermediate generation.

In vegetable physiology there seems a pause; the

purely physical line of inquiry, as already suggested, seems to have yielded its utmost. The more biological line of inquiry has only yet begun to yield a foretaste of the results which will undoubtedly ultimately flow from it.

Something must be added as to systematic and geographical botany. The "Genera Plantarum" of Bentham and Hooker, the work of a quarter of a century at Kew, affords a complete review of the higher vegetation of the world, and has been accepted generally as a standard authority. To Bentham also we owe the completion of the "Flora Australiensis," the first complete account of the flora of any great continent.

In geographical botany, perhaps the most interesting results have been the gradual elaboration of a theory as to the distribution of plants in Africa, and the botanical exploration of China, of the vegetable productions of which, twenty years ago, almost nothing was known.

In the classification of the lower plants, perhaps the most interesting result has been the happy observations of Lankester upon a coloured Bacterium, which enabled him to show that many forms previously believed to be distinct might be phases of the same life-history.

In geology probably the greatest advance has been in the application of the microscope to the investigation of rock structure, which has given rise to a really rational petrology. All except the coarser-grained rocks were only capable of being described in vague terms; with modern methods their crystalline constituents are determinable, however minute, and the conditions under which they were formed can be inferred.

It is impossible, even in a brief review of this kind, to think only of what has been won, and to ignore the loss of leaders who were once foremost in the fray. In England three names which will never be forgotten have been removed from the muster-roll. Darwin, Joule, and Maxwell can hardly be at once replaced by successors of equal eminence. As the need arises, however, men will no doubt be found adequate to the emergency, and it is at least satisfactory to know that they will appeal to a public more capable than heretofore of appreciating their efforts.

The support afforded by the Governments of Western Europe to scientific investigation has been markedly increased within the period which we survey. France has largely extended her subsidies to scientific research, whilst Germany has made use of a large part of her increased Imperial revenue to improve the arrangements for similar objects existing in her Universities. The British Government has shown a decided inclination in the same direction: the grant to the Royal Society for the promotion of scientific research has been increased from £1000 to £4000 a year; whilst subsidies have been voted to the Marine Laboratory at Plymouth, to the Committee on Solar Physics, to the Meteorological Council, and quite recently to the University Colleges throughout the country, of which last it is to be hoped that a fair proportion will be devoted to the promotion of research rather than to the reduction of class fees.

Twenty years ago England was in the birth-throes of a national system of primary instruction. This year has seen the State recognition of the necessity of a secondary and essentially a scientific system of education, and the Technical Instruction Act marks an era in the scientific annals of the nation.

The extension of scientific teaching has gone on rapidly within and without our Universities. Twenty years ago the Clarendon Laboratory at Oxford was approaching completion, and was the only laboratory in the country which was specially designed for physical work. Now, not only has Cambridge also its Cavendish Laboratory, but both Universities have rebuilt their chemical laboratories, both have erected buildings devoted to the study of biology, and the instruction of students in both zoology and botany has taken a characteristic practical form which we owe to the system of concentrating attention on a series of selected "types" introduced by Rolleston and by Huxley. Oxford has been furnished with an astronomical observatory by the liberality of Warren De la Rue, and Cambridge has accepted the noble gift of the Newall telescope. Nor have such proofs of the vitality of science been confined to the Universities.

Twenty years ago the Owens College was a unique institution: now, united with two thriving Colleges in Leeds and Liverpool, it forms the Victoria University; while science is studied in appropriate buildings in Birmingham, Newcastle, Nottingham, and half a dozen towns beside.

A race is thus springing up which has sufficient knowledge of science to enforce due recognition of its importance, and public opinion can now, far more than in the past, be relied on to support its demands. Fortunately, too, these can be authoritatively expressed. The Royal Society wields, if it chooses to exercise it, an enormous power for good. Admitted on all hands to be the supreme scientific authority in this country, its decisions are accepted with a deference which can spring only from respect for the knowledge and scrupulous fairness by which they are dictated. If sometimes it moves slowly, *pur se muove*, and it is delightful to turn from the babble of the politicians to the study of an institution which does its work well, and perhaps too noiselessly. But even the House of Commons, hitherto ignorant and therefore apathetic in matters scientific, is awakening to the fact that there are forces to be reckoned with and impulses to be stimulated and controlled which are of more enduring import to the national welfare than mere party politics. And the people, too, are beginning to see that it is to the economic working of these forces, and to the right direction of these impulses, that their representatives are bound to give attention. True it is that

another generation may possibly pass away before either the House of Commons or even Ministers are sufficiently instructed in science to recognize fully their responsibility in this direction.

Whatever, then, the future may bring, the last twenty years have been characterized by progress both steady and rapid. The tide flows on with no sign of check, and we accept the success of NATURE in no spirit of self-gratulation, but as a straw by which the speed of the current may be gauged.

MODERN VIEWS OF ELECTRICITY.

Modern Views of Electricity. By Oliver J. Lodge, D.Sc., LL.D, F.R.S. (London: Macmillan and Co., 1889.)

IN this interesting book Prof. Lodge gives a very lively and graphic account of many of the most recent speculations about the nature of electrical phenomena. A work with this object was urgently needed, as the method of regarding these phenomena given in popular treatises on electricity is totally different from that used by those engaged in developing the subject.

The attention called by Faraday and Maxwell to the effects produced by and in the medium separating electrified bodies has had the effect of diverting attention from the condition of the charged bodies in the electric field to that of the medium separating them, and it is perhaps open to question whether this of late years has not been too much the case. To explain the effects observed in the electric field we should require to know the condition not only of the ether, but also of the conductors and insulators present in it; just as a complete theory of light would include the state of the luminous bodies as well as of the ether transmitting the radiations excited by them. Since matter is more amenable to experiment than the ether, it seems most probable that we shall first gain an insight into the nature of electricity from a study of those cases where matter seems to play the chief part—such as in the electric discharge through gases, and the phenomena of electrolysis—rather than from speculations, however interesting, as to what takes place in the ether when it is transmitting electrical vibrations. Prof. Lodge, however, in the work under consideration, devotes most of his space to the consideration of the ether. In his preface he says, "Few things in physical science appear to me more certain than that what has so long been called electricity is a form, or rather a mode, of manifestation of the ether;" and he proceeds to give precision to this somewhat vague statement by developing a theory that electricity is a fluid, and a constituent of a very complex ether. In the first few chapters he supposes that all insulators, including the ether, have a cellular structure the cells being filled with a fluid which is electricity, and which is not able to get from one cell to another unless the walls of the cells are broken down; in conductors, however, there are channels between the cells, so that the electricity is able to flow more or less freely through them. A flow of this fluid is an electric current. But if this is the case, anything which sets the ether in motion will produce an electric current. Now, Fizeau's experiments show that moving bodies carry the ether with them to an extent depending on their index of refraction; so that a disk made of glass or other refracting substance, if set in rapid rotation about an axis through its centre, and at right angles to its plane, ought to act as if currents were circulating in the disk, and produce a magnetic field around it. In order to avoid the allied difficulty that nothing has ever been observed which indicates that a magnet or a current flowing through a coil possesses gyroscopic properties, Prof. Lodge assumes, in subsequent chapters, that the fluid in the cells of the ether is a mixture of two fluids, and that these two fluids are positive and negative electricity: and that, in order to exhibit any electrical effect, the compound fluid has first to be decomposed into positive and negative electricity by the application of an electromotive force. A current of electricity, on this view, consists of the flow of equal quantities of positive and negative electricity in opposite directions. Thus this, the most "modern view of electricity," is in its most important features almost identical with the old two-fluid theory published by Symmer in 1759. We confess we do not think the theory in its present form advances the science of electricity much: it does not suggest new phenomena, nor does it lend itself readily to explain the action of matter in modifying electrical phenomena; it demands, too, a very artificial ether. It would seem that the first steps required to make a theory of this kind a real advance on the old two-fluid theory would be the discovery of a structure for the ether, which would possess the same kind of properties as the mixture of the two electricities on that theory. A great deal, too, is left indefinite in the theory: thus, for example, we are not told whether for a given current these streams are moving slowly or with prodigious velocities. In fact, there is throughout the book rather a want of definite conclusions, and this is rather hidden by the vigorous style in which Prof. Lodge writes: he develops his ideas in such an enthusiastic and interesting way that on the first reading they seem to be a good deal more definite than they prove to be on calmer reflection.

But whatever may be thought of Prof. Lodge's theory of electricity, there can be, we think, no two opinions of the value of the numerous models illustrating the properties of electrical systems which he has invented. These must prove of the greatest assistance in enabling the student to gain a clear and vivid idea of electrical processes, and ought to be largely employed by all teachers of electricity.

In a work dealing so briefly with such a multitude of different and difficult subjects it is natural that there should be many statements to which exception might be taken. Prof. Lodge disarms criticism by his frank admission of this; sometimes, also, by an amusing vagueness of statement: thus, on p. 206, in speaking of the condition of the ether inside a strongly-magnetizable substance, he says: "Perhaps it is that the atoms themselves revolve with the electricity; perhaps it is something quite different." There are, however, some statements of a less theoretical kind which seem to us likely to mislead the student. Thus it is stated that the amount of the Peltier effect shows that the difference of potential between zinc and copper is only a few micro-volts. The Peltier effect, however, without further assumption, cannot tell us anything about the absolute magnitude of the difference of

potential between the metals; it can only give us the value of the temperature coefficient, which is equal to the Peltier effect divided by the absolute temperature. Then, again, the pyro-electricity of tourmaline is explained by the unilateral conductivity of a tourmaline crystal whose temperature is changing, discovered by the author and Prof. Silvanus Thompson. If this unilateral conductivity is regarded as proving the existence of an electromotive force in a crystal which is increasing or decreasing in temperature, the explanation is valid, but in the text nothing is said about an electromotive force, and the student might be led to infer that a mere difference in resistance could explain pyro-electricity. The way in which a current flows past an insulating obstacle, the lines of flow closing in on the obstacle, and leaving nothing corresponding to "dead water" behind it, is given as a proof that the electric current has no mechanical momentum; but unless the corners of the obstacle were infinitely sharp, a slowly-moving fluid might flow in the same way as electricity, even though it possessed inertia, so that the proof is not conclusive. It is also stated that the effects on light produced by a magnetized body, discovered by Dr. Kerr, of Glasgow, have been deduced by Prof. Fitzgerald from Maxwell's theory of light. As a matter of fact, however, the results deduced from this theory by Fitzgerald do not coincide with those observed by Dr. Kerr and Prof. Kundt. The production in an unequally-heated conductor of an electromotive force is explained by supposing the atoms in such a body to be moving faster in one direction than the opposite, and therefore, since they are supposed to drag the ether with them, producing a flow of ether in the direction in which they are moving fastest; but, on the dualistic theory of electricity adopted in this book, this ether stream would consist of equal quantities of positive and negative electricity moving in the *same* direction, and this would not produce any electrical effect.

At the end of the book are three popular lectures delivered by Prof. Lodge, the first on the relation between electricity and light, the second on the ether and its functions, and the third his admirable one at the Royal Institution, on the discharge of a Leyden jar, which is a model of what such a lecture ought to be.

Taken as a whole, we think that the book is one which ought to be read by all advanced students of electricity; they will get from it many of the views which are guiding those who are endeavouring to advance that science, and it is so stimulating that no one can read it without being inspired with a desire to work at the subject to which it is devoted.

THE CALCULUS OF PROBABILITIES.

Calcul des Probabilités. Par J. Bertrand. (Paris: Gauthier-Villars, 1889.)

"EVERYBODY makes errors in Probabilities at times, and big ones," writes De Morgan to Sir William Hamilton. M. Bertrand appears to form an exception to this dictum, or at least to its severer clause. He avoids those slips in the philosophical part of the subject into which the greatest of his mathematical predecessors have fallen. Thus he points out that, in investigating the "causes" of an observed event, or the ways in which it might have happened, by means of the calculus of probabilities, it is usual to make certain unwarranted assumptions concerning the so-called *a priori* probability of those causes. Suppose that a number of black and white balls have been drawn at random from an urn, and from this datum let us seek to determine the proportion of black and white balls in the urn. It is usual to assume, without sufficient grounds, that *a priori* one proportion of balls, one constitution of the urn, is as likely as another. Or suppose a coin has been tossed up a number of times, and from the observed proportion of heads and tails let it be required to determine whether and in what degree the coin is loaded. Some assumption must be made as to the probability which, prior to, or abstracting from, our observations, attaches to different degrees of loading. The assumptions which are usually made have a fallacious character of precision.

Again, M. Bertrand points out that the analogy of urns and dice has been employed somewhat recklessly by Laplace and Poisson. It is true that the ratio of male to female births has a constancy such as the statistics of games of chance present. But, before we compare boys and girls to black and white balls taken out at random from an urn, we must attend not only to the average proportion of male to female births, but also to the deviations from that average which from time to time or from place to place may be observed. The analogy of urns and balls is more decidedly inappropriate when it is applied to determine the probable correctness of judicial decisions. The independence of the judges or jurymen which the theory supposes does not exist.

> "Quand un juge se trompe il y a pour cela des raisons: il n'a pas réellement mis la main dans une urne où le hazard l'a mal servi. Il a ajouté foi à une faux témoignage, le concours fortuit de plusieurs circonstances a éveillé à tort sa défiance, un avocat trop habile l'a ému, de hautes influences peutêtre l'ont ébranlé. Ses collègues ont entendu les mêmes témoins, on les a instruits des mêmes circonstances, le même avocat a plaidé devant eux, on a tenté sur eux la même pression."

With equal force does M. Bertrand expose the futility of the received reasoning by which it is pretended to determine the probability that the sun will rise to-morrow from the fact that it has risen so many days in the past.

These reflections are just and important; but their value is somewhat diminished by the fact that they have been, for the most part, made by previous writers with whom our author seems unacquainted. Thus Prof. Lexis has more carefully considered the extent of the error committed by Laplace and Poisson in applying to male and female births and other statistics rules derived from games of chance. The fundamental principles of Probabilities have been more fully explored by Dr. Venn. M. Bertrand, like Laplace, starts by defining the probability of an event as the ratio of the number of favourable cases to the number of possible cases. He does not explain what constitutes a "favourable case"—that, when a die is thrown, the probability of obtaining the 3 or 4 is one-sixth, because as a matter of fact each side in the long run turns up once out of six times. Accordingly, when he argues that in a great number of trials each event is most likely to occur with a frequency correspond-

ing to its probability, he lays himself open to the charge of circularity which Dr. Venn has brought against Bernouilli's theorem. Without pronouncing on this delicate question, we may safely say, with respect to the first principles of the subject, that no point which has been left obscure by Dr. Venn has been cleared up by M. Bertrand.

It is with respect to the purely mathematical portion of the calculus, or that part of its metaphysics which is inextricably mixed with mathematics, that we expected and have found most assistance from M. Bertrand. Hitherto the study of Probabilities has been barred by the dilemma which M. Bertrand thus states:—

"On ne peut bien connaître le calcul des probabilités sans avoir lu le livre de Laplace; on ne peut lire le livre de Laplace sans s'y préparer par les études mathématiques les plus profondes."

Much of Laplace's analysis which must have affected many eager students like stickjaw has been simplified by M. Bertrand. He is in general more readable than Poisson. Several of the theorems which he gives seem to be new. His methods of determining from a given set of observations the characteristic, or *modulus*, appertaining to the source of error are specially interesting.

M. Bertrand's mathematical power enables him to carry the torch of common-sense to those perplexed parts of the subject where less qualified critics, awed by the imposing mass of symbols, have hesitated to differ from Laplace or Poisson. Of this kind is the simultaneous determination of several quantities from a great number of equations. When Laplace computes that the odds are a million to one against the occurrence of an error of assigned magnitude in the determination of Jupiter's mass, M. Bertrand shows reasons for suspecting the accuracy of such computations. In fact, he carries out Poinsot's witty direction:

"Après avoir calculé la probabilité d'une erreur il faudrait calculer la probabilité d'une erreur dans le calcul."

The true import and proper application of the theory of errors of observation are thus well expressed:—

"On peut accepter sans crainte le résultat, mais il est téméraire d'évaluer en chiffres la confiance qu'il doit inspirer."

M. Bertrand teaches with authority—and not like those who have not followed the higher mathematical reasonings of the calculus—in what spirit its conclusions should be accepted.

Still, even with regard to those parts of the subject where a first-rate mathematician has so great an advantage, we venture to think that the work would have been much more valuable if the writer had taken the trouble to acquaint himself more fully with what his predecessors had done. For example, in discussing the reasons for taking the arithmetic mean of a set of observations (presumed to be equally good) relating to a single quantity, M. Bertrand does not dwell on the argument that the probability-curve —with which the arithmetic mean is specially correlated—is apt to represent the grouping of errors for this reason, that an error may be regarded as a function of a great number of elements each obeying some definite law of facility, and that the values of such a function conform to the probability-curve. It is true that Laplace, from whom this argument may be derived, has not himself used it very directly. But in a writer on the method of least squares we may expect some conversance with more recent works, in particular with Mr. Glaisher's classical paper in the Memoirs of the Astronomical Society (London). Moreover, Laplace does employ the mathematical theorem which we have indicated, not indeed to prove that the law of facility for errors of observation in general is the probability-curve, but that, whatever that law of facility be, the most advantageous combination is a certain linear function. A treatise in which this celebrated argument is not discussed cannot be regarded as exhaustive. But it is remarkable that with respect to the combination of observations, M. Bertrand seems to defer more to Gauss than to his own eminent countryman.

M. Bertrand has indeed slipped in a doctrine for which the authority of Laplace may be quoted, that in choosing the best combination of a set of observations "there is an essential difference between the most probable value of a quantity and the value which it is best to adopt" (Bertrand, Art. 138); the latter being the mean (first power) of the observations (Art. 155)—which M. Bertrand rather awkwardly terms "la valeur probable." M. Bertrand does not seem to realize the gravity of the assumption which is contained in the latter clause. Later on he employs Gauss's criterion of erroneousness—namely, the mean square of error. But the ground, nature, and relation of these two principles are not very clearly explained by the writer. With respect to the philosophical foundation of the method of least squares he has not superseded the necessity of studying Laplace.

With these reservations, M. Bertrand's work may be regarded as one of the most complete treatises on the subject. Nowhere else are the two elements so peculiarly combined in the science of Probabilities—common-sense and mathematical reasoning—to be found existing together in such abundance.

F. Y. E.

ARGENTINE ORNITHOLOGY.

Argentine Ornithology. By P. L. Sclater, Ph.D., F.R.S., and W. H. Hudson, C.M.Z.S. Vol. II. (London: W. H. Porter, 1889.)

THE completion of this important work is an event of considerable importance to every lover of neotropical zoology, and the authors have both performed their parts well, while the ten plates by Mr. Keulemans are beautifully drawn and admirably coloured. Among the increasing number of Englishmen who settle in the Argentine Republic, there are sure to be many who will pursue natural history studies, and to all such a well-executed book like the present will be invaluable. The joint authors of the work are happy in their association, for while Dr. Sclater brings to the work a vast experience, and a sound scientific knowledge of his subject, it is certain that never was there a better describer of the habits of birds than Mr. Hudson. Although of English parentage, he is a native-born Argentine, and he has grown up among the birds whose life and history he so well knows how to portray. In turning over the pages of this volume, we have found many interesting extracts which we should have liked to present to our readers,

and we feel that we should not be doing justice to Mr. Hudson if we did not quote for their benefit one specimen of this naturalist's writing. He is describing the habits of the Carancho (*Polyborus tharus*):—

"When several of these birds combine they are very bold. A friend told me that while voyaging on the Paraná River a black-necked Swan flew past him hotly pursued by three Caranchos; and I also witnessed an attack by four birds on a widely different species. I was standing on the bank of a stream on the Pampas watching a great concourse of birds of several kinds on the opposite shore, where the carcass of a horse, from which the hide had been stripped, lay at the edge of the water. One or two hundred Hooded Gulls and about a dozen Chimangos were gathered about the carcass, and close to them a very large flock of Glossy Ibises were wading about in the water, while amongst these, standing motionless in the water, was one solitary white Egret. Presently four Caranchos appeared, two adults and two young birds in brown plumage, and alighted on the ground near the carcass. The young birds advanced at once and began tearing at the flesh; while the two old birds stayed where they had alighted, as if disinclined to feed on half-putrid meat. Presently one of them sprang into the air and made a dash at the birds in the water, and instantly all the birds in the place rose into the air screaming loudly, the two young brown Caranchos only remaining on the ground. For a few moments I was in ignorance of the meaning of all this turmoil, when, suddenly, out of the confused black and white cloud of birds the Egret appeared, mounting vertically upwards with vigorous measured strokes. A moment later, and first one, then the other, Carancho also emerged from the cloud, evidently pursuing the Egret, and only then the two brown birds sprang into the air and joined in the chase. For some minutes I watched the four birds toiling upwards with a wild zigzag flight, while the Egret, still rising vertically, seemed to leave them hopelessly far behind. But before long they reached and passed it, and each bird as he did so would turn and rush downwards, striking at the Egret with his claws, and while one descended the others were rising, bird following bird with the greatest regularity. In this way they continued toiling upwards until the egret appeared a mere white speck in the sky, about which the four hateful black spots were still revolving. I had watched them from the first with the greatest excitement, and now began to fear that they would pass from sight and leave me in ignorance of the result; but at length they began to descend, and then it looked as if the Egret had lost all hope, for it was dropping very rapidly, while the four birds were all close to it striking at it every three or four seconds. The descent for the last half of the distance was exceedingly rapid, and the birds would have come down almost at the very spot they started from, which was about forty yards from where I stood, but the Egret was driven aside, and sloping rapidly down struck the earth at a distance of two hundred and fifty yards from the starting point. Scarcely had it touched the ground before the hungry quartette were tearing it with their beaks. They were all equally hungry no doubt, and perhaps the old birds were even hungrier than their young; and I am quite sure that if the flesh of the dead horse had not been so far advanced towards putrefaction they would not have attempted the conquest of the Egret. I have so frequently seen a pure white bird singled out for attack in this way, that it has always been a great subject of wonder to me how the two common species of snow-white Herons in South America are able to maintain their existence; for their whiteness exceeds that of other white waterfowl, while, compared with Swans, Storks, and the Wood-ibis, they are small and feeble. I am sure that if these four Caranchos had attacked a Glossy Ibis they would have found it an easier conquest; yet they singled out the egret, purely, I believe, on account of its shining white conspicuous plumage."

In his introduction Dr. Sclater gives a *résumé* of the number of genera and species inhabiting the Argentine Republic, and shows that the avifauna of that portion of South America belongs to the Patagonian sub-region. A little sketch-map would have been useful, to show the configuration of the country and the proportions of the mountain-ranges, as it is evident that a district which can boast of a Dipper, and be at the same time the home of two Cariamas, must possess elements of two very different avifaunæ. Some day, no doubt, an exact exploration, such as that now being undertaken in Mexico by Messrs. Salvin and Godman, will trace the limits of the avifaunæ of the Pampas and the mountain regions. If Mr. Hudson could only be induced to resume his work of exploration and visit the interior of the Argentine Republic, the results would be, we venture to say, of the first importance to science.

Dr. Sclater, we notice, draws his comparisons of the different orders of Argentine birds from the "Nomenclator Avium Neotropicalium" of 1873, which is rather ancient history. The statistics of American birds must have altered considerably since that date, if we may judge from the Tanagers alone, which numbered 302 species in 1873, and in 1886 had reached 377 in number, according to Dr. Sclater's own estimate. In dividing the Neotropical Region into the sub-regions he adopts the conclusions of Prof. Newton in the "Encyclopædia Britannica," but the names of one or two of them are changed. The boundaries seem to be extremely natural, according to our present state of knowledge, though we would scarcely consider the Central American sub-region (or the Transpanamic sub-region, as Dr. Sclater renames it) to be bounded on the north by Tehuantepec! The author probably intended to give only a general outline, for the northern boundaries of the Central American sub-region are much more elaborately defined in fact.

R. BOWDLER SHARPE.

OUR BOOK SHELF.

The Chemistry of the Coal-tar Colours. From the German of Dr. R. Benedikt. Translated, with Additions, by Dr. E. Knecht. Second Edition. (London: George Bell and Sons, 1889.)

DR. BENEDIKT'S little book is a standard treatise in Germany, where the literature of the coal-tar colours is fast becoming a most important branch of the general literature of applied chemistry; and Dr. Knecht has done excellent service in making the work more generally known to English readers by means of his translation. It is remarkable that, although England may be said to have originated the coal-tar colour industry, she has contributed comparatively little to the general literature of the subject. Practically, all the systematized information we possess has come to us through the medium of French and German manuals. A number of our chemists could be named who have communicated original memoirs on the constitution of organic colouring-matters to the recognized organs of chemical research, but their work is very special in its character, and appeals rather to the pure chemist than to the technologist, and hence is seldom read by the latter. The want of a good, sound, and comprehensive treatise on the subject

of the coal-tar colour industry has, we think, not been without its influence on the development of this branch of applied organic chemistry in this country. Dr. Knecht's translation merits a place on the bookshelf of every person engaged in the manufacture and use of the so-called coal-tar colours.

A Bibliography of Geodesy. By J. Howard Gore, B.S., Ph.D. (Washington: Government Printing Office, 1889.)

THIS valuable work forms Appendix No. 16 to the 1887 Report of the United States Coast and Geodetic Survey, and is another example of the disinterested energy displayed by our Transatlantic cousins in scientific matters. With great perseverance, and at the cost of much time and trouble, Mr. Gore personally explored thirty-four of the principal libraries of America and Europe, and numerous minor libraries by proxy; and, in addition, he checked and completed many of his references by correspondence with the living authors of both continents. The extent of his labours is shown by the four hundred columns of references, and short remarks where the title alone is not sufficiently explanatory. An alphabetical arrangement is adopted, and this includes authors, abbreviations, and subjects.

It is gratifying to note that our own country, besides the assistance rendered by its libraries, lends its aid to such an important work in the shape of a manuscript supplement by Colonel Herschel to his pendulum bibliography, which was placed unreservedly at Mr. Gore's disposal, through the courtesy of the Royal Society. After the offers of publication made by various institutions, including the International Geodetic Association at Berlin, no further testimony to Mr. Gore's fitness for the work is needed, and the compiler is justly proud "to see the results of his labours issuing from an institution of his own country, which throughout the world is the recognized advance guard in geodetic science."

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Method of Quarter Squares.

I OMITTED any reference to Leslie in my review of Mr. Blater's table (NATURE, vol. xl. p. 573), as I have never supposed that he was an independent discoverer of the method, or an independent calculator of a table, of quarter squares. I have eferred to his table in my Report on Mathematical Tables Brit. Assoc. Report, 1873, p. 23); and the passage quoted by Prof. Carey Foster (p. 593) is given in full in the preface to Mr. Blater's table. It seems to me that the words in question—"This application of a table of quarter squares, as it is derived from the simplest principles, might have readily occurred to a mathematician; yet I have nowhere seen it brought into practical use till, last summer, I met with, at Paris, a small book by Antoine Voisin, printed in 1817"—do not indicate an independent discovery; and this view is confirmed by the fact that, in the first edition of the "Philosophy of Arithmetic" (1817), Leslie makes no mention of quarter squares. It was only in the second edition (1820), after having seen Voisin's work in the previous year, that he added, at the end of the volume, an account of the method, and a table extending to 2000. The table was copied, I presume, from Voisin, as Leslie does not claim it as the result of his own calculation. In the British Association Report I have described it as "reprinted from Voisin," and have pointed out that it did not appear in the first edition. In the preface to Mr. Blater's letter it is described as "an extract from Voisin's table." Although we may, I think, infer, almost with certainty, that the table is only a reprint,[1] it is to be regretted that Leslie did not say so explicitly.

J. W. L. GLAISHER.

Trinity College, Cambridge, October 26.

Darwinism.

MR. ROMANES states that it is "absurd" to call his essay on physiological selection an elaborate (I said "laborious") attack upon Mr. Darwin's theory of the origin of species. In that essay I find these words (p. 345), "the theory of natural selection has been misnamed: it is not strictly speaking a theory of the origin of species"; and on p. 403, "the theory of physiological selection [*i.e.* Dr. Romanes's theory] has this advantage over every other theory that has ever been propounded on the origin of species"; and again, "the problem of the origin of species which, as shown in the preceding paper [viz. the laborious essay], his [Mr. Darwin's] theory of natural selection serves only in small part to explain."

On the other hand, Mr. Darwin entitled his great work, "The Origin of Species by means of Natural Selection, or the preservation of favoured races in the struggle for life." He considered his theory of natural selection to be a theory of the origin of species. Mr. Romanes says it is not. I say that this is an attack on Mr. Darwin's theory, and about as simple and direct an attack as possible. Why Mr. Romanes wishes us to believe that he did not attack Mr. Darwin's theory it is difficult to conceive. That he should hope to persuade anyone that it is absurd to call his essay an attack on Mr. Darwin's theory when this is what it distinctly professes to be is curious. I trust you will not permit an empty discussion on this matter, but leave it to your readers to find out by reference to the Proc. Linn. Soc., vol. xix., where the absurdity exists.

E. RAY LANKESTER.

42 Half-moon Street, November 1.

Record of British Earthquakes.

WILL you allow me to ask your readers to help me in compiling notes of the earthquakes felt in this country during the present and following years?

Mr. Mallet's great Catalogue of all recorded earthquakes ends, as is well known, with the year 1842. Previously to this, Mr. David Milne had published a series of papers on the earthquakes of Great Britain in the *Edinburgh New Philosophical Journal* (vols. xxxi. to xxxvi. for the years 1841-44). These papers, which are of very great value, bring down our record to the end of August 1843. In recent years we have had the Catalogues of Prof. J. P. O'Reilly (Trans. Roy. Irish Acad., vol. xxviii. pp. 285-316 and 489-708) and the late Mr. W. Roper (published by T. Bell, *Observer* Office, Lancaster). The latter is a useful chronological list of shocks felt during the Christian era, down to February 10, 1889; but, except in a few cases, it is little more than a list. Prof. O'Reilly's important catalogues are arranged alphabetically according to the localities affected, and do not pretend to give detailed information with reference to the shocks themselves.

To make our seismic record more complete, I propose, therefore, to compile a descriptive list of British shocks noticed in newspapers and scientific journals from the time at which Mr. Milne's Catalogue closes down to the end of the year 1888; and I should be very grateful if your readers can in any way help me in this work.

What I wish particularly to ask for, however, is information relating to the shocks of the present and future years. For our knowledge of British earthquakes we must at present rely to a great extent on newspaper accounts; and these accounts, which for some points are fairly trustworthy, become difficult of access in after years. If any of your readers are willing to assist me in preserving these notices in a convenient and systematic form, may I ask if they would be good enough to send, to the address below, the names and dates of newspapers, and more especially local ones, in which any descriptions, however short, are given of British shocks? It is hardly necessary to say that any other notes, communicated by those who have felt the shocks or observed their effects, would be of great value, and would be most thankfully received.

The days are past for compiling earthquake catalogues for the

[1] After quoting the full title of Voisin's table, Leslie refers to his own table as "the specimen which I have given."

whole surface of the earth, and the value of an attempt at such a task would now be extremely doubtful. But for limited districts, like this country, the case is very different. It would indeed be difficult to over-estimate the value of a seismic record which can claim any approach to completeness for a definite earthquake area, however feeble the shocks which visit it may be.

I may add that I hope shortly to publish some notes or directions for the study of earthquakes, with special reference to those which occur in this country. CHARLES DAVISON.

38 Charlotte Road, Birmingham, October 10.

Effects of Lightning.

I HAVE known of the following case since July last, but owing to absence from this place have only been able to get particulars during the last few days.

During the terrific storm of the 12th of July last, a labourer's cottage was struck by lightning at Leagrave, near here. The lightning descended, according to an eye-witness's report, like a "spout of fire," and struck and descended the chimney, which it destroyed. In the room below there was an old shepherd, an invalid woman, a child, and a shepherd's dog. The shepherd was sitting in a chair leaning on a stick, a kettle was boiling on the fire, and the door was open. The lightning entered the room simultaneously by the chimney and an adjoining window. The window was utterly destroyed, and the kettle was thrown from the fire across the room, the stick on which the shepherd was leaning was torn from his hand and also thrown across the room, the lightning entered a cupboard containing glass and crockery and destroyed every article, and plaster was torn from the walls. The man and woman remained unhurt, but the child was thrown down and its knees stiffened. The dog was struck perfectly stiff, "like a log of wood," and was considered dead. The room seemed full of fire, water, and sulphur, and the occupants said the smell of sulphur was so strong that they would certainly have been suffocated had it not been for the open door. After the storm had abated, the dog, with all its limbs stiff, was laid in a barn, where it very slowly and partially recovered. It long remained both deaf and blind, and was entirely dependent upon smell for its recognition of persons and things. To the present day it has not entirely recovered its injured senses.

Dunstable. W. G. S.

Electrical Cloud Phenomenon.

A SHORT description of a curious cloud appearance observed by me this summer may be of interest to your readers. It was noticed in Kiushu, the southernmost of the three great islands of Japan, early in July, at a distance of ten or twelve miles from the sea.

The season had been, and was, after the time of the observation, an exceptionally rainy one, severe floods being produced in almost all parts of the country, but it was not raining in the place where I made the observation at that particular time. Time shortly after midday, thermometer about 85° F.

The sky was clear overhead, but there was a great bank of heavy "thunderous" looking clouds to the south. It is most difficult to judge even approximately of the distance of clouds, but these might be from one to two miles off; the lower edge was represented by a very nearly straight line, and there was an amount of blue sky visible under the clouds that would perhaps subtend from 10° to 15°.

My attention was attracted to a sort of "tail" of cloud stretching itself downwards from the straight under side of the cloud-bank. It gradually extended till it reached some two-thirds of the distance from the cloud to the earth. It remained of about constant length for a little over ten minutes, the lower end continually waving about in a most curious way, giving the impression almost that it was feeling for something.

Quite suddenly the filament of cloud straightened itself out, and extended itself towards the earth. The lower end became so very thin that, from the distance, it was impossible to see whether it actually made contact with the earth or not, but I have not the smallest doubt that it did, and that a silent discharge took place at the time. There was certainly no sound heard. Immediately after the contact the filament rapidly drew itself up to the cloud, and was incorporated with it. Almost immediately after this, whether as a mere coincidence or not I cannot tell, the cloud discharged a great amount of rain. W. K. BURTON.

Imperial University, Tokio, Japan.

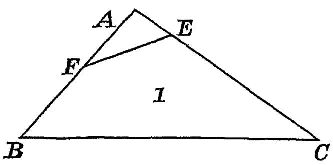
P.S.—The appearance was not unlike the illustrations of "water-spouts" that I have seen, but there was no whirling motion such as is always described as accompanying these, nor, indeed, was there any evidence of violent disturbance of any kind at all.

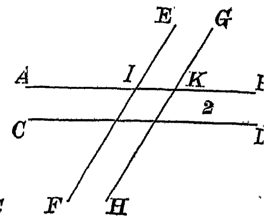
The Use of the Word Antiparallel.

THE following note on the use of the word antiparallel may prove of interest to the readers of NATURE.

In the second edition of "A New Mathematical Dictionary" by E. Stone, F.R.S. (London, 1743), appears a short article on antiparallels, the whole of which I will quote:—

"Antiparallels, are those lines, as FE, BC, that make the same angles AFE, ACB, with the two lines AB, AC, cutting them, but contrary ways, as parallel lines that cut them. But Mr. Leibnitz, in the *Acta Erudit.*, An. 1691, p. 279, calls antiparallels those lines (see Fig. 2) as EF, GH, which cut two parallels AB, CD; so that the outward angle AIF, together with the inward one AKH, is equal to a right angle. When





the sides AB, AC, of a triangle, as ABC (Fig. 1), are cut by a line EF antiparallel to the base BC, the said sides are cut reciprocally proportional by the said line EF; that is, AF : BF : : EC : AE, the triangles AFE, ABC being similar or equiangular."

The error in regard to the ratios of the segments of the sides is the same as the one noted in Hutton's "Miscellanea Mathematica," as quoted by Mr. Langley. I have no doubt that earlier instances of the use of this word can be found, and I would like to know whether the word is used in the first edition of "Stone's Dictionary." W. J. JAMES.

Wesleyan University, Middletown, Conn. U.S.A., October 15.

Fossil Rhizocarps.

IN Bennet and Murray's "Cryptogamic Botany," at p. 115, I am surprised to find in a reference to my paper on "Fossil Rhizocarps" (in *Bull. Ac. Sciences, Chicago*) the statement, with reference to the macrospores of *Protosalvinia*, that "inasmuch as they are borne on Lepidodendron scales this reference is inadmissible." Now no such fact has come to my knowledge, and on the contrary these bodies are found inclosed in cellular sporocarps like those of Salvinia, and are so described in the paper in question. If anyone has found them on "scales of Lepidodendron," the authority should have been stated.

Montreal, October 15. J. WM. DAWSON.

Specific Inductive Capacity.

ON p. 669 of Ganot's "Physics" (eleventh edition) the following statement is found:—"At a fixed distance above a gold-leaf electroscope, let an electrified sphere be placed, by which a certain divergence of the leaves is produced. If, now, the charges remaining the same, a disk of sulphur or of shellac be interposed, the divergence increases, showing that inductive action takes place through the sulphur to a greater extent than through a layer of air of the same thickness."

If this statement were correct, there should be less electric action on the side of the ball furthest from the electroscope when the dielectric is interposed. To test this I arranged an experiment as follows:—

The knob of a charged Leyden jar was placed midway between two insulated plates of metal, each plate being in connection with an electroscope. The leaves of each electroscope now diverged to an equal extent.

A plate of ebonite was now placed between the knob of the jar and one of the plates. If the statement above quoted is

correct, the leaves of the electroscope in connection with this plate should show an increased divergence, but the reverse effect was observed. *The leaves partially collapsed.* In all experiments that I have made by inserting dielectrics between a charged body and an electroscope, less electric action has been the result. If while the charged ball be near the electroscope the plate of it be touched with the finger, the leaves collapse, and on removing the finger and then the charged ball they again diverge.

Now let a dielectric be placed between the ball and the electroscope, touch the latter, and remove the finger and ball as before, and much greater divergence will be produced. In both cases the electroscope is charged by induction. Without putting the electroscope to earth, I fail to see theoretically why any greater divergence should occur. I suppose someone must have made the experiment as quoted, but if a greater effect was produced it must have been caused by the substance used for a dielectric being charged itself. I have found very great difficulty in preventing plates of ebonite, paraffin, sulphur, &c., becoming electrified when placed near a charged body.

I should like to know if anyone has experimented in this direction, because either the text-books or myself must be wrong.

In Guthrie's book (p. 101) there is a statement similar to Ganot's.

W. A. RUDGE.

Who discovered the Teeth in Ornithorhynchus?

ON returning from Central Arizona, where I have been engaged in biological explorations, I find upon my desk an important paper entitled "On the Dentition of Ornithorhynchus," by my friend Mr. Oldfield Thomas, Curator of Mammals in the British Museum (see Proc. Royal Soc., vol. xlvi, 1889, 126–131, pl. 2).

The opening sentence of this paper is as follows: "At the meeting of the 9th of February, 1888, Mr. E. B. Poulton communicated to this Society the first discovery of the presence of teeth in *Ornithorhynchus*, a discovery which naturally awakened extreme interest throughout the scientific world." A few lines further on Mr. Thomas continues: "The grand fact of the presence of teeth in Monotremes, and their mammalian nature, are discoveries on which Mr. Poulton may well be congratulated."

From the above I infer that considerable stir has been made by the assumed new "discovery" that the young *Ornithorhynchus* has teeth.

If my British colleagues will turn to the masterly work of their illustrious countryman, Sir Everard Home, they will find in the second volume of his "Lectures on Comparative Anatomy" (published in 1814), no less than three beautifully engraved plates, containing eight figures, of the skull and mouth parts of Ornithorhynchus. Four of these figures show the teeth —two on each side of each jaw. The explanation accompanying Fig. 1, Tab. lix., is as follows: "A view of the upper jaw and palate, to show that there are two grinding teeth on each side." Fig. 2 is "a similar view of the under jaw."

Washington, D.C., October 12. C. HART MERRIAM.

ON THE HARDENING AND TEMPERING OF STEEL.[1]

I.

THE fact that the British Association meets this year at Newcastle no doubt suggested to the Council that it would be well to provide, for the first time since 1848, a lecture on a metallurgical subject. In that year a discourse was delivered at Swansea by Dr. Percy, one of the most learned metallurgists of our time, who has recently passed away, after having almost created an English literature of metallurgy by the publication of his well-known treatises, without which it would have been comparatively barren. It was to him that the country turned in 1851 when it became evident that our metallurgists must receive scientific training.

I know that it has occurred to many that the various problems involved in the "hardening and tempering of steel" must be incapable of adequate treatment in the brief limits of a discourse like this, while others will think that the details of the process, which is practised daily in thousands of workshops, are so well known that it is unnecessary to devote a lecture to the subject. It seemed to me that the entire question was the most important I could choose, partly because it will enable a large number of people who are engaged in industrial work, and who are not expected to think about it in a scientific way, to know how such facts as we shall have to examine have been dealt with by scientific investigators; while those of our members who do not consider that their thoughts or work are scientific in its strictest sense, may perhaps be interested to see how absolutely industrial progress depends upon the advancement of science. This consideration has led me to deal with the subject in a somewhat comprehensive way. The treatment of iron in its several forms is the thing that we as a nation do well. If it be true that national virtues are manifestly expressed in the industrial art of a people, we may recall the sentence in Mr. Ruskin's "Crown of Wild Olive" in which he says, "You have at present in England only one art of any consequence—that is, iron-working," adding, with reference to the manufacture of armour-plate, "Do you think, on those iron plates your courage and endurance are not written for ever, not merely with an iron pen, but on iron parchment?" It may be well, therefore, to consider what properties iron possesses which entitle its application to industrial use to specially represent the skill and patience of the nation.

In 1863, Lord Armstrong, in his address as President of this Association, expressed the hope "that when the time again comes round to receive the British Association in this town, its members will find the interval to have been as fruitful as the corresponding period," since the previous meeting in 1838, "on which they were then looking back." In one way at least this hope has been realized, for the efforts of the last twenty years have resulted in the development of an "age of steel." When the Association last met here, steel was still an expensive material, although Bessemer had, seven years before, communicated his great invention to the world through the British Association at its Cheltenham meeting. The great future in store for Siemens's regenerative furnace, which plays so important a part in the manufacture of steel, was confidently predicted in his Presidential address by Lord Armstrong, than whom no one was better able to judge, for no one had done more to develop the use of steel of all kinds.

Steel, we shall see, is modified iron. The name iron is in fact a comprehensive one, for the mechanical behaviour of the metal is so singularly changed by influences acting from within and without its mass, as to lead many to think, with Paracelsus, that iron and steel must be two distinct metals, their properties being so different. Pure iron may be prepared in a form as pliable and soft as copper, steel can readily be made sufficiently hard to cut glass, and notwithstanding this extraordinary variance in the physical properties of iron and certain kinds of steel, the chemical difference between them is comparatively very small, and would hardly secure attention if it were not for the importance of the results to which it gives rise. We have to consider the nature of the transformations which iron can sustain, and to see how it differs from steel, of which an old writer has said,[1] "Its most useful and advantageous property is that of becoming extremely hard when ignited and plunged into cold water, the hardness produced being greater in proportion as the steel is hotter and the water colder. The colours which appear on the surface of steel slowly heated direct the artist in *tempering* or reducing the hardness of steel to any determinate standard." There is still so much confusion between the words "temper," "tempering," and "hardening," in the writings of even very eminent authorities, that it is well

[1] A Lecture delivered on September 13, by Prof. W. C. Roberts-Austen, F.R.S., before the members of the British Association.

[1] "The First Principles of Chemistry," by W. Nicholson, p. 312 (London, 1790).

to keep these old definitions carefully in mind. I shall employ the word tempering in the sense of softening, as Falstaff uses it when he says of Shallow :—

"I have him already tempering between my finger and my thumb, and shortly will I seal with him." [1]

softening, that is, as brittle wax does by the application of gentle heat. *Hardening*, then, is the result of rapidly cooling a strongly heated mass of steel. *Tempering* consists in re-heating the hardened steel to a temperature far short of that to which it was raised before hardening: this heating may or may not be followed by rapid cooling. *Annealing* consists in heating the mass to a temperature higher than that used for tempering, and allowing it to cool slowly.

First, let the prominent facts be demonstrated experimentally.

[Three sword-blades of identical quality, made by an eminent sword-smith, Mr. Wilkinson, were taken. It was shown by bending one that it was soft; this was heated to redness and plunged into cold water, when it became so hard that it broke on the attempt to bend it. Another was bent into a bow, the arc of which was four inches shorter than the sword itself, a common test for "temper," and it sprang back to a straight line when the bending force was removed; this had been tempered. A third, which had been softened by being cooled slowly, bent easily and remained distorted.]

The metal has been singularly altered in its properties by comparatively simple treatment, and all these changes it must be remembered have been produced in a solid metal to which nothing has been added, and from which nothing material has been taken. The theory of this operation which I have just conducted has been laboriously built up, and its consideration introduces many questions of great interest both in the history of science, and in our knowledge of molecular physics. First as regards the history of the subject. The knowledge that steel might be hardened must have come to us from remote antiquity. Copper hardened with tin was its only predecessor, and it continued to be used very long after it was known that steel might be hardened. It would, moreover, appear that a desire to appreciate the difficulties of a people to whom cutting instruments of hard steel were unknown, seems to have induced experimenters in quite recent times to fashion implements of bronze, and a trustworthy authority tells us that "Sir Francis Chantry formed an alloy containing about 16 parts of copper, 2½ of zinc, and 2½ of tin, of which he had a razor made, and I believe even shaved with it." [2] The Greek alchemical manuscripts which have been so carefully examined by M. Berthelot give various receipts from which it is evident that in the early days the nature of the quenching fluid was considered to be all-important. There were certain rivers the waters of which were supposed to be specially efficacious. Pliny, who says that the difference between waters of various rivers can be recognized by workers in steel, also knew that oil might be used with advantage for hardening certain varieties of the metal. It is sad to think how many of the old receipts for hardening and tempering have been lost. What would we not give, for instance, for the records of the Gallic prototype of our Iron and Steel Institute, the "*Collegium Fabrorum Ferrariorum*,"[3] a guild with similar aims, formed in the time of the Roman Republic, for the advancement of knowledge, for the good of the State, and not for that of its individual members? The belief, however, in the efficacy of curious nostrums and solutions for hardening steel could hardly have been firmer at any period than in the sixteenth century of our era. Shakespeare suggests that Othello's sword "of Spain" had been hardened in a cold stream for he says it had

"the ice brook's temper";

but cold water was far too simple a material for many a sixteenth century artificer to employ, as is shown by the quaint recipes contained in one of the earliest books of trade secrets, which, by its title, showed the existence of the belief that the "right use of alchemy" was to bring chemical knowledge to bear upon industry. The earliest edition was published in 1531,[1] and the first English translation[2] in 1583, from which the following extracts may be of interest. "Take snayles, and first drawne water of a red die of which water being taken in the two firste moneths of haruest when it raynes," boil it with the snails, "then heate your iron red hote and quench it therein and it shall be hard as steele." "Ye may do the like with the blood of a man of xxx yeres of age, and of sanguine complexion, being of a merry nature and pleasaunt . . . . distilled in the middst of May." This may seem trivial enough, but the belief in the efficacy of such solutions survived into the present century, for I find in a work published in 1810 that the artist is prettily directed[3] "to take the root of blue lilies, infuse it in wine and quench the steel in it," and the steel will be hard; on the other hand, he is told that if he "takes the juice or water of common beans and quenches iron or steel in it, it will be soft as lead." I am at a loss to explain the confusion which has arisen from this source. As must always be the case when the practice of an art is purely empirical, such procedure was often fantastic, but it is by no means obsolete, for probably at the present day there is hardly a workshop in which some artificer could not be found with a claim to possess a quaint nostrum for hardening steel. Even the use of absurdly compounded baths, to which I have referred, was supported by theoretical views. Otto Tachen,[4] for instance, writing of steel in about the year 1666, says that steel when it is "quenched in water acquires strength because the light alcaly in the water is a true comforter of the light acid in the iron, and cutlers do strengthen it with the alcaly of animals," hence the use of snails. Again, Lemery[5] explains in much the same way the production of steel by heating iron in the presence of horns of animals.

I have dwelt so long on these points in order to bring out clearly the fact that the early workers attached great importance to the nature of the fluid in which hot steel was quenched, and they were right, though their theories may have been wrong. The degree of rapidity with which heat is abstracted from the steel during the operation of hardening is as important at the present day as it ever was. Roughly speaking, if steel has to be made glass-hard, ice-cold water, brine, or mercury, is used; if it has only to be made slightly hard, hot water or oil may be employed; while, as Thomas Gill[6] suggested in 1818, both "hardening" and "tempering" may be united in a single operation by plunging the hot metal in a bath of molten lead or other suitable metal, which will of course abstract the heat more slowly.

We must now trace the development of theories relating to the internal constitution of steel. The advent of the phlogistic school with the teaching of Becher and Stahl led to the view that iron gained phlogiston during its conversion into steel. By phlogiston we know that the early chemists really meant *energy*, but to them phlogiston was represented to be a kind of soul possessed by all metals,

[1] King Henry IV, Part II., Act iv., Scene 3.
[2] "Engines of War," by H. Wilkinson, p. 194 (1841).
[3] "La Ferronnerie," par F. Liger, t. ii. p. 147 (Paris, 1875).

[1] "Rechter Gebrauch d. Alchimei," 1531. There were many English editions.
[2] "A profitable boke declaring dyuers approoued remedies," &c. (London, 1583). See Prof. Ferguson's learned paper "On some Early Treatises on Technological Chemistry," Phil. Soc., Glasgow, January 1886.
[3] "The Laboratory or School of Arts," 6th edition, 1799, p. 228. There is a later edition of 1810.
[4] "His Key to the Ancient Hippocratical Learning," p. 68 (London, 1690).
[5] "A Course of Chemistry," 2nd edition, 1686, p. 131.
[6] *Thomson's Annals of Philosophy*, xii., 1818, p. 58.

which they could lose by burning and regain by the process they called "revivification." "Hardness [in metals] is caused by the jeiunenese of the spirit and their imparity with the tangible parts," said Francis Bacon;[1] while, according to Stahl,[2] steel was merely iron possessing, in virtue of its phlogiston, the characteristics of a metal in a higher degree; and this view prevails in the writings of Henckel, Newmann, Cramer, Gellert, Rinman, and Macquer. This opinion survived with wonderful persistence, but it did not influence the teaching of Réaumur,[3] who, in 1722, was, so far as I know, the first to suggest a physical theory which has been in any way justified by modern research. He assumed that when steel was heated "sulphurs and salts" were driven out from the molecules, which he represents diagrammatically, into the interstitial space between them. The quenching of the steel and its sudden cooling prevented the sulphurs and salts from returning into the molecules, which were thus firmly cemented by the matter between them, and hard rigid steel was the result. In tempering, the sulphurs and salts partially returned into the molecules, and the metal became proportionately soft. I have elsewhere shown[4] that he used the Torricellian vacuum to demonstrate that the hardening of steel is not accompanied by the evolution of gas, and he concluded that "since the hardening of steel is neither due to the intervention of a new substance nor to the expulsion of air, it only remains to seek its cause in the changes occurring in its structure." Notwithstanding this, the phlogistic school were not daunted, and this brings me to the work of Torbern Bergman, the great Professor at the University of Upsala, who in 1781 showed[5] that steel mainly differs from iron by containing about $\frac{2}{10}$ per cent. of plumbago, while iron does not. Read in connection with modern research, his work seems wonderfully advanced. He was so forcibly impressed by the fact that the great difference in the mechanical properties of different specimens of iron is due to the presence of small quantities of impurity, and that the properties of iron do not vary, as he says, unless by chance the iron has gathered foreign matter, "*nisi forte peregrinum paullo uberius inhærat metallum.*" We find, even, the dawn of the view that under the influence of small quantities of foreign matter iron is, as he calls it, polymorphous, and plays the part of many metals. "*Adeo ut jure dici queat, polymorphum ferrum plurium simul metallorum vices sustinere.*"[6] Unfortunately he confounded the plumbago or carbon he had isolated with phlogiston, as did Rinman in 1782, which was strange, because, in 1774, the latter physicist had shown that a drop of nitric acid simply whitens wrought iron, but leaves a black stain on steel. Bergman tenaciously held to the phlogistic theory in relation to steel; it was inevitable that he should. The true nature of oxidation had been explained; no wonder that the defenders of the phlogistic theory should seek to support their case by appealing to the subtle and obscure changes produced in iron by such apparently slight causes. Bergman's view was, however, combated by Vandermonde, Berthollet, and Monge,[7] who showed in a report communicated to the Académie des Sciences, in 1786, that the difference between the main varieties of iron is determined by variation in the amount of carbon, and further that steel must contain a certain quantity of carbon in order that it might possess definite qualities. Bergman died in 1784, and the report to which I have referred is full of respect for "this grand chemist," as its authors call him, "whom science had lost too soon."

[1] "Sylva Sylvarum," 2nd edition, 1628, p. 215.

[2] "Fundamenta Chemiæ," Part 3, p. 451, quoted by Guyton de Morveau in the article "Acier," "Encyc. Méthodique," p. 421 (Paris, 1786).

[3] "L'art de convertir le fer forgé en acier," p. 321 *et seq.* (Paris, 1722).

[4] Proc. Inst. Mech. Engineers, October 1881, p. 706.

[5] "Opuscula Physica et Chemica," vol. iii. "De Analysi Ferri" (Upsala, 1783). A dissertation delivered June 9, 1781.

[6] "De Analysi Ferri," p. 4.

[7] "Histoire de l'Académie Royale des Sciences," 1786 (printed 1788), p. 132.

Kirwan's essay on phlogiston,[1] in which Bergman's views were defended, elicited a reply from Lavoisier himself, and brought down the French school in strength to contest almost the last position occupied by the believers in phlogiston.[2]

An entire lecture might be profitably devoted to Bergman's work. His was almost the first calorimetric research, and is specially interesting when taken in connection with the calorimetric investigations of Lavoisier and Laplace in 1780, and it is impossible to read it without feeling that in paying the just tribute to Lavoisier's genius Bergman has been overlooked. He desired to ascertain whether pure iron, steel, and cast iron contain the same amount of heat. He therefore attacked the materials with a solvent, and noted the heat evolved. He says the solvent breaks up the assemblage of the aggregation of molecules and forms other unions. If the new body demands more heat than the body which has been disunited, then the thermometer will fall. If, on the other hand, the degree of heat required is less, the environment will be heated, which will result in the rise of the thermometer. The modern development is that, when a chemical compound is formed, heat is evolved and energy is lost, but if one substance, say a metal, simply dissolves another, the solution is attended with absorption of heat, and the product when attacked by a suitable solvent should evolve practically the same amount of heat, but certainly not less than would be evolved by the individual metals present in solution.[3] This is specially interesting from its relation to the calorimetric work of Lavoisier and Laplace in 1780 and of Lavoisier in 1782, which led the latter to explain the nature of oxidation, and to show that a metal could be as truly "calcined" or oxidized by the action of a solution as by the action of air at an elevated temperature. Now that the importance of thermochemistry is beginning to be recognized in relation to industrial chemistry and metallurgy, it is to be hoped that Bergman's merits will be more fully considered. We are, however, mainly concerned with the fact that he taught us that the difference between iron and steel consists in the $\frac{2}{10}$ to $1\frac{1}{2}$ per cent. of carbon which steel contains. It was only natural that Black, writing in 1796, should have attributed the hardening of steel to the "extrication of latent heat"; "the abatement of the hardness by the temper" being due, he says, "to the restoration of a part of that heat."[4] Black failed to see that the work of Bergman had entirely changed the situation. The next step was made in France. It was considered necessary to establish the fact that carbon is really the element which gives steel its characteristic properties, and with this object in view, Clouet,[5] in 1798, melted a little crucible of iron, weighing 57·8 grammes, containing a diamond, weighing 0·907 gramme, and obtained a fused mass of steel (Fig. 1).

His experiment was repeated by many observers, but the results were open to doubt from the fact that furnace gases could always obtain access to the iron, and might, as well as the diamond, have yielded carbon to the metal.

[1] R. Kirwan, "Essay on Phlogiston and the Constitution of Acids," p. 134 (1787).

[2] "Essai sur le Phlogistique," traduit de l'Anglois de M. Kirwan, avec des notes de MM. de Morveau, Lavoisier, de la Place, Monge, Berthollet, et de Fourcroy (Paris, 1788).

[3] See French translation of Bergman's work (Paris, 1783), p. 72. The question is, however, so important that I append the original Latin text:—"Menstruo laxatur compages molecularum, et nova formantur connubia, quæ, si majorem, quam diruta, figunt materiæ caloris quantitatem, in vicinia calor ad restituendum æquilibrium diminuatur oportet, et thermometri hydrargyrum ideo subsidet; si minorem, differentia liberatur et viciniam calefacit, unde etiam adscendit thermometri liquor; si denique nova connubia eamdem præcise quantitatem postulant, quod raro accidit, nulla in thermometro videbitur variatio."—Torberni Bergman, "Opuscula Physica et Chemica," vol. iii. p. 58, 1783 ("De Analysi Ferri").

[4] "Lectures on the Elements of Chemistry," vol. ii. p. 505 (1803).

[5] Experiment described by Guyton de Morveau, *Ann. de Chim.*, xxxi. 1799, p. 328.

The carbon might have been presented to the iron in the form of a gas capable of yielding carbon, and this element would as surely have found its way into the steel.

Margueritte,[1] for instance, in 1865, repeated Clouet's experiment, and showed that, although carburization can be effected by simple contact of iron and carbon, it is nevertheless true that in the ordinary process of cementation the gas carbonic oxide plays an important part, which had until then been overlooked. The discovery by Graham,[2] in 1866, of the occlusion of carbonic oxide by

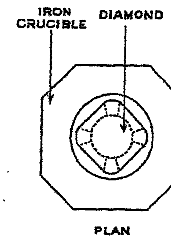


Fig. 1.—Plan of iron crucible and diamond from the drawing in Guyton de Morveau's paper. In the original, the diamond and the crucible are drawn, in plan, separately.

iron, gave additional support to this theory. I am glad to remember that he intrusted the experiments to me.

The question, however, of the direct carburization of iron by the diamond has never been doubted since 1815, when a working cutler, Mr. Pepys,[3] heated iron wire and diamond dust together and obtained steel, the heat being afforded by a powerful electric battery. I am anxious to make this absorption of carbon in the diamond form clear by this diagram (Fig. 2).

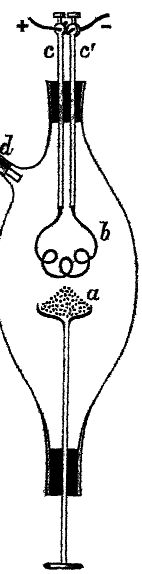


Fig. 2 represents a glass vessel which may either be rendered vacuous or may be filled with an atmosphere of gas through the tube *d*. An iron wire, *b*, placed between the terminals of a battery, *c*, *c′*, is heated to redness, and remains glowing until it is touched by pure diamond dust, which is effected by raising the cup *a*. The iron combines with the diamond dust and fuses.

Do not think for a moment that the steel owes its hardness to the passage of diamond into the iron, *as diamond*. I have repeated Margueritte's form of Clouet's experiment, using, however, a vacuum instead of an atmosphere of gas, and employing the form of apparatus shown in this diagram (Fig. 3). [The carburized iron which was the result of the experiment was thrown upon the screen.] The diamond by union with iron has passed partially at least to the other form of carbon, graphite, while treatment with a solvent which removes the iron shows that carbon has entered into intimate association with the iron, a fact which leads us to the next step in the study of the relations between carbon and iron.

[1] "Sur l'aciération," *Ann. Chim. et Phys.*, t. vi. [4], 1865.
[2] Phil. Trans. Roy. Soc., 1866, pp. 399-439.
[3] *Ibid.*, 1815, p. 371.

Hempel[1] has shown that, in an atmosphere of nitrogen, iron appears to assimilate the diamond form of carbon more readily than either the graphitic or the amorphous

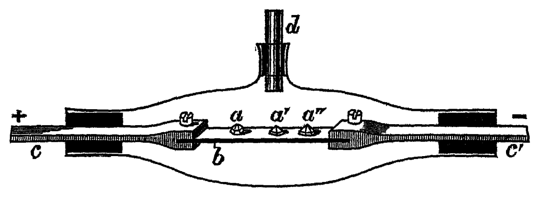


Fig. 3 represents an arrangement for heating the diamond and iron *in vacuo*. A strip of pure iron, *b*, is placed between two terminals, *c*, *c′*, which are connected with a dynamo. The vessel (of glass) is rendered vacuous by connecting the tube *d* with a Sprengel pump. The iron is then heated by the dynamo, and maintained glowing until all occluded gas is expelled from the iron, which is then allowed to cool *in vacuo*. Small pure diamonds, *a*, *a′*, *a″*, are then placed on the strip of iron through the orifice into which the tube *d* fits. The vessel is rendered vacuous, and when the iron is again heated in contact with the diamonds it fuses and combines with them.

forms, but directly carbon is associated with *molten* iron it behaves like the protean element it is, and the state which this carbon assumes is influenced by the rate of cooling of the molten mass, or even by the thermal treatment to which the solidified mass is subjected. Let me repeat, all are familiar with carbon in the distinct forms of diamond, graphite, and soot: all are alike carbon. It need not be considered strange, then, that carbon should be capable of being present in intimate association with iron, but in very varied forms.

Now the mode of existence of carbon in soft annealed steel is very different from that in which it occurs in hard steel. I believe that Karsten was the first to isolate, in 1827, from soft steel a true compound of iron and carbon. Berthier[2] also separated from soft steel a carbide of iron, to which he assigned the formula FeC; but to attempt to trace the history of the work in this direction would demand an entire lecture. I will only add that within the last few years Sir F. Abel has given much experimental evidence in favour of the existence in soft cold rolled steel of a carbide, Fe_3C, which he isolated by the slow solvent action of a chromic acid solution. His work has been generally accepted as conclusive, and has been the starting-point of much that has followed.

It will occur to you that the microscope should reveal wide differences between the structure of various kinds of iron and steel, and I am happy to be able to give you enlarged diagrams made from the drawings of Mr. Sorby, the eminent microscopist, which illustrated his very delicate investigations into the structure of steel.[3]

The point I am mainly concerned with is the existence of a substance which Sorby called the "pearly constituent" in soft steel. This pearly constituent is closely related to the carbide of iron, Fe_3C of Abel,[4] and is probably a mixture of Fe_3C and pure iron. I have diagrammatically indicated its presence in Fig. 4, which will enable me to summarize the work of many experimenters. The diagram (Fig. 4) will serve, for the purpose of illustration,

[1] *Ber. der deutsch. chem. Gesellschaft*, vol. xviii. p. 998.
[2] *Ann. des Mines*, t. iii., 1833, p. 229.
[3] The reader must refer to the Journal of the Iron and Steel Institute, No. i., 1887, 255.
[4] Proc. Inst. Mech. Engineers, January 1883.

to indicate the appearance when soft, hardened, and tempered steel are respectively treated with a solvent which acts gently on the mass.

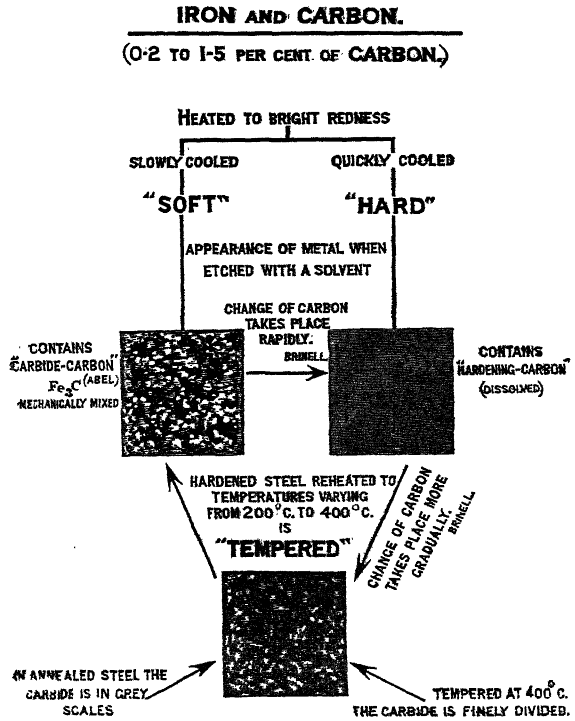


FIG. 4.

A study of the above diagram and of the admirable work of Ledebur[1] will show how complex the relations of carbon and iron really are, but, for the purposes of the present inquiry it may fairly be asked, Does a change in the "mode of existence" of carbon in iron sufficiently explain the main facts of hardening and tempering? It does not. It is possible to obtain by rapid cooling from a certain temperature steel which is perfectly soft, although analysis proves that the carbon is present in the form which we have recognized as "hardening carbon." No doubt in the hardening of steel the carbon changes its mode of existence, but we must seek some other theory to explain all the facts, and in order to do this we will turn to the behaviour of the iron itself.

In approaching this portion of the subject a few elementary facts relative to the constitution of matter must be recalled, and in doing so I must again appeal briefly to history. It is universally accepted that metals, like all elements, are composed of atoms of definite weights and volumes grouped in molecules. In order actually to transmute one metal into another it would be necessary to discover a method of attacking not the molecule but the *atom*, and of changing it, and this, so far as is known, has not yet been done; but it is possible, by influences which often appear to be very slight, to change the relations of the molecules to each other, and to alter the arrangements or distribution of the *atoms* within the *molecules*, and by varying in this sense the molecular arrangement of certain elements, they may be made to pass into forms which are very different from those in which we ordinarily know them. Carbon, for instance, when free, or when associated with iron, may readily be changed from the diamond to the graphitic form, though the converse change has not as yet been effected.

[1] *Stahl und Eisen*, vol. viii., 1888, p. 742.

Sulphur, again, with which you are familiar as a hard, brittle, yellow solid, may be prepared and maintained for a little time in the form of this brown viscous mass, but this latter form of sulphur soon passes spontaneously and slowly at the ordinary temperature, and instantaneously at 100°, to the solid octahedral yellow modification with evolution of heat. The viscous form of sulphur is an allotropic modification of that element. A few cases of allotropy in metals have already been established, and when they do occur they give rise to problems of vast industrial importance. Such molecular changes in metals are usually produced by the addition of a small quantity of foreign matter, and I have elsewhere tried to show that the molecular change produced by the action of *traces* upon *masses* is a wide-spread principle of nature, and one which was recognized at the dawn of the science of chemistry, even in the seventh century, although distorted explanations were given of well-known facts, and gave rise to entirely false hopes. But it is the same story now as in mediæval times: the single grain of powder which Raymond Lully said would transmute millions of its weight of lead into gold—the single grain of stone that Solomon Trismosin thought would secure perpetual youth—had their analogues in the small amount of plumbago which, to Bergman's astonishment in the eighteenth century, converted iron into steel. By his time it was recognized that the right use of alchemy consisted in the application of its methods to industry, and we still wonder at the minuteness of the quantity of certain elements which can profoundly affect the properties of metals. The statements are true, and are not derived from poetical literature, early or late. Even in the moral world the significance of the action of traces upon masses has been recognized, and the method of the alchemist survives in the administration of the small quantity of powder which, in the imagination of Robert Louis Stevenson, will produce the malevolent Hyde modification of the benevolent Dr. Jekyll. In thus borrowing an illustration from one of the most refined and subtle writers of our time, I do not fear the taunt of Francis Bacon,[1] that "sottishly do the chymics appropriate the fancies and delights of poets in the transformation of bodies to the experiments of their furnaces;" for, although it may not be possible to *transmute* metals, it is easy so to *transform* them, by very slight influences, that as regards special service required from them they may behave either usefully or entirely prejudicially.

In attempting to illustrate this part of the subject I cannot take the most striking cases, as it is difficult to demonstrate them in the time at my disposal. The following experiment, which does not, however, depend upon the action of a trace upon a mass, will enable me to lead up to the point I wish to insist upon. It consists in the release of gold from its alloy with potassium. When the alloy is treated with water, the gold comes down in a finely divided, dark brown, chemically active state. [Experiment shown on the screen.]

I have chosen this experiment because it was a similar one that first roused suspicion that pure iron could exist in more than one form.

The question at once suggests itself, Can iron behave in a similar manner: is an allotropic form of iron known? Joule afforded experimental evidence for an affirmative answer to this question nearly forty years ago by communicating to the British Association in 1850 a paper on some amalgams. The result of his experiments, published in detail later,[2] in a paper which has been sadly neglected, showed that iron released from its amalgam with mercury is chemically active, as it com-

[1] Preface to the "Wisdom of the Ancients."
[2] "On some Amalgams," Mem. Lit. Phil. Soc. Manchester, vol. ii. [3] p. 115.

bines readily with the oxygen of the air at the ordinary temperature, and he claims that the iron so set free is allotropic; but Joule did much more than this. Magnus had shown (1851) that the thermo-electric properties of hard and soft steel and iron differ. Joule, in a paper on some thermo-electric properties of solids, incidentally shows that the generation of a thermo-electric current affords a method of ascertaining the degree of carburization of iron, and he appeals to the "thermo-electricity of iron in different states" as presenting a "fresh illustration of the extraordinary physical changes produced in iron by its conversion into steel," and he adds the expression of the belief "that the excellence of the latter metal might be tested by ascertaining the amount of change in thermo-electric condition which can be produced by the process of hardening."[1] It is by a thermo-electric method that the views as to the existence of iron in allotropic forms has been confirmed. Jullien seems to have inclined to the view that iron is allotropic in his "Théorie de la Trempe,"[2] published in 1865, but he cannot be said to have added much to our knowledge, although he certainly directed attention to the importance of hardening and tempering steel.

The next step was made in Russia, in 1868. Chernoff, who has found an admirable exponent to English readers in Mr. W. Anderson, President of Section G, showed that steel could not be hardened by rapid cooling until it had been heated to a definite temperature—to a degree of redness which he called *a*. Then in 1873, Prof. Tait[3] used this expression in a Rede Lecture delivered at Cambridge: "It seems as if iron becomes, as it were, a different metal on being raised above a certain temperature; this may possibly have some connection with the ferricum and ferrosum of the chemists." He also published his now well-known "first approximation to a thermo-electric diagram," which is of great interest in view of recent work. At about this time those specially interested in this question remembered that Gore[4] had shown that a curious molecular change could be produced by heating an iron wire, which sustains a momentary elongation on cooling. Barrett repeated Gore's experiment, and discovered that as an iron wire cools down it suddenly *glows*, a phenomenon to which he gave the name *recalescence*, and these investigations have been pursued and developed in other directions by many skilful experimenters.[5] In 1879, Wrightson[6] called attention to the abnormal expansion of carburized iron at high temperatures.

The next point of special importance seems to me to be that recorded by Barus, who, by a thermo-electric method, showed, in an elaborate paper published in 1879,[7] that "the hardness of steel does not increase continuously with its temperature at the moment of sudden cooling, but at a point lying in the dark-red heat the glass-hard state" may suddenly be attained by rapid cooling. I shall have again to refer to the remarkable series of papers published by Barus and Strouhal,[8] embodying the results of laborious investigations, to which, in the limited space of this lecture, I can do but scanty justice; and finally, within the last few years, Pionchon[1] showed that at a temperature of 700° the specific heat of iron is altogether exceptional, and Le Chatelier[2] has detected that at the same temperature a change occurs in the curve representing the electromotive force of iron—both experimenters concluding that they had obtained evidence of the passage of iron into an allotropic state.

Osmond,[3] in France, then made the observations of Gore and Barrett the starting-point of a fresh inquiry, which will now be considered at some length, as Osmond has arrived at conclusions of much interest and importance.

(*To be continued.*)

[1] Phil. Trans., cxlix., 1859, p. 91.

[2] "Annexe au traité de la Métallurgie du Fer," 1865.

[3] NATURE, viii., 1873, pp. 86, 122; and Trans. Roy. Soc. Edin., xxvii., 1873, p. 125.

[4] Proc Roy. Soc., xvii., 1869, p. 260.

[5] G. Forbes, Proc. Roy. Soc. Edin., viii., 1874, 363; Norris, Proc. Roy. Soc., xxvi., 1877, 127; Tomlinson, *Phil. Mag.*, xxiv., 1887, 256; xxv., pp. 45, 103, and 372; xxvi. p. 18; Newall, *Phil. Mag.*, xxiv., 1887, 435; xxv, 1888, p. 510.

[6] Journ. Iron and Steel Inst., No. ii. 1879; No. i. 1880.

[7] Barus, *Phil. Mag.*, viii., 1879, p. 341.

[8] "Hardness (Temper), its Electrical and other Characteristics," Barus, *Phil. Mag.*, viii p. 341, 1879; *Wied. Ann.*, vii. p. 383, 1879; Strouhal and Barus, *Wied. Ann.*, xi. p. 930. 1880; *ibid.*, xx. p. 525, 1883. "Hardness and Magnetization," *Wied. Ann.*, xx. pp. 537, 662. 1883. "Density and (Internal) Structure of Hard Steel and of Quenched Glass," Barus and Strouhal, *American Journ.*, xxxi. p. 386, 1886; *ibid.*, p. 439; *ibid.*, xxxi. p. 181, 1886. "Temper and Chemical Composition," *Am. Journ.*, xxxii. p. 276, 1886 "Temper and Viscosity," *Am. Journ.*, xxxii. p. 444, 1886; *ibid.*, xxxiii. p. 20, 1887; Barus, *ibid.*, xxxiv. p. 1, 1887; *ibid.*, xxxiv. p. 175, 1887. These papers, systematically discussed and enlarged, are embodied with new matter in the *Bulletins* of the United States Geological Survey, viz.:—*Bull.*, No. 14, pp. 1-226, 1885; *Bull.*, No. 27, pp. 30-61, 1886; *Bull.*, No. 35, pp. 11-60, 1836; *Bull.*, No. 42, pp. 98-131, 1887.

[1] *Comptes rendus*, cii., 1886, pp. 675 and 1454, ciii. p. 1122.

[2] *Ibid.*, cii. p. 819.

[3] The reader will find the principal part of Osmond's work in the following papers: Osmond et Werth, "Thé rie Cellulaire des Propriétés de l'Acier," *Ann. des Mines*, vii., 1885. p. 5; "Transformations du Fer et du Carbone," Paris, Baudoin et Cie., 1888; "Études Métallurgiques," *Ann. des Mines*, Juillet-Août, 1888. There is also a very interesting paper, "Sur les Nouveaux Procédés de Trempe," which he communicated to the Mining and Metallurgical Congress, Paris, 1889.

ON A NEW APPLICATION OF PHOTOGRAPHY TO THE DEMONSTRATION OF CERTAIN PHYSIOLOGICAL PROCESSES IN PLANTS.

MR. WALTER GARDINER, Lecturer on Botany in the University of Cambridge, who delivered the evening address at Newcastle on "How Plants maintain themselves in the Struggle for Existence," has discovered a new method of printing photographic negatives, employing living leaves in place of sensitive paper. Mr. Gardiner read a paper on the subject before the British Association. Before dealing with the immediate subject of his paper, the author described how prints may be obtained from *Protococci*, or the free-swimming swarm-spores of many green Algæ. It is possible to take advantage of their sensitiveness to light. Into one end of a water-tight box, a thin glass plate is securely fitted. The negative to be printed is then placed next the glass, film side nearest. The box is filled with water containing a fairly large quantity of swarm-spores. The lid is shut down, and the whole is exposed to diffused light. In the case of a strong and well-developed negative, the swarm-spores swim towards the most highly-illuminated parts, and there in the greatest numbers come to rest, and settle upon the glass, so that, after some four or six hours, on pouring out the water and removing the negative, a print in green swarm-spores can be obtained. The print may be dried, fixed with albumen, stained, and varnished. The author then dwelt upon the well-known fact that the whole of the animal life upon the globe depends directly or indirectly upon the wonderful synthetic formation of proteid and protoplasm which takes place in the living tissue of plants containing chlorophyll, *i.e.* green plants, or, to be more exact, in the green chlorophyll corpuscles. He stated that, whatever is the exact chemical nature of the process, this is at least clear, that the first *visible* product of the assimilatory activity is starch, which, moreover, is found in the chlorophyll grains. The presence of this starch can be made manifest by treating a decolorized leaf with a water solution of iodine dissolved in potassic iodide. This formation of starch only takes place under the influence of light; the radiant energy of the sun providing the means of executing the profound synthetic chemical change, and building up proteid from the carbonic acid of the air which is taken up by the leaves and the salts and water of the soil absorbed by the roots. If a plant (and preferably a plant with thin leaves) be placed in the dark over-night, and then brought out into the light next morning, the desired leaves being covered with a sharp and well-developed negative, starch is formed

when light is transmitted, and in greatest quantity in the brightest areas. Thus a positive in starch is produced which can be developed by suitable treatment with iodine. [A leaf was then developed, and handed round to the audience for inspection.] The author showed that it might be possible to obtain a permanent print by suitable washing and treatment with a soluble silver salt, silver iodide being formed. The author regards this discovery as a most striking illustration of the way in which plants are working for themselves, and so for all living things, and points out that the extraordinary manner in which the green parts of plants (so to speak) catch the radiant energy of the sun, and employ it for analytical and synthetical chemical processes, may be easily and clearly demonstrated.

NOTES.

WE understand that the late Mr. John Ball, F.R.S., has bequeathed his botanical library and herbarium to Sir Joseph Hooker, to the Director of the Royal Botanic Gardens at Kew for the time being, and to the President of the Royal Society for the time being, requesting them to give the same to such person or persons or public institution in this country, the British colonies, or elsewhere in the world, as they or any two of them may select, with the sole object of promoting the knowledge of natural science. Right is, however, reserved for Kew to select previously such specimens or books as it may want.

THE following is the list of names recommended by the President and Council of the Royal Society for election into the Council for the year 1890, at the forthcoming anniversary meeting on the 30th inst.:—President: Sir George Gabriel Stokes, Bart. Treasurer: Dr. John Evans. Secretaries: Prof. Michael Foster, the Lord Rayleigh. Foreign Secretary: Dr. Archibald Geikie. Other Members of the Council: Prof. Henry Edward Armstrong, Prof. William Edward Ayrton, Charles Baron Clarke, Prof. W. Boyd Dawkins, Dr. Edward Emanuel Klein, Prof. E. Ray Lankester, Dr. Hugo Müller, Prof. Alfred Newton, Captain Andrew Noble, C.B., Rev. Stephen Joseph Perry, Sir Henry E. Roscoe, Dr. Edward John Routh, William Scovell Savory, Prof. Joseph John Thomson, Prof. Alexander William Williamson, Colonel Sir Charles William Wilson, R.E.

IN the list of Englishmen decorated in connection with the British Section of the Paris Exhibition, the names of the following men of science are included:—Grand Officer of the Legion of Honour: Sir William Thomson, F.R.S. Officers of the Legion of Honour: Sir Douglas Galton, K.C.B., Sir Henry Roscoe, M.P., F.R.S., Mr. W. H. Preece, F.R.S. Chevaliers of the Legion of Honour: Prof. Francis Elgar, Prof. W. Roberts-Austen, F.R.S., Dr. C. Le Neve Foster. Officer of Public Instruction: Mr. C. V. Boys, F.R.S.

THE Naturforschende Gesellschaft at Emden is to celebrate its seventy-fifth anniversary on December 29 next. The Society was founded in 1814 by twenty-four burgesses of Emden. The festivities in December will consist of a general meeting of the Society and the Society's correspondents at noon in the Museum, and a *Festessen* at four o'clock.

A REPORT of the proceedings of the International Zoological Congress, held in Paris two months ago, will be published shortly.

A FRENCH translation of Dr. Wallace's "Darwinism" will be published next year.

THE greater part of the ethnographical collection sent to the Paris Exhibition is to remain in Paris, in the Colonial Museum.

THE following botanical appointments are announced:—The Directorship of the Botanic Garden at Berlin, vacant by the death of Dr. Eichler, having been conferred on Prof. Engler, of Breslau, Prof. Urban becomes Second Director of the Berlin Botanic Garden; and Prof. Prantl, of Aschaffenburg, succeeds Prof. Engler as Director of the Botanic Garden at Breslau. Prof. Sadebeck, of Hamburg, is appointed Director of the Botanic Garden in that town, in the place of the late Dr. Reichenbach. Dr. G. von Lagerheim vacates the Professorship at Lisbon, to which he was lately appointed, and goes to Ecuador as Professor of Botany and Director of the Botanic Garden at Quito. Dr. H. Molisch, of Vienna, takes the Chair of the late Dr. Leitgeb in the Polytechnic at Gratz. Dr. F. Hueppe is appointed Professor of Bacteriology at the University of Prague, and is succeeded in the same Chair at Wiesbaden by Dr. G. Frank, of Berlin. The venerable Professor von Naegeli retires from the Directorship of the Botanic Garden at Munich. Mr. F. S. Earle, Prof. E. S. Goff, and Prof. L. R. Taft have been appointed special agents in the Section of Vegetable Pathology of the United States Department of Agriculture. Mr. H. H. Rusby has been appointed Professor of Botany and Materia Medica in the New York College of Pharmacy.

THE Economic Museum, Calcutta, has completed and despatched the first instalment of important Indian fibres required by the India Office for presentation to the Museums of the Royal Botanical Gardens at Kew and Edinburgh, and to the Chambers of Commerce at Dundee and Manchester.

A PRIZE of about £20 is offered by the Geographical Societies of Dresden and Leipzig, for "a physico-geographical description of the course of the Elbe between Bodenbach and its entrance on the flat country, with special reference to depth, quantity of water and its variations, ice, and changes in the form of the banks." The date is the end of 1890.

IN his address at the opening of the winter session of the University of Toronto, Sir Daniel Wilson, the President of the University, referred to the recent Toronto meeting of the American Association for the Advancement of Science. "Everything available for the special requirements of the Association," he said, "was placed at the disposal of the Sections; and we are gratified by the assurance that, at the close of a highly successful meeting, our visitors carried away with them pleasant memories of their reception here." The meeting of the representatives of science in the buildings of the Toronto University was in some respects, as the President pointed out, peculiarly opportune. "The long-felt need of adequately furnished and equipped laboratories and lecture-rooms for our scientific staff was anew brought into prominence by the restoration to the University of its Medical Faculty; and we now enter on the work of another year provided with buildings admirably adapted for biological and physiological study and research. Plans, moreover, have been approved of, which, when carried out to their full extent, will furnish equally satisfactory accommodation for the departments of botany, chemistry, geology, and palæontology, along with laboratories, work-rooms, museum, and other requisites for efficient instruction in the various branches of science."

THE thirty-fourth general meeting of the Society for Psychical Research was held on Friday afternoon, October 25, at the Westminster Town Hall. The President (Prof. Sidgwick) gave an account of the International Congress of Experimental Psychology held in Paris last August. The Congress had adopted the scheme of a census of hallucinations, already set on foot by the Society for Psychical Research in England, France, and the United States, and it was hoped that the collection of statistics might gradually be extended to other European countries. Much matter valuable to psychologists was

thus being collected; and he trusted that fresh light would be thrown on the subject of coincidental or veridical hallucinations, which specially interested their Society. He would be glad to supply information in reply to letters addressed to him at Hill Side, Cambridge. A paper on recent telepathic experiences was also read.

We learn from *Humboldt* that the project of a lacustrine biological station on Lake Plön, in East Holstein, is likely to be soon carried out, thanks to the energy of Dr. Otto Zacharias, and the liberality of the Bohemian Baron Bela Dertcheni. This station is to afford Prof. Anton Fritsch, of Prague, and his assistants, constant opportunities of research on fresh-water fauna. The scheme finds a good deal of favour in Berlin, and it is hoped that the researches at the station may prove of considerable benefit to fisheries.

We send to America some return for the Colorado beetle and the Canadian water-weed. The "weed-law" of the State of Wisconsin requires from farmers, under penalties, the destruction of the following weeds:—*Cnicus arvensis*, *Arctium Lappa*, *Chrysanthemum Leucanthemum*, *Sonchus arvensis*, *Xanthium strumarium*, *Linaria vulgaris*, and *Rumex crispus*. Only one of these is a native of the United States; all the rest being naturalized importations from Europe, and common wild plants in this country.

Prof. Righi showed, last year, that ultra-violet radiations reduce to the same potential two conductors, a plate and a piece of netting, applied to each other, the rays being thrown on the netting-side. He now points out (*Riv. Sci. Ind.*, July–August) that this suggests a very simple and convenient way of measuring differences of potential of contact. One notes the deflection of an electrometer connected with the plate (the netting being permanently connected with earth); then, having connected the electrometer for an instant with earth, makes the radiations act a sufficient time. He used a zinc electric lamp, and the metals examined were placed in some cases in a bell jar, to which some gas or vapour was admitted. From measurements of different plates with the same metallic net (copper, zinc, or platinum), the differences of potential of pairs of metals could be deduced. Prof. Righi found the differences sensibly the same in dry and moist air and in carbonic anhydride; but with hydrogen, very different values (from those in air) appeared, where one of the metals examined was platinum, palladium, nickel, or iron (doubtless owing to absorption). In ammonia all the metals, examined with zinc net, seemed to have become less oxidizable; and in coal gas, carbon and platinum behaved like more oxidizable metals. A memoir on the subject will shortly appear.

In an interesting paper on the management of aquaria, printed in the Bulletin of the United States Fish Commission, Mr. W. P. Seal points out that, in the feeding of the fish, care must be taken to introduce no more food than they can eat in a short time, as what is not eaten will soon decompose and make the water cloudy, and generate noxious gases as well. If due care is observed in regard to quantity, it does not matter how often fish are fed, except that if fed abundantly they will grow rapidly, which is not generally desired. Fish may be fed every day, or but two or three times a week, with equally good results apparently. They will always find a small amount of food in the aquarium in the vegetation. Where they are not fed sufficiently, they are apt to strip the plants of their leaves. In a natural condition fish are feeding continually and grow very rapidly.

On November 2 a slight shock of earthquake was felt in St. Louis, U.S.A., and the vicinity.

The following summary of the phases of Vesuvius during the past year has been supplied by Prof. Palmieri, of the Vesuvian Observatory of the University of Naples, to the British Consul there, and is appended by the latter to his last Report. Mount Vesuvius, during the past year, has continued its moderately eruptive activity, which began in the month of December 1875. There were various emissions of small lava streams, which did not reach further than the base of the cone. An additional cone was gradually formed, caused by the activity of the motive power of the crater which, towards the end of the year, had reached a height of 100 metres (equal to 328 feet) above its original level. On various occasions the detonations and the red-hot projectiles thrown up with the large quantities of smoke indicated greater eruptive power. During the whole year no ashes were thrown up, and consequently the crops in the surrounding country were not destroyed. The sublimations on the smoke issues were relatively scarce, and did not present any product that called for attention. The seismographic instruments at the Observatory did not show an activity proportionate to that of the volcano. All the lava streams that issued during the year flowed towards the eastern slopes of the mountain.

The Meteorological Council have published Part I. of the Quarterly Weather Report for 1880. The work is (as before) divided into three sections: (1) a general summary of the chief features of the weather for the quarter; (2) tables showing the movements and peculiarities of the principal cyclonic and anti-cyclonic systems; and (3) remarks on the distribution of the various elements for each month, illustrated by charts. An appendix contains tables and diagrams illustrating the diurnal range of the barometer in Great Britain and Ireland during the years 1876–80, by F. C. Bayard. The data used are the hourly observations at seven Observatories in connection with the Meteorological Office, and at Greenwich and Liverpool Observatories. The paper shows that, even in these high latitudes, the daily range is well marked during all months, notwithstanding the interference caused by non-periodic changes. Important seasonal differences are shown, the morning maximum being distinctly higher than the evening maximum in winter, while in summer the evening maximum is the higher of the two. The values exhibit the influence of locality on the amplitude and epoch of the diurnal inequalities, and furnish material for more minute inquiry.

It is interesting to read of a part of the world where the buffalo is not dying out, but increasing in numbers. A journal of Perth, in Western Australia, says that few Australians are aware that certain parts of Northern Australia have vast herds of the wild buffalo (*Bos bubalus*) careering over its plains and wallowing in its shady pools. The *Sydney Mail* states that the animals are massive and heavy, with splendid horns, and afford sport of a sufficiently dangerous nature to possess charms for the most daring hunter, a wounded buffalo being one of the most dangerous animals known, his great weight, prominent horns, and splendid courage, making him as well respected as sought after. The first buffaloes were landed at Port Essington, North Australia, about the year 1829.

The *Naturalist's Gazette* has issued an excellent series of what it calls "label lists." On one sheet there is a list of British birds' eggs; on another, a list of dragon-flies; on another, a list of British butterflies; and so on. The names are printed in suitable type on gummed paper, and collectors, in labelling their specimens, will find the lists of considerable service.

The next volume of Messrs. Ward, Lock, and Co.'s "Minerva Library of Famous Books" will be "Travels on the Amazon and Rio Negro," by Dr. Alfred Russel Wallace.

F. A. Brockhaus, 16 Querstrasse, Leipzig, has issued a catalogue, in four parts, containing lists of works relating to various branches of botany.

THE *Colonies and India* states that a discovery has recently been made on a Fiji plantation, which will probably prove extremely valuable in all tropical countries where the cultivation of bananas is regarded as a settled industry. The banana disease had for some time been causing much havoc on a plantation on Vanua Levu, and it appears that the discovery of an antidote was due to an accidental occurrence. On a flat near the sea-shore there was a patch of bananas much diseased, and some time ago the sea swept into it and remained on it for about an hour. All the plants were killed as far as the standing stems were concerned, but vigorous young shoots came up freely from the roots, and were not only quite free from disease, but soon began to bear much larger bunches of fruit than the parent plants ever did. Upon noting this effect the planters determined to try the experiment upon a number of badly diseased plants which the sea had not reached. They cut down the diseased plants, and, having stirred the ground about them, poured from one to four buckets of sea-water over each. The result was that, while the parent stems withered, vigorous young shoots came freely away, without a sign of disease.

A SERIES of successful experiments upon the simultaneous production of pure crystals of sodium carbonate and chlorine gas from common salt are described by Dr. Hempel in the current number of the *Berichte*. The experiments simply consisted in passing a current of carbon dioxide gas through a solution of salt contained in a special form of electrolytic cell, through which an electric current from a few Bunsen's cells or a small dynamo was circulated. The kathode found most convenient consisted of a plate of iron or carbon perforated with numerous holes about 4 millimetres in diameter, bored obliquely, so that bubbles of gas could readily escape upwards. For anode a similar plate of thin perforated carbon was employed. Both electrodes were circular in shape, and between them was placed a diaphragm of thick asbestos paper, which was directly squeezed between the two plates. This arrangement was found to possess the double advantage of bringing the two electrodes within 1 millimetre of each other, and so greatly diminishing the internal resistance, and of affording such excellent support to the asbestos diaphragm that any rupture of the latter was entirely prevented. The electrodes and their enclosed diaphragm were supported in a circular glass cell in such a manner that they divided the cell into two distinct chambers. To the glass wall of the cell on the positive or anode side was fitted a wide side tube, through which the salt was supplied as often as necessary in solid pieces, a little water being also from time to time added to replace that taken up in the crystallization of the sodium carbonate. A delivery tube was also attached to the upper portion of the anode chamber in order to conduct away the liberated chlorine gas. The negative or kathode chamber was supplied at its upper end with an opening serving on the one hand to introduce the carbon dioxide delivery tube, and on the other to extract the crystals of sodium carbonate. The apparatus was thus found to work continuously for weeks together, the asbestos diaphragm withstanding the pressure very satisfactorily. The separation of the soda crystals is readily explained by the well-known fact of the difficult solubility of sodium carbonate in solutions of sodium chloride; as fast as the electric current decomposes the sodium chloride into chlorine and sodium, the carbon dioxide converts the sodium hydrate formed by the reaction of the sodium upon water into the normal carbonate, which, in presence of the constantly replenished common salt, at once separates in the usual monoclinic form of $Na_2CO_3 . 10H_2O$. The total resistance of the cell is only about five and a half volts, which may be still further reduced by constructing both electrodes of carbon. Using a small dynamo-electric machine, 64·5 grams of chlorine and 259·8 grams of $Na_2CO_3 . 10H_2O$ per horse-power of 680 volt-amperes were produced per hour, so that the experiments, in addition to their interest from a purely chemical point of view, may turn out to bear fruit technically. The soda produced is stated to be chemically pure, and the chlorine to contain but a very small admixture with other gases.

THE additions to the Zoological Society's Gardens during the past week include a Patas Monkey (*Cercopithecus patas* ♂) from West Africa, presented by the Rev. James Vernall; a Cheetah (*Cynælurus jubatus* ♂) from South Africa, presented by Captain M. P. Webster, s.s. *Roslin Castle*; a Ring-tailed Coati (*Nasua rufa* ♀) from South America, presented by Mr. J. A. Martin; two Short-toed Larks (*Calandrella brachydactyla*) from Devonshire, presented by Commander W. N. Latham, R.N., F.Z.S.; a Sharp-nosed Crocodile (*Crocodilus acutus*) from Jamaica, presented by the Jamaica Institute; two Tuatera Lizards (*Sphenodon punctatus*) from New Zealand, presented by Rear-Admiral Henry Fairfax, R.N., C.B., F.Z.S.; a Smooth-headed Capuchin (*Cebus monachus* ♂) from Brazil, deposited; a Collared Peccary (*Dicotyles tajaçu* ♀), four Rosy-billed Ducks (*Metopiana peposaca* ♂ ♂ ♀ ♀) from South America; two Grey Squirrels (*Sciurus cinereus*) from North America; four —— Finches (*Munia nana*) from Madagascar, purchased.

OUR ASTRONOMICAL COLUMN.

STELLAR PARALLAX BY MEANS OF PHOTOGRAPHY.—Prof. Pritchard has sent us his eminently successful "Researches in Stellar Parallax" by the aid of photography, from observations made at the Oxford University Observatory. The advantage in point of convenience and rapidity in the multiplication of observations which this method possesses over all others is incalculable, and it is interesting to note that in the case of 61_1 Cygni the parallax obtained was $0''·4294 \pm 0''·0162$, and that Bessel's probable error is practically identical with this here stated. Hence, as far as the present results are concerned, photographic and heliometric measures of parallax may be regarded as possessing an equality of accuracy.

The following list contains the stars whose parallax has been determined by this novel method, and some of the results obtained:—

61_1 Cygni	+ 0''·429	± 0''·016
61_2 ,, ...	+ 0·432	± 0·019
μ Cassiopeiæ	+ 0·021	± 0·023
Polaris	+ 0·052	± 0·011
α Cassiopeiæ	+ 0·035	± 0·024
β ,, ...	+ 0·157	± 0·036
γ ,, ...	− 0·032	± 0·026
α Cephei	+ 0·073	± 0·031

The almost identical parallax of the two components of 61 Cygni is worthy of note. The average of eight determinations gives a value $0''·437$, which is a close approximation to Dr. Belopolsky's value of 0·50 as the *absolute* parallax of 61 Cygni.

Bessel determined a small negative parallax for μ Cassiopeiæ, but Dr. Struve assigned it a value $+ 0''·342$. The very small positive parallax given by Prof. Pritchard may be explanatory of Bessel's negative determination.

The small negative parallax found for γ Cassiopeiæ would indicate that it and the comparison stars are in the same group, although its bright line spectrum points to a constitution different from that of other stars in this constellation.

Even a cursory examination of the summary of results renders it evident that no relation exists between the lustre and parallax of stars, and indeed, since we probably view bodies which are still in various stages of condensation, we should hardly expect to find any such relation.

MEASUREMENTS OF DOUBLE STARS.—*Astronomische Nachrichten*, Nos. 2929–30, contain a series of double star observations made with the 36-inch refractor of the Lick Observatory by Mr. S. W. Burnham. The discovery is claimed of two very faint stars in the trapezium of Orion, and an excessively faint double has also been detected by Mr. E. E. Barnard just outside and preceding the trapezium. The observers believe that, in spite of the numerous alleged discoveries of faint stars in this

region, it is impossible to see such as these now found with an aperture much less than that of the Lick telescope. A list is therefore given of the principal communications to astronomical periodicals relating to the alleged discovery of faint stars in the trapezium of Orion.

BARNARD'S COMET, 1888-89.—*Comptes rendus*, No. 17, October 21, 1889, contains some observations made by MM. Rayet and Courty of the motion of Barnard's comet, the positions of the comparison stars being also given. The series of observations extend from September 11, 1888, to September 27, 1889.

BIOGRAPHICAL NOTE ON J. C. HOUZEAU.—M. A. Lancaster, the collaborator with Houzeau of the most comprehensive bibliography extant, has proved himself, in this note, to be the most capable of writing his deceased friend's biography. Houzeau's scientific and literary labours cover an extensive field: astronomy and geodesy, mathematics and meteorology, geology and geography, are all represented in his works; and when but a young man, he directed the triangulation of his country. In politics Houzeau was an enthusiast, and whilst in America, about 1861-69, he gave a considerable amount of attention to the subject of the emancipation of the slaves, and wrote numerous and important articles upon it. In 1875, Houzeau completed a series of astronomical and meteorological observations made at Jamaica, and in the following year was appointed Director of the Brussels Observatory. His crowning work—the "Vade Mecum of Astronomy," was finished in 1882. It represented the work of a lifetime, and as a guide to astronomers is invaluable. Such a compilation, however, calls for continual additions, and a general bibliography was published in 1887, with the assistance of M. A. Lancaster. This was Houzeau's last work, but before his death, on July 12, 1888, he earnestly expressed the wish that it should be carried on by his collaborator. Houzeau's life was full of vicissitudes, and his biography is most interesting.

THE KARLSRUHE OBSERVATORY.—The third volume of the Publications of the Grand-Ducal Observatory of Karlsruhe has recently been published by Dr. W. Valentiner, the Director. The bulk of the volume is by Dr. E. von Rebeur-Paschwitz, and consists, first, of a series of measures with the 6-inch refractor of the two star-clusters M. 35 and M. 25; secondly, of a discussion of the orbit of Comet Wells, 1882 I., and the derivation of definitive elements; and lastly, of auxiliary tables for the computation of parallax for 169 different observatories.

Dr. Boy Mattheissen adds a short paper on the orbit of Comet Denning, 1881 V.

The volume contains three plates, the first two being maps of the star-clusters under observation, whilst the third gives photographs of the same two clusters as taken by Dr. E. von Gothard at Herény.

OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at Greenwich, at 10 p.m. November 7 = 1h. 9m. 9s.

Name.	Mag	Colour.	R.A. 1890·0.	Decl. 1890·0.
			h. m. s.	° ′
(1) Nebula in Andromeda	—	Greenish-white.	0 35 4	+48 30·14
(2) γ Cassiopeiæ	2	Bluish-white.	0 50 1	+60 7
(3) 47 Piscium	5	Yellowish-red.	0 42 58	+ 6 59·2
(4) ι Ceti	3	Yellowish-white.	0 13 48	− 9 26
(5) γ Pegasi	3	White.	0 7 34	+14 34
(6) D.M. + 34° 55′	8	Deep red.	0 21 42	+34 53
(7) T Herculis	Var.	Reddish.	18 4 56	+31 0

Remarks.

(1) Dr. Huggins notes that the spectrum ends abruptly in the orange. Maxima of brightness have since been recorded by myself at, approximately, 468-474, 517, and 546, and the latter two have also been confirmed by Mr. Taylor. Further confirmation is required. For comparison, a Bunsen or spirit-lamp flame will be found convenient for the first two, and the brightest fluting seen when lead chloride is introduced into the flame for the third. Mr. Lockyer suggests that since the central condensation is probably at a higher temperature than the surrounding portions of the nebula, different parts of the nebula should show differences in their spectra. Observing with Mr. Lockyer's 30-inch reflector at Westgate-on-Sea, on October 20, I suspected some change in the spectrum away from the nucleus, but was unable to complete the observation on account of clouds, and have not since had an opportunity of repeating it.

(2) The bright lines most constantly seen in the spectrum of this star are C, F, and D_3, but their appearance is somewhat irregular. Continuous observations, with special reference to the relative intensities of the lines, are suggested. The lines are well seen in a 10-inch equatorial with a Maclean spectroscopic eye-piece. Bright flutings of carbon have also been suspected, and comparisons should be made with the Bunsen or spirit-lamp to confirm these. The continuous spectrum should also be carefully examined for maxima. *b*, D, and other absorption-lines, have also been recorded.

(3) This is a star which gives a spectrum of dark flutings fading away towards the red. Dunér records bands 2 to 9, and describes the spectrum as superb. Band 3, near D, is of extraordinary width. The spectra of this type have been explained as mixed metallic fluting absorption and carbon fluting radiation. The carbon flutings probably present are 517 and 468-474, which again may be determined by comparison with the spirit-lamp, 517 being the brightest green fluting.

Dunér's notation and mean wave-lengths of the dark bands are as follows:—(1) 648-666, (2) 616·2-629·8, (3) 586·7-596·8, (4) 559·8-564·9, (5) 545·2-551·5, (6) 524·3-528·1, (7) 516·8-522·2, (8) 495·9-503·0, (9) 476·0-483·0, (10) 460·7-473. The bright spaces between 7 and 8, and 9 and 10 are probably due to carbon.

(4) This is a star of Class II.*a*, which is now divided into two groups, one having spectra of the type of α Tauri (Group III.), and the other of the sun (Group V.). The lines should be carefully observed, and differences from the solar spectrum, if any, noted, so that the star can be classed in one group or the other. The principal criteria so far determined for Group III. are strong lines at 499 and 540, 568 and 579. The line at 540 forms with E (5268), and the iron line at 5327 (both solar lines), an equi-distant trio. The difference between the two groups may perhaps best be observed by a comparison of Aldebaran and Capella.

(5) The spectrum of this star is Class I.*a* (Group IV.). The relative intensities of the hydrogen and metallic lines should be noted, in order that the star may be arranged with others in order of temperature.

(6) Dunér gives the spectrum of this star as Class III.*b* (Group VI.), in which the main features are three dark carbon flutings fading away towards the blue. Other absorptions, if any, should be carefully observed, and their relative intensities recorded.

(7) This is a variable star, which reached its maximum on November 6. The magnitude at maximum is given by Gore as 6·9-8·3, and the period as 165·1 days. The spectrum has not yet, so far as I know, been recorded.

A. FOWLER.

GEOGRAPHICAL NOTES.

THE telegrams in the papers of Monday and Tuesday from Mr. Stanley are of the most suggestive and interesting character. For one thing, Emin, Casati, and others who have been holding out, are safe, though the brave Pasha has evidently been deserted by most of his men. That Mr. Stanley's expedition was needed the result has proved. He reached the Albert Nyanza for the third time, not a moment too soon to rescue the retreating party. We need not dwell on the sacrifices that have been entailed; they might to some extent have been avoided, but personally Mr. Stanley is not to blame. The geographical results of the expedition, as shadowed in the too brief telegram in Tuesday's papers, are evidently of the highest interest. There is now no doubt that there is a southern Albert Lake, Muta Nzigé, which Mr. Stanley has named Lake Albert Edward. From the time when he himself discovered what he called Beatrice Gulf until the present, no one had seen this lake. At first it was thought to be a part of the northern lake, Albert Nyanza, but that idea had to be given up. Now it is clear that it is connected with that lake by the River Sempliki. The southern lake is 900 feet higher than the northern, and so is about 3200 feet above sea-level, and 450 feet above Lake Tanganyika, with which it is unlikely to have any connection. Mr. Stanley skirted the snowy mountain range referred to in his letters of six months ago, and found that they send down fifty streams to feed the

[1] Roy. Soc. Proc., vol. xlv. pp. 380-392.

Sempliki. Awamba, Usongora, Toro, Ahaiyama, Unyampaka, and Anhori, are all districts around the west, north, and east shores of the Lake Albert Edward, three sides of which Mr. Stanley says he has traversed—probably the east, west, and north sides, though it is possible he may have gone round the south side. It is probable that the lake as laid down on our maps is much too large, and that it is comparatively small Mr. Stanley found it to be 15 miles wide at Beatrice Gulf. From the lake he struck south-east to Karagwe and Uzinze, on the south-west and south of Victoria Nyanza, and no doubt found at Mslala the stores which have been accumulating for many months. Thus it will be seen Mr. Stanley has solved one of the few remaining problems of African geography. He has found the south-west source of the Nile, and established the true relations which exist among the great lakes of Central Africa. He has filled up an important blank in our maps, and collected observations which will enable us to understand the physical geography of one of the most interesting regions on the continent. Probably he will be able to tell us what has become of the Alexandra Lake of his former expedition. It may be as well to state that the telegram of Monday was in effect the first part of that of Tuesday, and therefore Emin's safety was not again referred to in the latter.

THE Zanzibar Correspondent of the *Times* telegraphed on November 5 that authentic news had reached Lamu that Dr. Peters and the whole of his party had been massacred, except one European and one Somali, wounded, who are at Ngao. Some say they were killed by Masais, and some by Somalis.

FROM the Journal of the Anthropological Society in Vienna, we take the following conclusions of Dr. B. Hagen, respecting the Malay peoples:—Their great predilection for the sea, which makes them pray to Allah that they may die on sea, seems to render the Malay race adapted for the Polynesian and Further Indian Archipelago. The centre from which they migrated is to be sought in the highlands of West Sumatra, particularly in the old kingdom of Menang-Kabau. Thence the peoples extended slowly eastwards; at first probably the races now to be found only in the interior of the great islands (the Battas in Sumatra, the Sundanese in Java, the Dayaks in Borneo, the Alfurus in Celebes, &c.). These "aborigines" of the islands crushed out a population already in possession, as remains of which the Negritos may be taken. The Malays in the narrower sense occupying Sumatra, Malacca, and North Borneo, are to be regarded as the last emigration from the centre referred to, occurring from the twelfth to the fifteenth century A.D. With the Indians and Chinese, who have been long in intercourse with the archipelago, arose mixtures and crosses, in less measure also with the Arabs. One must not therefore expect the pure racial type, especially in the coast population. The crania of the anthropological collections are too imperfectly determined in respect of their *locale* to be of any service for a judgment of the Malay peoples. Of more value are the measurements of the living begun by Dr. Weisbach and executed by Dr. Hagen in 400 cases. The latter's conclusions are:—(1) The peoples in the interior of Sumatra—the Battas, the Allas, and the Malays of Menang-Kabau—compose a closely allied group always in direct contrast with the hither-Indian peoples, and yet showing just as little community with the Chinese. We must therefore take them for the pure original type, characterizable as follows:—Small, compact, vigorous figure of less than 1600 mm. average size; long arms; very short legs; very long and broad mesocephalous skull of very great compass, with high forehead; a prognathous face 10 per cent. broader than long, with large mouth, and uncommonly short, flat, and broad nose with large round nostrils opening mostly frontwise, and with broad nasal root. (2) The Malays of the east coast of Sumatra and those of the coasts of Malacca indicate a much greater affinity to the Indians than to their tribal peoples of Menang-Kabau. They are plainly therefore thoroughly mixed with Indian blood. (3) The Javanese peoples stand much nearer to the original type of the Sumatrans than to the Malays just mentioned. They show therefore less mixture with Indian, but on the other hand more mixture with Chinese, blood, and the Javanese more so than the Sundanese.

THE second number of this year's "Information respecting Kaiser Wilhelmsland and the Bismarck Archipelago," issued by the German New Guinea Company, contains a description of the north coast of New Guinea, from Cape Cretin to the Legoarant Islands, by the former Governor, Vice-Admiral Freiherr von Schleinitz, with a map designed by him. According to this account, Kaiser Wilhelmsland is subject to the south-east trade wind. This is, however, occasionally relieved by the opposite wind, when, viz., the sun in southing imparts to the Australian continent a temperature higher than that of New Guinea. The temperature, averaging 26° to 27° C., is not so high as might be inferred from the equatorial situation of the land, a fact due in part to the prevalence of the trade wind, which also brings with it a cooling sea-current to the coast, and in part to the considerable elevation of most of the island. The north-west, blowing especially from January to April, comes on the whole with greater force than the south-east. Calms often occur from March to May and from October to December. Precipitation is on the whole copious, but there are many differences according to the local variations in the configuration of the land. The navigation of the coast offers no particular dangers and difficulties, either for steamers or sailing-vessels. Serious storms are extremely rare, nor are there any reefs in the channel proper. Sea currents do not strike direct on the coast, and they are not generally very strong. The tides are inconsiderable, the spring floods keeping under 1 metre.

SOME interesting remains have been found in Hamburg on the site of the new Rathhaus. At a depth of 0 to 0·7 metre the ground was covered to a height of 10 to 15 centimetres with dams of thin willow twigs (*Salix fragilis*), in many places two, sometimes even three, layers above one another, and separated from one another by equally thick earth layers. The building rests on clay, *i.e.* submerged ground, which contained heaps of freshwater shells, *e.g.* *Valvata piscinalis*, *Bythinia tentaculata*, &c., as also *Cardium edule*, *Tellina baltica*, *Mactra solida*, &c. When therefore the dam was made, the water must have been strongly brackish. The interest in this discovery was heightened when there was found, under St. Anne's Bridge, at a depth of 0·5 metre, a regularly paved street of small boulders, such as were still used for stone pavement in all North German towns in the last century. The stone dam was about 5 metres broad, and encased on both sides by thick wooden planks, in order, in the swampy ground, to prevent the slipping out of the stones sideways. The ascertained changes in the level of the North Sea give no positive clue to the age of the Hamburg finds.

THE INSTITUTION OF ELECTRICAL ENGINEERS.

ON Monday evening the first annual dinner of the Institution of Electrical Engineers took place at the Criterion Restaurant, Sir William Thomson, the President, occupying the chair. Many different branches of science were represented on the occasion, and some of the after-dinner speeches rose to a high level of excellence.

Due honour having been done to the usual loyal toasts, and Major Webber and Captain Wharton having responded for the Army and Navy, the Chairman proposed "Her Majesty's Ministers" Lord Salisbury said, in response:—

Sir William Thomson and Gentlemen,—I have to thank you on behalf of my colleagues in the Goverment and myself for the exceedingly kind reception you have given to the kind words in which Sir William Thomson has proposed this toast. I do not feel that I can accept the guise in which he put my name forward. On the contrary, though recognizing, as every individual must do, and as I have especial reason to do, the enormous benefits which electrical science confers upon mankind, I feel that I have reason rather to apologize for my appearance in this assembly. When I look round on so many learned and distinguished men, I feel rather in the position of a profane person who has got inside the Eleusinian mysteries. But I have an excuse. The gallant gentlemen who replied for the Army and Navy were able to show many particulars in which their special professional vocation was sustained and pushed forward by the discoveries of electrical science. But I will venture to say that there is no department under the Government so profoundly indebted to the discoveries of those who have made this science as the Foreign Office, with which I have the honour to be connected. I may say that we positively exist by virtue of the electric telegraph. The whole

work of all the Chancelleries in Europe is now practically conducted by the light of that great science, which is not so old as the century in which we live. And there is a strange feeling that you have in communicating constantly and frequently day by day with men whose inmost thoughts you know by the telegraph, but whose faces you have never seen. It is something more than a mere departmental effect which these great discoveries have had upon the government of the world. I have often thought that if history were more philosophically written, instead of being divided according to the domination of particular dynasties or the supremacy of particular races, it would be cut off into the compartments indicated by the influence of particular discoveries upon the destinies of mankind. Speaking only of these modern times, you would have the epoch marked by the discovery of gunpowder, the epoch marked by the discovery of the printing-press, and you would have the epoch marked by the discovery of the steam-engine. And those discoveries have had an influence infinitely more powerful, not only upon the large collective destinies, but upon the daily life and experience of multitudes of human beings, than even the careers of the greatest conquerors or the devices of the greatest statesmen. In that list which our ignorance of ancient history in its essential character forbids us to make as long as no doubt it might be made, the last competitor for notice and not the least would be the science of electricity. I think the historian of the future when he looks back will recognize that there has been a larger influence upon the destinies of mankind exercised by this strange and fascinating discovery than even in the discovery of the steam-engine itself, because it is a discovery which operates so immediately upon the moral and intellectual nature and action of mankind. The electric telegraph has achieved this great and paradoxical result, that it has, as it were, assembled all mankind upon one great plane where they can see everything that is done, and hear everything that is said, and judge of every policy that is pursued at the very moment when those events take place; and you have by the action of the electric telegraph, combined together almost at one moment, and acting at one moment upon the agencies which govern mankind, the influences of the whole intelligent world with respect to everything that is passing at that time on the face of the globe. It is a phenomenon to which nothing in the history of our planet up to this time presents anything which is equal or similar, and it is an effect and operation of which the intensity and power increases year by year. When you ask what is the effect of the electric telegraph upon the condition of mankind, I would ask you to think of what is the most conspicuous feature in the politics of our time, the one which occupies the thoughts of every statesman, and which places the whole future of the whole civilized world in a condition of doubt and question. It is the existence of those gigantic armies held in leash by the various Governments of the world, whose tremendous power may be a guarantee for the happiness of mankind and the maintenance of civilization, but who, on the other hand, hold in their hands powers of destruction which are almost equal to the task of levelling civilization to the ground. What gives these armies their power? What enables them to exist? By what power is it that one single will can control these vast millions of men and direct their destructive energies at one moment on one point? What is the condition of simultaneous direction and action which alone gives to these vast armies this tremendous power? It is nothing less than the electric telegraph. And it is from that small discovery, worked out by a few distinguished men in their laboratories upon experiments of an apparently trivial character, on matter and instruments not, in the first instance, of a very recondite description—it is on that discovery that the huge belligerent power of modern States, which marks off our epoch of history from all that have gone before, must be held, by anyone who investigates into the causes of things, absolutely to depend. I would venture to hope that this is not all, in its great effect upon the history and government of our race, that electricity may achieve. Whether it so far is good or evil in the main, it must be for the future to determine. We only know that the effect, whatever it is, will be gigantic. But in the latter half of the short life of this young science another aspect of it has been developed—an aspect which I cannot help hoping may be connected with great benefits to the vast community of industrious and labouring men—I mean that facility for the distribution of power of which electricity has given such a splendid instance. The event of the last century was the discovery of the steam-engine. But the steam-engine was such that the forces which it produced could only act in its own immediate neighbourhood, and therefore those who were to utilize its forces and translate them into practical work were compelled to gather round the steam-engine in vast factories, in great manufacturing towns, and in great establishments where men were collected together in unnatural, and often unwholesome, aggregation. Now an agent has been discovered, by which the forces of the steam-engine, stiff, confined to its own centre, can be carried along, far away from its original sources, to distances which are already great, and which science promises to make more considerable still. I do not despair of the result that this distribution of forces may scatter those aggregations of humanity, which I think it is not one of the highest merits of the discovery of the steam-engine to have produced. If it ever does happen that in the house of the artisan you can turn on power as now you can turn on gas—and there is nothing in the essence of the problem, nothing in the facts of the science, as we know them, that should prevent such a consummation from taking place—if ever that distribution of power should be so organized, you will then see men and women able to pursue in their own homes many of the industries which now require the aggregation at the factory. You may, above all, see women and children pursue these industries without that disruption of families which is one of the most unhappy results of the present requirements of industry. And if ever that result should come from the discoveries of Oersted and Faraday, you may say that they have done more than merely to add to the physical forces of mankind. They will have done much to sustain that unity, that integrity of the family, upon which rest the moral hopes of our race and the strength of the community to which we belong. These are some of the thoughts which electricity suggests to one of my trade. Pardon me if I have wandered into what may seem to be speculative and unfamiliar fields. But, after all, the point of view from which we must admire the splendid additions to our knowledge which the scientific men of the world, and especially of England, during this century have made, is, that they have enabled mankind to be more happy, to be more contented, and therefore to be more moral.

Sir Frederick Abel proposed, and Sir George Gabriel Stokes responded for, "The Learned Societies"; and Sir John Coode responded for the toast of "The Professional Societies," which was proposed by Mr. Latimer Clark. The toast of "The Institution of Electrical Engineers" was then proposed by Lord Salisbury. In the course of his response, Sir William Thomson said:—

One very remarkable piece of work they should think of especially this year, and during the last few weeks, when they deplored the loss of one of the greatest workers in electrical science and its practical application that the world had ever seen—Joule. The great scientific discoveries of Faraday, which were prepared almost deliberately for the purpose of allowing others to turn them to account for the good of man, had been going on for about fifteen years, when a young man took up the subject with a profound and penetrating genius most rare in any branch of human study, and perceived relations with mechanical power which had never been suspected before. Joule saw the relations between electricity and force, and his very first determination of the mechanical equivalent was an electrical measurement. His communication to the British Association, when it met in Cork in the year 1841, pointed out for the first time the distinct mechanical relation between electric phenomena and mechanical force. Joule was not a mere visionary who saw and admired something in the air, but he pursued what he saw to the very utmost practical point of work, and he it was who determined the mechanical equivalent of heat. Afterwards he thoroughly confirmed the principle of his first determination of the mechanical equivalent of heat. Both in electricity and mechanical action he laid the foundation of the great development of thermodynamics, which would be looked upon in future generations as the crowning scientific work of the present century. It was not all due to Joule, but he had achieved one of the very greatest monuments of scientific work in the present century. For an Institution of Electrical Engineers it was interesting to think that the error relating to one of the most important electrical elements, the unit of resistance (now called the ohm), as determined electrically in the first place by a Committee of the British Association, and by purely electrical method, was first discovered by Joule's mechanical measurement. It was Joule's mechanical measurement which first corrected the British Association unit, and gave the true ohm.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—The following examiners have been appointed: Natural Sciences Tripos: Physics, Prof. Carey Foster and W. N. Shaw; Chemistry, Prof. W. A. Tilden and Prof. Liveing; Mineralogy, Prof. Lewis and L. Fletcher; Geology, Prof. Green and W. W. Watts; Botany, F. Darwin and D. H. Scott; Zoology, Prof. Lankester and S. F. Harmer; Human Anatomy, Drs. Hill and Windle; Physiology, Prof. Stirling and C. S. Sherrington.

First M.B. and Special B.A.: in Elementary Physics, S. L. Hart and H. F. Newall; Elementary Chemistry, F. H. Neville and S. Ruhemann; Elementary Biology, S. F. Harmer and Prof. H. M. Ward; Special B.A. in Geology, Prof. Green and W. W. Watts; in Pharmaceutical Chemistry for Second M.B., M. M. Pattison Muir and H. Robinson.

The following are Moderators (Mathematical Tripos) for the year beginning May 1, 1890:—W. W. R. Ball and A. J. Wallis. Examiners in Part I., W. L. Mollison and E. G. Gallop; in Part II., Prof. Darwin, J. Larmor, and R. Lachlan.

W. B. Hardy, of Gonville and Caius College, has been appointed Junior Demonstrator of Physiology.

L. R. Wilberforce, M.A., of Trinity College, is approved as a Teacher of Physics for M.B. lectures.

There has been a serious discussion of the financial management and prospects of the mechanical workshops at Cambridge. Whatever be the merits of the points in dispute, such division of opinion and feeling is very unfortunate, and much to be deplored in the interests of mechanical science and engineering in the University. It was unfortunate that the University declined to establish an advanced examination or Tripos in engineering subjects; and it is calamitous that the Museums work should not be given to the Department located within their own borders. We trust a cordial understanding may soon be re-established; for this division is very unlike the strong action by which, even when opinions have been divided, scientific teaching has steadily progressed of late years at Cambridge.

The managers of the John Lucas Walker Fund, have made the following grants in aid of original research in pathology:—£14 2*s*. 3*d*. to J. G. Adami, Demonstrator of Pathology, for expenses of his investigations on the pathology of the heart; £35 to William Hunter, M.D. Edin., John Lucas Walker Student, to defray expenses incurred in his research on the pathology of the blood; £30 to E. Hanbury Hankin, to defray expenses of his research on the nature of immunity from infectious diseases.

Mr. J. W. Clark has been re-elected President of the Philosophical Society.

ST. JOHN'S COLLEGE.—At the annual election of Fellows, on Nov. 4, the choice of the Council fell upon the following members of the College: John Parker, Seventh Wrangler, 1882, well known as the author of numerous papers, in the *Philosophical Magazine* and elsewhere, on thermodynamics and electricity; Humphry Davy Rolleston, First Class Natural Sciences Tripos (Human Anatomy and Physiology), 1886, who has been University Demonstrator in Pathology, in Human Anatomy, and in Physiology, author of memoirs on endocardiac pressure and on other anatomical, physiological, and pharmacological subjects, now one of the Assistant Demonstrators of Anatomy at St. Bartholomew's Hospital; Alfred William Flux, bracketed Senior Wrangler, 1887, and First Class (Division 1) Mathematical Tripos, Part II., 1888, Marshall Prrizeman in Political Economy, 1889, author of papers on physical optics. Mr. Rolleston is the son of the late Prof. Rolleston, of Oxford. The success of students of physical and biological science at this College is striking.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, October 28.—M. Des Cloizeaux, President, in the chair.—M. Bertrand presented a volume entitled "Lectures on the Mathematical Theory of Electricity, delivered at the College of France."—On some hybrids observed recently in Provence, by M. G. De Saporta. Three are described: (1) between *Pinus halepensis*, Mill., and *P. pinaster*, L.; (2) between *Quercus Mirbeckii* and *Q. pubescens*, Wild.; (3) between *Tilia platyphylla*, Scop., and *T. argentea*, Desf.; in each case, the pollen of a preponderating species acting on that of a subordinate one, or one accidentally introduced, being carried by wind or insects, while the agency of man, birds, or wind, disseminated the hybrid seeds.—On the relation of certain magnetic perturbations to earthquakes, by M. Mascart. The former, in the Park of St. Maur, and the latter, at Gallipoli, seem to have occurred simultaneously at 11.35 p.m. on October 25. The suspended copper bar was not in the least deflected, and the magnetic disturbance cannot be attributed to mechanical transmission of the shock.—On certain harmonic linear elements, by M. Raffy.—On a formula connecting vapour-pressure with temperature, by M. N. de Saloff.—On the equilibrium of distribution between chorine and oxygen, by M. H. Le Chatelier. He shows that the value of all the coefficients may be calculated *a priori*, and supplies the required formulæ.—On some double nitrites of ruthenium and potassium, by MM. A. Joly and M. Vèzes. In contact with alkaline nitrites, the brown sesquichloride of ruthenium is transformed into a red salt. According to the temperature, and according as the nitrite or the red chloride predominate, a deposit is formed either of yellow crystalline powder, sparingly soluble in cold water, or of large, very soluble orange-red crystals. These two substances are double nitrites of potassium and ruthenium. The formulæ obtained do not at all agree with those for similar compounds obtained by Claus.—Fixation of nitrogen by the Leguminosæ, by M. Bréal. Having before found that nodosities full of Bacteria could be easily produced on the roots of a leguminous plant, by pricking with a needle previously inserted in a nodosity, he here shows that such plants, with nodosities, flourish on soil poor in azotized matter; yielding crops rich in nitrogen, and fixing this element in the soil by their roots.—On air in the soil, by M. Th. Schlœsing, *fils*. Ploughed land was found to contain a relatively large amount of oxygen at least to the depth of 50 or 60 cm. The carbonic acid generally increased with the depth; but in two cases the reverse occurred, when high wind (renovating the upper layer) had been followed by hot and calm weather, and more CO_2 was generated in the soil than in the sub-soil. In sloping pastures, most CO_2 was found at the bottom. The mobility of air in the soil should be taken into account.—On sorbite, by MM Vincent and Delachanal. This substance very frequently occurs in nature; it is found in all fruits of Rosaceæ, and is especially abundant in pears (8 grammes per kilogramme), cherries and prunes (7 grammes). Acted on by hydriodic acid it yields β-hexylene and other products (the same as are thus obtained from mannite). The formation of a hexacetyl derivative from sorbite proves that it is a hexatomic alcohol. The formula of anhydrous sorbite is $C_6H_8(OH)_6$.—Researches on crystallized digitaline, by M. Arnaud. He regards it as a definite chemical species; and it appears to be the type of a whole series, including tanghinine (one of the active principles of the tanguin.—Experimental researches on the metamorphosis of Anoura, by M. E. Bataillon. He finds acceleration of the rhythm of respiration (65 to 120), and retardation of that of the heart (70 to 45) during metamorphosis. Before appearance of the fore-legs, the two movements were nearly synchronous. At the stage of this appearance, further, the production of carbonic acid was found to have diminished considerably, and the curve rose suddenly when aërial respiration was established.—On the earthquake of July 28, 1889, in the island of Kiushiu, in Japan, by M. J. Wada. This was preceded by exceptional rains during July. The longer axis of the ellipse of land affected was north-east to south-west, and cut in the middle, at right angles, the line joining two volcanoes, 100 kilometres apart.

BERLIN.

Physiological Society, October 18.—Prof. du Bois-Reymond, President, in the chair.—Prof. Kossel spoke on the application of the microscope in connection with physiological chemistry. It has long been the practice to seek for and identify any minute crystals in tissues which occur either naturally or as the result of treatment with reagents, in order to arrive at a qualitative determination of the localized distribution of certain well-known substances in the organism. To identify a crystal by measurement of its angles is a laborious process, and to determine it by mere comparison of its appearance with drawings of known crystals is insufficient. The optical properties of crystals are extremely well adapted to assist in their identification; this is exemplified in the case of determining the plane of vibration of the ordinary and extraordinary rays when crystals are examined between crossed Nicols. To carry out the determination by this means, the field of view of the microscope is provided with cross-wires,

whose directions are parallel to the principal planes of the two Nicols. The crystal under examination is then placed with one edge under one of the cross-wires; if the field of vision remains dark, then the planes of vibration in the crystal are known to correspond to the chief planes of the two Nicols. If, however, the field of vision becomes bright the crystal must be rotated, by means of a graduated object-carrier until it is again dark. The angle through which the carrier has been rotated is a measure of the angular inclination of the planes of vibration to the edges of the crystal. When convergent polarized light is used, the majority of crystals of organic substances, which are mostly biaxial, exhibit a lemniscate whose poles are at varying distances apart for various crystals. The distance between the poles of the lemniscate may be measured by suitable methods, is extremely characteristic for those crystals of greatest physiological importance, and may be used, in conjunction with the measurement of the planes of vibration, as a very certain means of determining the crystal. The pleochromatism of many crystals is itself in many cases sufficiently characteristic.—Dr. Virchow described the distribution of blood-vessels in the eye of Selachians, and the several types according to which the vessels are developed in the eyes of various classes of animals.—Dr. Benda made a communication to the effect that the coiled glands which are so widely distributed as sweat-glands in the skin when they exhibit an enlarged secretory part, and a more complicated structure, are known as cerumenous and as mammary glands. They are characterized specially by the fact that during secretion there is no destruction of their epithelium. These modifications of the typical coiled glands have been found by Dr. Benda in large numbers and widely spread in the skin of Protopterus.—Dr. Schneider spoke on the distribution and significance of iron in the animal organism. He was able to find iron in greater or less quantity in the cell protoplasm and nucleus of all classes of animals, the liver and spleen being the organs in which its occurrence was most marked. The connective tissues were very rich in iron, and it was found with similar constancy in the cuticular layers and quite constantly in the extreme tips of fishes' teeth. The more he extended his investigations over the most widely differing classes of animals, whether on land, or in fresh-water, or in the sea, and the more widely different were the organs he examined, by so much the more was it seen that iron is universally pre ent in the animal organism. Its importance is pre-eminently physiological.

AMSTERDAM.

Roy l Academy of Sciences, September 28.—Prof. van der Staals in the chair.—M. Suringar dealt with the Melocacti of Aruba, stating what he had himself observed concerning the development of those plants from seed and their subsequent growth. He spoke also of the manner in which the Melocacti might be classified according to their natural affinities, and sketched a pedigree of the species.—M. Schoute spoke of tetrahedra, bounded by similar triangles, and described a new species with pairs of opposite edges 1 and r^3, r and r, r^2 and r^2.

STOCKHOLM.

Royal Academy of Sciences, October 9.—Musci Asiæ Borealis (second part): feather mosses, by the late Prof. S. O. Lindberg, of Helsingfors, and Dr. H. W. Arnell.—On the permanent committee for a photographic map of the heavens and its work, by one of its members, Prof. Dunér.—On the Metre Congress in Paris, September 14–28, this year, and on the prototypes of the metre and the kilogramme, by Prof. Thalén.—Emanuel Swedenborg as a mathematician, by Dr. G. Eneström.—On naphtoë acids, by Dr. A. G. Ekstrand.—Chemical investigation of some minerals from the neighbourhood of Langesund, by Herr H. Bäckström.—An attempt to determine the velocity of light from observations on variable stars, by Dr. C. Charlier.

DIARY OF SOCIETIES.

LONDON.

THURSDAY, NOVEMBER 7.

LINNEAN SOCIETY, at 8.—On a Collection of Dried Plants chiefly from the Southern Shan States, Upper Burma: Colonel H. Collett and W. Botting Hemsley, F.R.S.

CHEMICAL SOCIETY, at 8.—The Isolation of a New Hydrate of Sulphuric Acid existing in Solution: S. U. Pickering.—Further Observations on the Magnetic Rotation of Nitric Acid, of Hydrogen Chloride, Bromide and Iodide in Solution: Dr. W. H. Perkin, F.R.S.—On Phosphoryl Trifluoride: T. E. Thorpe, F.R.S., and F. T. Hambly.—On the Acetylation of Cellulose: C. F. Cross and E. Bevan.—On the Action of Light on Moist Oxygen: A. Richardson.—Anhydracetophenonebenzil and the Constitution of Linius lepideus: Drs. Japp, F.R.S., and Klingsman.

FRIDAY, NOVEMBER 8.

ROYAL ASTRONOMICAL SOCIETY, at 8.

MONDAY, NOVEMBER 11.

ROYAL GEOGRAPHICAL SOCIETY, at 8.30.—Cyprus: Lieut.-General Sir Robert Biddulph, G.C.M.G.

TUESDAY, NOVEMBER 12.

ANTHROPOLOGICAL INSTITUTE, at 8.30.—Observations on the Natural Colour of the Skin in certain Oriental Races: Dr. J. Beddoe, F.R.S.—Manners, Customs, Superstitions, and Religions of South African Tribes: Rev. James Macdonald.

INSTITUTION OF CIVIL ENGINEERS, at 8.—Inaugural Address of Sir John Coode, K.C.M.G., President, and Presentation of Medals, Premiums, and Prizes awarded during Last Session.

WEDNESDAY, NOVEMBER 13.

ROYAL MICROSCOPICAL SOCIETY, at 8.

THURSDAY, NOVEMBER 14.

MATHEMATICAL SOCIETY, at 8.—Isoscelian Hexagrams: R. Tucker.—On Euler's ϕ-Function: H. F. Baker.

FRIDAY, NOVEMBER 15.

PHYSICAL SOCIETY, at 5.—On the Electrification due to the Contact of Gases and Liquids: J. Enright.—On the Effect of Repeated Heating and Cooling on the Electrical Resistance and Temperature Coefficient of Annealed Iron: H. Tomlinson, F.R.S.—Notes on Geometrical Optics, Part II.: Prof. S. P. Thompson.

INSTITUTION OF CIVIL ENGINEERS, at 7.30.—The New Harbour and Breakwater at Boulogne-sur-Mer: S. C. Bailey.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

A Popular Treatise on the Winds: W. Ferrel (Macmillan).—South African Butterflies; vol. iii., Papilionidæ and Hesperidæ: R. Trimen and J. H. Bowker (Trübner).—Light, 2nd edition: P. G. Tait (Edinburgh, Black).—The Vertebrate Animals of Leicestershire and Rutland: M. Browne (Birmingham, M. E. C.).—Sitzungsberichte der k. b. Gesellschaft der Wissenschaften Math.-Naturw. Classe, 1889, i. (Prag).—Outlines of a Course of Lectures on Human Physiology: E. A. Parkyn (Allman).—Flower-Land: R. Fisher (Bemrose).—Potential and its Application to the Explanation of Electrical Phenomena: R. Tumlirz, translated by D. Robertson (Rivingtons).—Index Catalogue of the Library of the Surgeon-General's Office, United States Army, vol. x. (Washington).—The Birds of Berwickshire, vol. i.: G. Muirhead (Edinburgh, Douglas).—Idylls of the Field: F. A. Knight (E. Stock).—Atti della Reale Accademia delle Scienze Fisiche e Matematische, serie seconda, vol. iii. (Napoli).—Ferneries and Aquaria: G. Eggett (Dean).—Traité Encyclopédique de Photographie, 15 Octr. (Paris).

CONTENTS.

THURSDAY, NOVEMBER 14, 1889.

SCIENCE AND THE FUTURE INDIAN CIVIL SERVICE EXAMINATIONS.

THE following memorial, signed by a numerous and highly-distinguished body of resident graduates of the University of Cambridge, has been presented to the Civil Service Commissioners :—

"We, the undersigned resident graduates of the University of Cambridge interested in the study of natural science, understanding that a reorganization of the open competitive examination for the Civil Service of India is under the consideration of the Civil Service Commissioners, beg respectfully to urge on the Commissioners the desirability of widening the range of the examination so as to include the several branches of natural science. We think it especially important that the maximum number of marks obtainable by a candidate in natural science in the examination should be the same as that obtainable by a candidate in classics or in mathematics. In support of this opinion we venture to point out that the Natural Sciences Tripos, both from its numbers and from the rewards assigned by the Colleges to those of their members who distinguish themselves therein, is now of equal importance with the Classical or Mathematical Tripos.

"We have the honour to append a statement of the numbers who have during the last five years taken honours in natural science, classics, and mathematics. We inclose a copy of the *Cambridge University Reporter* of June 12, 1888, containing a report to the Senate and a schedule of the numbers examined in each branch of natural science in the years 1883–87.

"We would desire to call attention to the acknowledged educational value of the study of natural science, and to point out that the training which it affords, combining as it does both theory and practice, is such as peculiarly to fit a student for the pursuits of practical life.

"We beg to state that a deputation would be happy to wait on the Commissioners to explain more fully our views on the subject should it be their pleasure to receive them."

This memorial is signed, among others, by two Heads of Houses, thirteen Professors, and twenty Fellows. The memorialists, as will be seen, urge that in future competitions the position of a candidate offering natural science shall be not less favourable than that of those who offer classics or mathematics. And in a highly instructive schedule they show how important a place the study of the natural sciences has now attained in the University of Cambridge.

It may be unknown to many of our readers that the subject to which this memorial relates has lately become one of great importance, in consequence of a proposed reorganization of the higher branches of the public services in India. A Commission, which we believe sat in India, known as the Public Service Commission, has advised that the following changes should be made with the object of admitting natives of India to higher and more extensive employment in the public services :—

(1) That the strength of the Covenanted Civil Service should be reduced to what is necessary to fill the chief administrative appointments of the Government, and such a proportion of smaller appointments as will secure a complete course of training for junior Civilians. This branch of the service to continue to be recruited by means of open competitions in England, at which natives of India should be allowed to compete unreservedly, and for which the maximum age of the Native candidates, and therefore presumably of the English candidates, should be raised to twenty-three years.

(2) That a certain number of appointments should be transferred from the Covenanted Civil Service to a local Civil Service, which is to be recruited, locally, from Natives and resident Europeans who satisfy certain prescribed preliminary conditions.

We do not know how far these proposals have been adopted by the home authorities, though we understand that they have received the general approval of the Indian Government. We will therefore only say, in passing, that they appear to be open to two serious objections.

First, that it seems a dangerous thing to select so limited a number of young men for the higher branch of the service by open competition, since doing so will give to each one of those who succeed almost the certainty of the reversion of one of the prizes of the public services. Under such a condition there will be far too little inducement for zeal in the service, and too little opportunity for selection and rejection when age and experience have developed the administrative powers of the selected men.

Secondly, unless care be taken to regulate the previous training of the candidates, as, for example, by requiring that every candidate shall have taken a University degree in England or India before presenting himself at the competitive examination, it is likely that well-taught rather than well-educated men will be selected, and that an inferior order of men will offer themselves, since many of the ablest men would be unable to submit to some years of private tuition, and to give up, as they would probably have to do, a University education for the chance of obtaining an appointment in India.

Whatever decision may have been made, however, it is of the utmost importance that the representatives of Cambridge who have addressed themselves to the Civil Service Commissioners should be supported in every possible way, and at once, by all those who have the interest of science and education at heart. For there is reason to fear that the Commissioners have contemplated the complete withdrawal of science from these examinations; and unfortunately many of the various regulations for the Army examinations which have been brought forward with their sanction in recent years give an air of probability to this suggestion. This is in no way weakened when we consider the extremely unfortunate position that science candidates for the Indian Civil Service have occupied under the administration of the Commissioners for many years past. This position, it should be said, has been due, not so much to the marks allotted to science in the present scheme, as to the methods adopted by the Commissioners in conducting their examinations, which have long caused it to be recognized by those who are engaged in the instruction of Civil Service candidates that, as a rule, only those candidates who are excellent either in classics or mathematics, or those who are distinctly good in both, have a really good chance of success.

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But though all these facts give reason for regarding the rumour we refer to as very possibly correct, they need by no means prevent those who are interested in the question from entertaining strong hopes of averting such a national disaster as that which we fear. We have only to remind them of the very considerable degree of success that followed the efforts recently made by Sir Henry Roscoe and other leaders in science in the case of the examinations for admission to the Royal Military Academy at Woolwich. These efforts, we may remind our readers, not only resulted in an advantageous revision of the Woolwich examinations, but brought about satisfactory changes in the case of the Sandhurst competitions. In connection with this result it is satisfactory to observe, in the Report of the Civil Service Commission for 1888, that the Commission, in a letter directed to the Director-General of Military Education on July 10 in that year, have described the changes that had been submitted to them as likely to influence beneficially the education of officers in the army before they begin their professional studies.

Whatever difficulties there may be in the way of obtaining just treatment for science candidates under the new scheme for the selection of Indian civil servants, it has, we fear, become again imperative that men of science should unite to protest against the assumption that natural science studies are in themselves inferior as a mental training to the classical languages and mathematics, and to insist, so far as they may, upon such studies being placed upon a proper footing in this particular examination. This should be done in the interests of education, and still more of our Indian fellow-subjects, whose administrators should be men of as wide and liberal an education as possible, as has, indeed, been recognized in more than one public investigation of the regulations for these appointments.

THE LUND MUSEUM IN THE UNIVERSITY OF COPENHAGEN.

E Museo Lundii: En Samling af Afhandlinger om de i det indre Brasiliens Kalkstenshuler af Professor P. V. Lund udgravede Dyre-og Menneskeknogler. Udgivet af Dr. Lütken. (Kjöbenhavn: H. Hagerup, 1888.)

THIS work, as its title indicates, consists of various monographs, descriptive of the collections made by Dr. Lund in his interesting exploration of the limestone caverns in the interior of Brazil. These important finds are the fruits of nearly ten years' unremitting labour in the neighbourhood of Lagoa Santa, on the Rio das Velhas, in the province of Minas Geraes, where Dr. Lund prosecuted his researches from 1835 to 1844. On the completion of his cave explorations he presented the whole of his incomparable collections to the Danish nation. The gift has been duly appreciated, and now constitutes, under the name of the "Lund Museum," one of the most important palæontological sections of the Zoological Museum in the University of Copenhagen.

Dr. Lund inspected as many as 800 of the Brazilian *lapas*, or bone-caves, of which he had discovered 1000. Of these only sixty yielded any very interesting results, while scarcely half that number contained a sufficient quantity of bones to demand any very prolonged investigation. In some instances, on the other hand, the mass of broken bones was so enormous that from the earth collected in a packing-case whose dimensions did not exceed half a cubic foot, he extracted 400 half jaw-bones of a marsupial and 2000 belonging to different rodents, besides the remains of innumerable bats and small birds. This discovery led to further research, and, after fifteen weeks' continued exploration, he found that one cave, which he had at first estimated to be about 25 feet deep, had a depth of nearly 70 feet, and was so densely packed with bones that the yield of 6500 barrels, of the size of an ordinary butter-firkin, justified the assumption that this special *lapa* contained the remains of seven and a half millions of animals, belonging for the most part to Cavia, Hystrix, and small rodents and marsupials, the estimate being based on the numbers of half jaw-bones extracted from the mould.

In these enormous cave deposits we have, according to Dr. Lund, and his biographer Dr. Reinhardt, a prehistoric ornithological *kökken mödding*, birds of prey having resorted to the *lapas* of Brazil as suitable retreats in which to devour their innumerable victims, whose fractured bones, belonging in almost equal proportions to extinct and living animals, have revealed to us many long-hidden secrets in the history of the changes which the Brazilian fauna has experienced in the course of ages. Comparatively few remains of the larger living mammals have been found, three caves only having yielded evidence of the presence of bears, of which, moreover, the bones of only five individuals were recovered. But while various groups, as *e.g.* the Ungulata, were sparsely represented, several families among the Edentata have contributed so largely to the bone remains of the Brazilian *lapas* that this order would appear to have constituted the most important section of the local fauna, both in past and recent times. Among the cave armadillos, Lund recognized several forms, differing only by their larger size from *Dasypus punctatus*, and *D. sulcatus*; but besides these he found one of colossal dimensions, which, with a body of the size of an ox, and a tail 5 feet in length, exhibited differences of dentition which induced him to assign it to a special genus, to which he gave the name Chlamydotherium. A peculiar characteristic of this fossil animal, whose food he believes was leaves, and not insects, was the fusion or overlapping of several of the vertebræ into nodes, or tangles. In this respect it resembles the still more remarkable armadillo, of whose scales and bones he found enormous quantities, and which he described under the name of Hoplophorus. This animal, of which the different species varied from the size of a hog to that of a rhinoceros, was described about the same time by Prof. Owen, to whom various specimens of its bones had been sent from La Plata, and who established a new species for its reception, to which he gave the name of Glyptodon. The extraordinary rigidity of the shields of some of the Brazilian armadillos, the apparent immobility of the head, and the interlocking of the vertebral bones, make it difficult to understand how these unwieldy animals could have obtained their food. The most probable solution of the problem seems to be supplied by a study of the short massive hind legs, which, with their sharp and powerful claws,

may have served to grub up roots and tubers, and tear off the branches of trailing plants. There is no evidence that our living tardigrades had appeared among the cave fauna of Brazil, where their place was supplied by gigantic gravigrades, resembling the Megatherium.

The results yielded by a careful study of the enormous and varied materials obtained by Dr. Lund in his explorations would appear, generally, to indicate that in post-Pliocene ages the Mammalian fauna of Brazil was richer than in recent times, entire families and sub-orders having become extinct in the intervening ages, or at all events greatly reduced as to the numbers of their genera and species. This is more especially the case in regard to the Edentata, Ungulata, Pachydermata, and Carnivora, which still continue to be characteristic representatives of the South American fauna. In two cases only there is evidence that species which are now exclusively limited to the Old World once inhabited the American continent. A far more marked difference between extinct and living animals is to be observed in the western than in the eastern hemisphere. Thus while the existing Brazilian fauna comprises very few large animals, the predominant forms being almost dwarf-like when compared with their Eastern analogues, the post-Pliocene Brazilian Mastodons, Macrauchenians, Toxodons, and gigantic armadillos and tardigrades, may rank in size with the elephant, rhinoceros, and hippopotamus, which were their contemporaries in Europe at that period of the world's history.

There is no ground for assuming that the change in the South American fauna was due to any natural cataclysm, and it would rather seem to be the result of some regular and slow geological changes, which, by affecting the then existing climatic relations, may have disturbed the conditions of animal life, and thus brought about the destruction, or deterioration, of the larger mammals, which, according to Owen, succumb where the smaller ones adapt themselves to altered conditions.

It was not till near the close of his explorations that Dr. Lund succeeded in finding human bones in such association with fossil remains as to justify the conclusion that man had been the contemporary in Brazil of animals long since extinct in South America. Only seven of the 800 *lapas* examined by him contained any human bones, and in several instances these were either not associated directly with fossil bones, or there were grounds for suspecting that they might have been carried into the caves in comparatively recent ages with the streams that traverse them. In one of these, however, the Sumidouro Lapa, remains of as many as thirty individuals of all ages were found so intermingled with the bones of the gigantic cave jaguar, *Felis protopanther*, and the monster Cavia, *Hydrochœrus sulcidens*, together with several extinct ungulates, that whatever may have been the reason of their presence, there seems to be no ground for doubting that primæval man was contemporaneous with these animals.

The crania, of which admirably drawn illustrations are given, are of a dolichocephalic type, characterized by strongly-marked prognathism, and remarkable for the excessive thickness of the cranial walls. The first communication by Lund of his discovery of human remains in the Lapa di Lagoa do Sumidouro was made (in 1840) in a letter addressed to Prof. Rafn, in which his fear of being accused of recklessness in attaching too high an antiquity to man in Brazil is shown by the pains he takes to indicate every possible means by which these bones might have found their way into the cave. Thus it remained for his annotator, the late Dr. Reinhardt, whose descriptive history of the caves and their exploration has added largely to the interest of the volume before us, to be the first to accept without reservation the co-existence of man with extinct animals which, according to Lund himself, occupied parts of South America more than 5000 years ago.

The monograph treating of the human remains found by Lund is from the pen of Dr. Lütken, the editor of the present work, who also supplies a *résumé* in French of the treatises contributed by his colleagues, Drs. O. Winge and H. Winge, the former of whom writes on the birds of the Brazilian *lapas*, and the latter on the living and extinct rodents of the Minas Geraes district. Besides these important contributions to the work, the reader is indebted to the late Dr. Reinhardt for a detailed description of the situation and geological character of the Brazilian bone-caves, and for an interesting biographical notice of Dr. Lund.

We learn from the preface that this collection of monographs owes, if not its publication, at any rate the complete and elegant form in which it has been produced, to the liberality of the directors of the Carlsberg Trust, at whose cost, with the sanction of the Danish Royal Society, it now forms one of those *éditions de luxe* which have of late years so largely enriched the scientific literature of Denmark. The objection that may be advanced against this, as well as others of the series, is that the writers appear to be moved by an uncalled-for impulse to write down to the level of the general reader, and to explain the origin and progress of each special branch of natural history they are concerned with. Such efforts to popularize the subject lead only to an inconvenient addition to the bulk of the volumes, and are wholly at variance with the scientific aim and object of such publications.

HYDRAULIC MOTORS.

Hydraulic Motors: Turbines and Pressure Engines. By G. R. Bodmer, A.M.I.C.E. "The Specialist's Series." (London: Whittaker and Co., 1889.)

THE essential detail which lifts the mere water-wheel to the rank of a turbine consists, according to the author, in some arrangement for directing the water over the buckets in the most advantageous manner, instead of allowing the water merely to follow its own course. Again, in a water-wheel only a small part of the wheel is really at work at a time, the buckets of the remaining part being empty; while a turbine is arranged, as a rule, with a vertical axis, and all parts of the wheel are simultaneously taking their fair share of the work. In this respect there is a great resemblance and analogy to the distinction between the two chief instruments of ship propulsion by steam—the paddle-wheel and the screw propeller. In the paddle-wheel only a few of the floats act on the water at a time; while in the screw propeller, completely submerged, all parts are equally at work, implying a great saving of weight in the propelling instrument. Mr.

Thornycroft, with his turbine propeller, is able to emphasize this economy of weight still further, and, but for difficulties of going astern not yet surmounted, would be able to save considerable weight and space in sea-going steamers with this contrivance.

As regards their construction, turbines are divided into three classes (p. 24)—the radial, axial, and mixed-flow—according to the mode in which the water enters and passes through the turbine; but as regards the dynamical principle on which the turbines work, they are divided into two classes (p. 25), the *reaction* and the *impulse* turbine.

In the reaction or Jonval turbine, described in chapters iii. to vi., the passages are completely filled with water, and the changes of pressure play an important part in the work performed. This turbine possesses the advantage of being able to work when drowned by the tail race, or when elevated above the tail water to a height anything less than the height of the water barometer, a suction tube of properly adjusted shape being fitted below the turbine to carry off the water at pressure gradually increasing downwards to the atmospheric pressure. Against this are the disadvantages of imperfect regulation for varying load, and that with a high fall this turbine must be made so small and must run so fast as rapidly to wear out, as in the Fourneyron turbines at St. Blaise (p. 422); but this disadvantage the author professes (p. 263) to avoid by compounding the turbine, just as we compound the steam-engine with high-pressure steam.

The impulse or Girard turbine, on the other hand (chapters vii. and viii.), derives its power entirely from the change of momentum of the water without change of pressure; the buckets are freely ventilated, and consequently this turbine can only work in communication with the surrounding air. It possesses, too, the great advantage of complete regulation of power by merely altering the supply of water. Girard turbines are divided into outward flow (Fourneyron) turbines, and inward flow (James Thomson); the latter, although more weighty and costly, possessing the advantage of greater stability of motion.

In their difference of action we may compare the Jonval turbine with the screw propeller, which works entirely immersed, and derives its reaction partly from the change of pressure in the water; while the Girard turbine resembles the paddle-wheel in working at the surface of separation of the water and air, so that no appreciable change of pressure is manifest. Against this analogy, however, we find the screw propeller far less susceptible to changes of immersion than the paddle-wheel, whence the manifest superiority of the screw for long voyages.

In chapters ix. to xi. the author gives a very valuable collection of numerical applications of his theories to actual turbines on a large scale. In designing a turbine to utilize a fall, the first important measurement is that of the quantity of the stream of water; the speed of the turbine is next determined from the consideration that the best theoretical speed is half (or a little more than half) the speed at which the turbine would run if unloaded; and then various practical considerations intervene in deciding whether the turbine should be reaction or impulse, outward, inward, or mixed flow.

At Holyoke, Mass., the Water-Power Company, under Mr. James B. Francis, controlling the falls of the Connecticut, undertake the commercial testing of turbines submitted to them, and have checked to some extent the wild claims of efficiency, reaching and even exceeding 100 per cent., which American turbine makers are said to have claimed in their advertisements. There is still, however, an efficiency claimed for American turbines which has not been rivalled in Europe: this cannot be attributed to defect in our designs, and the author thinks must be attributed to the less care bestowed in America on the measurement of the quantity of water consumed. It is noticeable that the American turbines are generally of the reaction Jonval type, which is more suitable for their unlimited supplies of water by reason of its smaller weight and cost; here in Europe, where water is scarcer, the impulse Girard turbine is more in favour.

For mining purposes, especially in California, with great falls of 400 or 500 feet and small quantities of water, the hurdy-gurdy or Pelton wheel (p. 419) is a favourite, and in a paper by Mr. Hamilton Smith, Jun., of the American Society of Civil Engineers, the efficiency of this wheel and its practical advantages are declared to be very high. Similar small impulse turbines seem likely to come into general domestic use.

The author concludes (chapter xiii.) with a description of the various hydraulic pressure engines and motors of Armstrong, Rigg, and others. These engines act by pressure only, like the steam-engine, with the disadvantage of using the same quantity of water whether working at high or low power, except in the case of Mr. Rigg's motor. Such motors are, however, coming into great use on ships, not only for working the guns, but for steering, loading, and discharging cargo.

Although designed, and amply fulfilling its purpose, as a practical treatise on hydraulic motors, this book will provide the pure theorist with some of the most elegant applications of relative velocity, aberration, dynamical principles, and of hydromechanics; and it is instructive to notice that, as in all practical mechanical treatises, gravitation units of force only are employed, even in the hydrodynamical equations of Borda and Carnot, or of Bernoulli, as we think they should be called. All this is in direct opposition to the theoretical text-books; theorist or practical man, which is to give way?

A. G. G.

PHYSIOLOGY OF EDUCATION.

Physiological Notes on Primary Education and the Study of Language. By Mary Putnam Jacobi, M.D. (New York and London: G. P. Putnam's Sons, 1889.)

THIS is a remarkable book. The authoress is an original thinker who knows how to express her thoughts clearly and strongly. It is worthy of being read by all interested in the science of education, though few perhaps even of the advocates of the present educational renaissance would be prepared to receive every one of her conclusions.

The work consists of four distinct essays. The first two are entitled "An Experiment in Primary Education," and describe the way in which Dr. Mary Jacobi taught

her own little girl. She commences the account with some very valuable remarks on the right order of studies.

"The first intellectual faculties to be trained are perception and memory. The subjects of the child's first studies should therefore be selected, not on account of their ultimate utility, but on account of their influence upon the development of these faculties. What sense is there then in beginning education with instruction in the arts of reading and writing? ... From the modern standpoint, that education means such an unfolding of the faculties as shall put the mind into the widest and most effective relation with the entire world of things—spiritual and material,—there is an exquisite absurdity in the time-honoured method. To study words before things tends to impress the mind with a fatal belief in their superior importance."

As forms and colours are the elements of all visual impressions, Dr. Jacobi began to teach her child geometrical forms before she was four years of age. At four and a half the little girl began elementary colours. Afterwards she made acquaintance with the points of the compass, the main ideas of perspective, and then maps and geography. The study of number, of course by concrete illustrations, followed that of form and outline. The observation of natural objects, especially that of plants and plant-life, was then commenced. The growth of beans and hyacinths was carefully watched, and the daily observations made by the child were written down by the mother, till she attempted them herself, and became gradually initiated into the mysteries of writing. This led her on easily to the art of reading when she was about six years of age. The progress of the child's mental development during these early years is fully described, with many pleasant recollections of her sayings.

The third part consists merely of a criticism of Miss Youman's views on the teaching of botany, and an argument in favour of commencing in a child's education with the flower rather than the leaf.

Half the book, however, is occupied by the fourth essay, in which the authoress treats of "The Place for the Study of Language in a Curriculum of Education." Of course she places it after the mind has been trained to deal with sense perceptions of external objects; but she contends earnestly for the importance of the study of words, especially for the power it possesses of enabling the child to form abstract conceptions. The authoress enters largely into the brain action involved in the use of verbal signs or complex ideas, and illustrates her views of the matter by means of physiological diagrams. She also describes a little device for the comparison of verbal roots, which she terms "language tetrahedrons," and which are intended to show the relation between Latin, French, German, and English. She would devote to literary studies, including English, the best part of the time between the Kindergarten training and the age of fourteen.

"To the study of words may be brought the scientific methods used in the study of things—observation, analysis, comparison, classification; and the child may thus begin to be trained for physical science at a time when the pursuit of most physical sciences is impossible."

It may be that Dr. Mary Jacobi claims too much time for the study of language, but the old-fashioned educationalists will get little consolation from her concessions; for she not only places the study of words after that of things, but she would have several forms of Aryan speech studied simultaneously, and she would postpone the study of grammar till two years after the serious study of language has commenced. She believes that the power of abstraction and the general mental training gained by these philological studies will enable the young person at an early age to enter upon more serious matters of study or those of more immediate practical utility.

J. H. G.

OUR BOOK SHELF.

Steam-Engine Design. By Jay M. Whitham, Professor of Engineering, Arkansas Industrial University. (London: Macmillan and Co., 1889.)

In this work the author treats of the application of the principles of mechanics to the design of the parts of a steam-engine of any type or for any duty. He acknowledges that he has culled as much information as he has required from well-known sources, both English and American; and he has embodied, as a sort of foundation for his work, a course of lectures given to his class at the United States Naval Academy by P.A. Engineer John C. Kafer, U.S.N.

After careful study, we can say that the book appears to be well suited for its purpose. The arrangement of information, both principles and details, is much the same as that in Mr. A. E. Seaton's excellent work on marine engineering; but the field covered is of far less extent, and the boiler and its accessories are not included. The author being a Professor of Engineering in an American University, we expected to find some variations from our own practice in steam-engine design. In this, however, we were disappointed. A few of the woodcuts represent parts of engines differing in insignificent details from those used in this country, but the main design is practically the same. It is gratifying to find many of our own engineers quoted as authorities in the volume—viz. D. K. Clark, A. E. Seaton, R. Sennett, and many other well-known English authorities.

It must not be supposed that there is no original work in this book. Chapters ii. and iii. for instance, on the design of slide valves and reversing gears, are ample evidence of hard work on the part of the author: his descriptions and diagrams of the various motions are excellent. Chapter iv. deals with the general design and proportions of the steam-chest, valves with their various connections. Chapters v. and vi. are on compound and triple-expansion engines, and contain also a theoretical treatment of indicator diagrams of a compound engine. These chapters are well written, and contain much useful information, but as a whole they do not teach anything new. To chapters vii. and viii., written by P.A. Engineer Asa M. Mattice, U.S.N., the same remarks will apply. The remaining chapters deal with the design of the various other parts of a steam-engine. The methods used are those well understood in every drawing-office worthy of the name, and they need not be further noticed here.

Taken as a whole, the book deserves praise for good and careful work; and we may especially call attention to the theoretical considerations, which are always clearly expressed. Although published by Messrs. Macmillan, the work is from an American press, that of Messrs. Ferris Bros., New York. The printing and woodcuts are excellent—far better, as usual, than English work of the same class.

N. J. L.

Coloured Analytical Tables. By H. W. Hake, Ph.D., F.I.C., F.C.S. (London: George Phillip and Son, 1889.)

Novelties in text-books of elementary qualitative analysis are usually conspicuous by their absence, but the

book before us takes an entirely new departure. The idea of representing the various coloured reactions by tinted imitations is, so far as we know, quite new. Apart from this, the usual well-worn paths are followed. The tables are of the simplest character, and are only sufficient for the detection of common bases in salts or oxides, no attempt being made to separate the members of the various groups. The second part is devoted to reactions for the detection of a few acids and organic substances.

The book is apparently primarily intended for the use of students preparing for the preliminary examination of the Conjoint Board of the Royal College of Physicians and Surgeons, but it will no doubt have a much wider field of usefulness if it survives the test of experience. The new method of representation seems excellently adapted for young students, and certainly no harm can be done by giving it a fair trial.

The reactions illustrated include precipitates, charcoal reactions, borax beads, and flame colorations, most of which are fairly well represented.

The Story of a Tinder Box. By Charles M. Tidy, M.B.M.S., F.C.S., &c. (London: Society for Promoting Christian Knowledge, 1889.)

POPULAR lecturers have discovered for some time that the history of the methods that have been used for obtaining a light is an excellent subject wherewith to please the public mind, and this book contains the reports of three such lectures delivered to a juvenile auditory last Christmas. An attempt has also been made to describe the experimental portion of the lectures, and the author has not committed the common error of giving a multiplicity of pretty but irrelevant experiments conveying a paucity of information. In fact, in some parts the reverse seems the case, for we must confess our inability to discover why a consideration of the allotropic modifications of carbon should necessitate a detailed description of the manufacture of black lead pencils. This digression, however, does not detract from the interest and general merit of the work, which certainly contains the explanation in simple language of some elementary physical and chemical phenomena.

Magnetism and Electricity. Part I. Magnetism. By Andrew Jamieson, M.I.C.E. (London: Griffin and Co., 1889.)

ALTHOUGH elementary text-books of physics continue to increase in number, there is still room for one of such general excellence as Prof. Jamieson's elementary manual. The book is specially arranged for the use of first year Science and Art Department and other electrical students. Numerous questions and specimen answers are distributed throughout the book, and though this may be rather suggestive of cram, there is nothing in the text to justify such a suggestion. It is unnecessary to go into details, but it may be stated that the arrangement of subjects is as good as it well can be, and on the whole the descriptions are very clear. The numerous diagrams are also excellent, those of the mariner's compass being especially good; indeed, the whole chapter on terrestrial magnetism is the best elementary account of the subject which has come under our notice.

The subject is throughout considered as an essentially practical one, and very clear instructions are given for the making of magnets, and compass and dipping needles.

If the succeeding parts of the book confirm the good opinion created by the first, teachers of the subject are to be congratulated on having such a thoroughly trustworthy text-book at their disposal.

Time and Tide: A Romance of the Moon. By Sir Robert S. Ball, LL.D., F.R.S. (London: Society for Promoting Christian Knowledge, 1889.)

THE ability of the author of this work to give a lucid exposition of an abstruse subject is a matter of common knowledge; and hence the fact that the book contains two of his lectures delivered at the London Institution last November is in itself sufficient commendation. However, be this as it may, we have no hesitation in saying there could hardly be a clearer explanation of Prof. George Darwin's theory of tidal evolution than that contained in the work before us. The hypothesis being accepted, every feature of the past and future condition of our satellite is described in a most comprehensive manner. It is first shown how, when the earth was rotating on its axis with an enormous velocity, the tidal action set up by the sun caused a portion to become detached and form our satellite. The employment of the term "conservation of spin" facilitates considerably the demonstration of the fact that as by tidal action the spin of the earth decreases—as our day lengthens—so must the dimensions of the moon's orbit be increased, and the length of the month therefore become proportionally greater. The application of Prof. Darwin's theory to other members of our system is also inquired into; and although the author does not attempt to go back to the first stage in the evolution of celestial species, he shows that tidal evolution is an extension of the hypothesis that does so. Indeed, the book is replete with information, and by the general scientific reader will be found exceedingly interesting.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Specific Inductive Capacity.

PERHAPS a better mode of performing the experiment quoted by Mr. Rudge (p. 10) is to have two insulated parallel metal plates, one connected with an electroscope, the other with a slightly-charged Leyden-jar. On now interposing a thick slab of paraffin or ebonite (recently passed through a flame) between the plates, a very decided increase of divergence will be perceived. Unless, indeed, the electroscope should happen to have overflowed to earth during the charging of the jar, in which case it will be oppositely charged and a decreased divergence may be caused. To interpose the slab is, in fact, virtually to diminish the distance between the plates, and its effect is therefore the same as that of pushing the plates closer together.

The advantage of the Leyden-jar is that it keeps the potential practically constant. If an isolated plate or sphere is used as the charged body the circumstances are not so simple; for the insertion of the slab reduces the potential and slightly increases the charge on the near face of the plate, so that whether the divergence of the leaves is increased or diminished depends on several unimportant considerations, of which the size of the slab may be one. A slab of area comparable to that of the plates between which it is put would in this case be the most suitable; and in any case it should be supported by a long insulator, so that the operator's arm, as it approaches, shall not complicate and mask the effect. OLIVER J. LODGE.

University College, Liverpool, November 9.

"La Pietra Papale."

ABOVE Stresa, on the western bank of Lago Maggiore, there is an enormous granite boulder, which deserves the attention of geologists. It lies on the left slope of an old moraine, near the little village of Gignese, and not far from the Hotel Alpino, at an elevation of about 2500 feet above the sea-level. It is roughly oblong in shape, and measures some 75 feet in length, and perhaps half as much in breadth and thickness. The projected mountain railway from Stresa to the summit of Monte Motterone will pass close to the spot where it lies, and the masons are already engaged in converting the smaller boulders into building-stones. It is to be hoped, however, that *la pietra papale*, as this splendid example of the carrying powers of ice is

called by the villagers, will not suffer the like fate. The Italian Alpine Club, will, we may trust, interest themselves in this matter. P. L. SCLATER.

Hotel du Parc, Lugano, October 21.

Who discovered the Teeth in Ornithorhynchus?

As Dr. Hart Merriam's letter on the above subject in your issue of the 7th inst. (p. 11) will be read by many who have not access to Sir Everard Home's "Lectures on Comparative Anatomy," allow me to point out that the description and figures in that work referred to by Dr. Merriam have no bearing whatever upon the very interesting discoveries recently made. They represent, not the real teeth of the young animal discovered by Mr. Poulton, and fully described by Mr. Oldfield Thomas, but the well-known horny plates which functionally take their place in the adult, and which are called "grinding teeth" by Sir Everard only in a very general sense. W. H. FLOWER.

British Museum (Natural History), November 9.

THE account of the teeth of Ornithorhynchus, given by Sir Everard Home in "Lectures on Comparative Anatomy," vol. i. p. 305, explanatory of Tab. lix. vol. ii., referred to by Mr. Hart Merriam in your last issue (p. 11), shows, even more clearly than the figures, that the *true* teeth had not been noticed at that time (1814). The passage is as follows:—"In the posterior portion of the mouth, both in the upper and lower jaw, are placed grinding teeth with broad flattened crowns, four in number, one on each side of each jaw. *They are composed of a horny substance* (the italics are my own), only embedded in the gum, to which they are connected by an irregular surface in the place of fangs. When cut through, the substance appears fibrous, like that of nail; the direction of the fibres being perpendicular to the crown, similar to that of the horny crust of the gizzard. The teeth in the young animal are smaller, and two on each side, so that the first teeth are probably shed, and the two small ones replaced by one large one."

It is perfectly evident that here no reference is made to the *true* teeth, and, moreover, the figure of the two smaller "teeth" of young specimens represents merely the immature horny plates. The honours, therefore, still remain with Mr. Poulton and Mr. Oldfield Thomas. OSWALD H. LATTER.

Anatomical Department, The Museum, Oxford, November 8.

On a Mite of the Genus *Tetranychus* found infesting Lime-trees in the Leicester Museum Grounds.

ABOUT the 13th of last September my attention was called to the strange appearance of a row of lime-trees standing in front of the School of Art buildings in Hastings Street. On examination I found that the whole row, with, I think, only one exception, were almost entirely devoid of leaves, the trunks and branches being covered with a fine web, very closely spun, giving them the appearance of being coated with a thin layer of ice, this glazed look being specially noticeable when standing in such a position as to catch the reflected rays of the sun. At first sight I imagined that I was examining the work of a spider, though I was unable to recollect any whose webs would accord with the character of those under observation. However, a close inspection revealed the webs to be tenanted by an innumerable number of yellowish or orange-coloured mites which were in some places associated together in dense masses or clusters, and more or less abundant over the whole of the trunks and branches.

These mites appeared, on being subjected to a careful microscopical examination, to be identical with *Tetranychus tiliarum*, Mull., a species which it seems that Claparède considers to be only a variety of *T. telarius*, the common "red spider." However that may be, they are at any rate closely allied forms—members of the family *Trombidiidæ*, which possess, as one of their distinguishing characteristics, a pedipalpus with a claw and a lobe-like appendage. In the genus *Tetranychus* the palpi are chelate, the mouth is furnished with a barbed sucking apparatus for the extraction of plant juices, and spinning organs are usually present. It is needless to comment upon their destructiveness to vegetation, for most keepers of gardens and hothouses are familiar with their ravages in one direction or another, and the difficulty experienced in thoroughly extirpating them.

In connection with the species which forms the subject of the present communication, I notice that Murray, in his work on the "Aptera," says: "It occasionally occurs in such numbers as almost to denude the trees of their foliage; and it has been noted that the stems and branches of such trees seemed covered with a bright glaze. Can this be a fine web?" It was so, most certainly, in the present instance, which afforded me a most favourable opportunity for examination. Again, it appears that the mites are normally found on the under-surface of the leaves, which they cover with a fine web of silk, on which (to again quote Murray) "they are sometimes crowded together in vast numbers; for example, we have seen them so thick on the leaves that they looked as if they were not merely sprinkled with a yellow orange-coloured powder, but as if it was actually in parts heaped up on them, so that none of the green colour of the leaf was visible." Their presence is of course highly injurious, causing the leaves to shrivel and drop; and it seems to me that the fact of their occurrence on the bare bark of the trunks was attributable to the death of the leaves causing them to retreat to that position, uncongenial though it would seem to be. Such trees as preserved their foliage presented no abnormal appearance on the branches, &c., notwithstanding which, in one or two instances, I believe the parasites were present on the leaves, though seemingly not in such extraordinary profusion.

Dugès, writing of *T. telarius*, states his belief that that species passes the winter under stones, and instances the finding of several active individuals so situated in a garden near Paris in the month of October. Regarding this point I may say that my specimens of *T. tiliarum*, which I placed in a box immediately after removal from the trees, speedily ensconced themselves in the most convenient nooks and crannies, in which they spun fine webs. It may be worth noting that the days on which my observations were made were warm and damp, with scarcely any wind, quite typical early autumn days in fact.

F. R. ROWLEY.

Leicester Museum.

Retarded Germination.

I SHALL be much obliged to any of your readers who can give an explanation of the probable cause of the above phenomenon, which I have remarked this year. I sowed a number of patches of seeds of various hardy annuals in the garden in the last week of April; about half of them came up after the usual interval, strongly and regularly. Such were *Calendula Pongei*, *Convolvulus minor*, *Lavatera trimestris*, *Collinsia bicolor*, *Iberis* white and red, *Specularia speculum*, *Linum rubrum*, &c., &c. Then there were some of which a few scattered seedlings made their appearance at this time, and after an interval of about six weeks the greater part of them also came up; among these were *Eutoca viscida*, *Nigella damascena*, *Sphenogyne*, and *Clarkia pulchella*. Thirdly, there were some of which I quite despaired; mignonette, however, appeared thinly about the end of June, and at intervals till August; and in the middle of June a few plants (in proportion to the seed sown, a few) of *Linaria bipartita*, *Madia elegans*, and *Xeranthemum* came up—one consequence being that the last named has not yet flowered. Some of the seeds were obtained this spring from seedsmen, some were my own collection of the last year or two—of the latter were *Calendula*, *Lavatera*, *Convolvulus*, *Specularia*, *Eutoca*, *Nigella*, *Sphenogyne*, and mignonette—so that cannot be said to give any clue. The conditions for germination and growth were favourable, and the season also. I have never remarked before any *annuals* so long in appearing above ground; though in some herbaceous plants I have noticed it, *e.g.* *Gaillardia*, *Myosotis alpestris*, and *Anemone coronaria*. E. A.

Herefordshire, September 19.

The Relation of the Soil to Tropical Diseases.

As a humble subscriber to and student of NATURE, will you bear with me while I ask your help, as shortly and plainly as I can? I am in a very secluded corner of one of the Native States of Rajpootana, and I am collecting facts and making observations on the relation of the *soil* to tropical diseases; my ambition being to discuss it not so much from a statistical and geographical standpoint, as from the geological, in its chemical and biological

aspects; though, as I conceive, the geographical, climatological, and geological elements in the problem are not to be arbitrarily distinguished. Now I am far away from all books of reference, and it is of course essential that I make myself acquainted with what has already been done in these subjects, and I venture to ask for any hints as to the bibliography of them. Can you tell me if anyone has done for geology what Hirsch, of Berlin, has done for geography (in his work on the distribution of disease)? Is there any authority on the chemistry of soils, and what I roughly call their physiology and pathology, their structural and functional changes under influences—climate notably—and their own intrinsic, and the deeper geological interactions?

A. ERNEST ROBERTS.

Meywar Bheel Corps, Kherwara, Central India, September 9.

The Earthquake of Tokio, April 18, 1889.

DR. VON REBEUR-PASCHWITZ's letter, which appeared in NATURE, vol. xl. p. 294, is of special interest to us in Japan, countenancing as it does the conjecture that the very peculiar earthquake felt and registered here on April 18 was the result of a disturbance of unusual magnitude. It was my good fortune on the day in question to be engaged in conversation with Prof. Sekiya in the Seismological Laboratory at the very instant the earthquake occurred. We at once rushed to the room where the self-recording instruments lay, and there, for the first time in our experience, had the delight of viewing the pointers mark their sinuous curves on the revolving plates and cylinders. At first sight it seemed as if the pointers had gone mad, tracing out sinuosities of amplitudes five or six times greater than the greatest that had ever before been recorded in Tokio. There was not much *sensation* of an earthquake; indeed, after the first slight tremor that attracted our attention, we felt nothing at all, although in the irregular oscillations of the seismograph pointers we had evidence enough that an earthquake was passing. Very few in Tokio were aware that there had been an earthquake till they read the report of it in the next day's papers. Thus the motion, though large, was too slow to cause any of the usual sensations that accompany earthquakes, and suggested a distant origin and a large disturbance, with a consequent wide extension of seismic effect. Excepting the slight tremors recorded at Potsdam and Wilhelmshaven, there has been, so far, no evidence of any such far-reaching action.

My object in writing this note, however, is to correct an error of calculation which Dr. von Rebeur-Paschwitz has unwittingly made. He has assumed that Tokio standard time is mean local time. On the contrary, the standard time for all Japan is the mean solar time for longitude 135° E.,—that is, nine hours in advance of Greenwich mean time. Hence, instead of the Tokio earthquake having preceded the German disturbance by 1h. 4·3m. it preceded it by only 45m. This correction increases the velocity of transmission to 3060 metres per second. We must assume, then, either that large disturbances in the heart of the earth travel with exceptionally high speeds, or that the origin of the disturbance was a considerable distance from Tokio. The latter assumption seems sufficiently satisfactory, if in other respects Dr. von Rebeur-Paschwitz's views meet with approval.

CARGILL G. KNOTT.

Imperial University, Tokio, Japan, September 25.

A Brilliant Meteor.

YESTERDAY evening, November 4, at 7.55 p.m., I was fortunate enough to observe a very brilliant meteor. It became visible almost exactly at the zenith, or a little west of it, and moved, as nearly as I could judge, due east, magnetic; it remained visible for about from one to two seconds, disappearing, finally, rather low down on the eastern horizon. For the first half of its journey it was of a dazzling white brightness, and then it suddenly became a dull red spark. The light emitted from it when brightest reminded me of the light from an arc lamp, and was very much brighter than any of the fixed stars.

As it was so short a time in view, and there were no stars visible, I could only approximately estimate its point of appearance and path. There were a few clouds about, mostly in the west, and the moon was behind them. PAUL A. COBBOLD.

Warwick School, November 5.

ON THE HARDENING AND TEMPERING OF STEEL.[1]

II.

THE following considerations appear to have guided Osmond in beginning his investigations (see *ante*, p. 16). Bearing in mind the fact that molecular change in a body is always accompanied by evolution or absorption of heat, which is, indeed, the surest indication of the occurrence of molecular change, he studied with the aid of a chronograph what takes place during the slow cooling and the slow heating of masses of iron or steel, using, as a thermometer to measure the temperature of the mass, a thermo-electric couple of platinum and of platinum containing 10 per cent. of rhodium, converting the indications of the galvanometer into temperatures by Tait's formulæ.

FIG. 5.

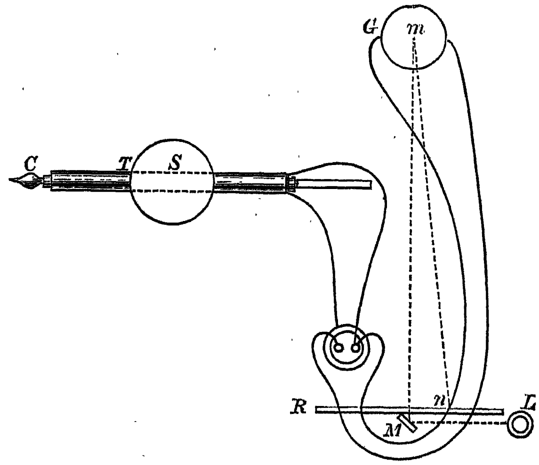


FIG. 6.

FIGS. 5 and 6 show the actual mode of conducting the experiments. F (Fig. 5) is a piece of steel into which a platinum and platinum-rhodium couple, *t*, *t'*, is fixed. It is inclosed in a glazed porcelain tube and heated to bright redness in the furnace, S (Fig. 6). This tube, T, may be filled with any gaseous atmosphere. C is a bulb filled with chloride of calcium. The metal under examination is slowly cooled down. The wires from the thermo-couple pass to the galvanometer, G. The rate of cooling of the mass is indicated by the movement of a spot of light from the galvanometer mirror at *m*, on the screen, R, and is recorded by a chronograph. The source of light is shown at L; M is a reflector.

In the next diagram (Fig. 7) temperatures through which a slowly-cooling mass of iron or steel passes, are arranged along the horizontal line, and the intervals of time during which the mass falls through a definite number (6·6) of degrees of temperature are shown vertically by ordinates. See what happens while a mass of electro-deposited iron (shown by a dotted line), which is as pure as any iron can be, slowly cools down. From 2000° to 870° it falls uniformly at the rate of about 2·2° a second, and the intervals of temperature are plotted as dots at the middle of the successive points of the intervals. When the temperature falls down to 858°, there is a sudden arrest in the fall of temperature, the indicating spot of light, instead of falling at a uniform rate of about 2° a second, suddenly takes 26

[1] A Lecture delivered on September 13, by Prof. W. C. Roberts-Austen, F.R.S., before the members of the British Association. Continued from p. 16.

seconds to fall through an interval of temperature which hitherto and subsequently only occupies about 6 seconds. Turn to the diagram, and see what actually happens when the iron contains carbon in the proportion required to constitute it mild steel (shown by thin continuous line, Fig. 7); there is not one, but there are two such breaks in the cooling, and both breaks occur at a different temperature from that at which the break in pure iron occurred. As the proportion of carbon increases in steel, the first break in cooling travels more and more to the right, and gradually becomes confounded with the second break, which, in steel containing much carbon, is of long duration, lasting as much as 76 seconds in the case of steel containing 1·25 per cent. of carbon (thick continuous line, (Fig. 7).

[In the experiments shown to the audience the spot of light moved slowly and uniformly along a screen ten feet in length. It halted for a few seconds as the temperature of the cooling mass of steel fell to about 850° C., and when the metal was at dull redness, the spot of light remained stationary for 68 seconds, and then resumed its course.]

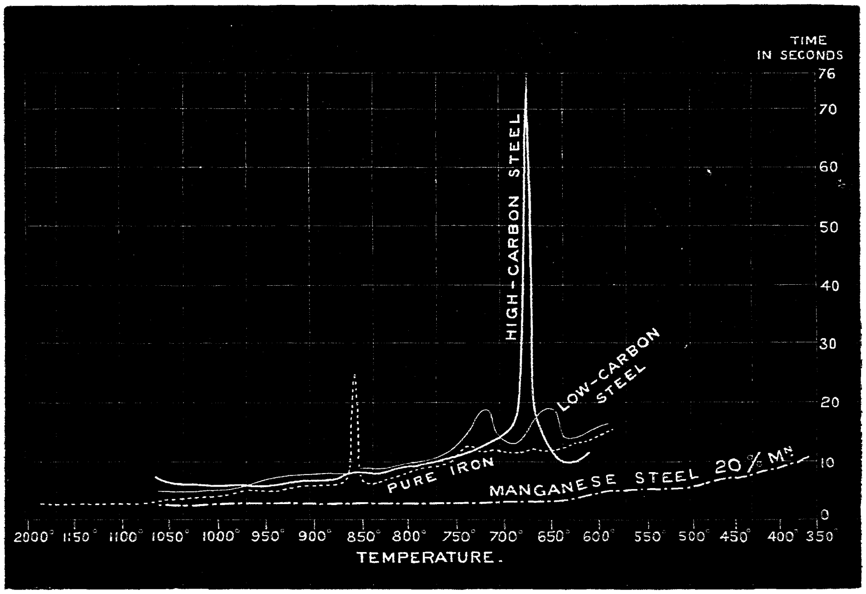


FIG. 7.—The curves in this diagram show how the rate of movement of the spot of light varies with different samples of steel. The stoppage of the movement of the spot of light of course indicates the evolution of heat from the cooling mass of steel, F (Fig. 5).

Now, it may be urged, evidently the presence of carbon has an influence on the cooling of steel when left to itself: may it not affect molecular behaviour during the rapid cooling which is essential to the operation of hardening? We know that the carbon, during rapid cooling, passes from the state in which it is combined with the iron into a state in which it is dissolved in the iron; we also know that, during slow cooling, this dissolved carbon can re-enter into combination with the iron so as to assume the form in which it occurs in soft steel. Osmond claims that this second arrestation in the fall of the thermometer corresponds to the recalescence of Barrett, and is caused by the re-heating of the wire by the heat evolved when carbon leaves its state of solution and truly combines with the iron.

If it is hoped to *harden* steel, it must be rapidly cooled before the temperature has fallen to a definite point, not lower than 650°, or the presence of carbon will be unavailing. But what does the first break in the curves mean? You will see that a break occurs in electrotype iron which is free from carbon (thin dotted line, Fig. 7); it must then indicate some molecular change in iron itself, accompanied with evolution of heat—a change with which carbon has nothing whatever to do, for no carbon is present; and Osmond argues thus:—There are two kinds of *iron*, the atoms of which are respectively arranged in the molecules so as to constitute *hard* and *soft iron*, quite apart from the presence or absence of carbon. In red-hot iron the mass may be soft but the molecules are hard—let us call this β iron; cool such red-hot pure iron, whether quickly or slowly, and it becomes soft; it passes to the α soft modification—there is nothing to prevent its doing so. It appears, however, that if carbon is present, and the metal be rapidly cooled, the following result is obtained: a certain proportion of the molecules are retained in the form in which they existed at a high temperature—the hard form, the β modification—and hard *steel* is the result.

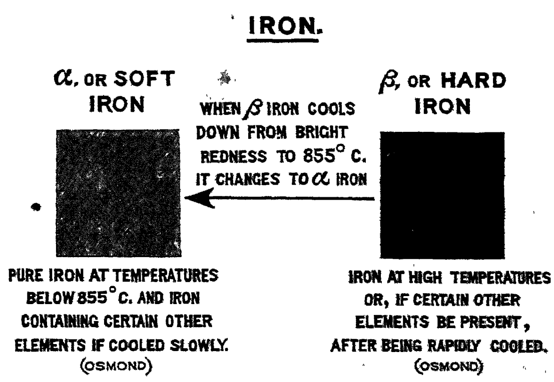


FIG. 8.

The main facts of the case may, perhaps, be made clearer by the aid of this diagram (Fig. 8) which shows the relation between α and β iron. This molecular change from β iron to α iron during the slow cooling of a mass of iron or steel is, according to Osmond's theory, indicated by the first break in the curve, representing the slow cooling of iron, as is proved by the fact that it occurs alone in electro-iron. A second break, usually one of much longer duration, marks the point at which carbon itself changes from

the dissolved or hardening carbon to the combined carbide-carbon. It follows that, if steel be quickly cooled *after the change from β to α* has taken place but before the carbon has altered its state—that is, before the change indicated by the second break in the curve has been reached—then the iron should be soft, but the carbon, hardening carbon; and as such, the action of a solvent should show that it cannot be released from iron in the black carbide form. This proves to be the case, and affords strong incidental proof of the correctness of the view that two modifications of iron can exist.

It will be seen, therefore, that, although the presence of carbon is essential to the hardening of steel, the change in the mode of existence of the carbon is less important than has hitherto been supposed.

The α modification of iron may be converted into the β form by stress applied to the metal at temperatures below a dull red heat, provided the stress produces permanent deformation of the iron,[1] but the consideration of this question would demand a lecture to itself. I am anxious to show you an experiment which will help to illustrate the existence of molecular change in iron.

Here is a long bar of steel containing much carbon. In such a variety of steel, the molecular change of the iron itself, and the change in the relations between the carbon and the iron, would occur at nearly the same moment. It is now being heated to redness, but if you will look at this diagram (Fig. 9), you will be prepared for what I want

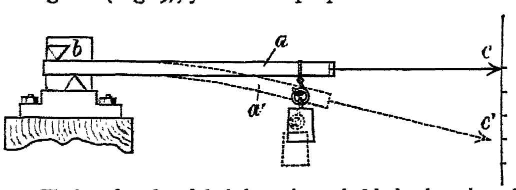


FIG. 9.—The bar of steel, *a*, ¼ inch in section and 18 inches long, heated to bright-redness and firmly fixed in a vice or other support at *b*. A weight of about 2 pounds is rapidly hung on to the free end, and a light pointer, *c*, is added to magnify the motion of the bar. It remains perfectly rigid for a period varying from 30 to 40 seconds, and then, when the bar has cooled down to very dull redness, it suddenly bends, the pointer falling from 6 to 8 inches to the position *c'*.

you to see in the actual experiment. One end of the red-hot bar *a* will be firmly fixed at *b*, a weight *not sufficient to bend it* is slung to the free end, which is lengthened by the addition of a reed, *c*, to magnify any motion that may take place. Now remember that as the bar will be red-hot it ought to be at its softest, you would think, when it is freshly withdrawn from the furnace, and if the weight was ever to have power to bend it, it would be then; but, in spite of the rapidity with which such a thin bar cools down in the air and becomes rigid, points of molecular weakness come when the iron changes from β to α, and the carbon passes from hardening carbon to carbide-carbon; at that moment, at a temperature much below that at which it is withdrawn from the furnace, the bar will begin to bend, as is shown by the dotted lines *a'*, *c'*. It has been found experimentally that this bend occurs at the point at which, according to Osmond's theory, molecular change takes place. Mr. Coffin takes advantage of this fact to straighten distorted steel axles.[2]

There is a sentence in the address which has just been delivered before Section G, by Mr. Anderson, which has direct reference to molecular change in iron. He says:—

"When, by the agency of heat, molecular motion is raised to a pitch at which incipient fluidity is obtained, the particles of two pieces brought into contact will interpenetrate or diffuse into each other, the two pieces will unite into a homogeneous whole, and we can thus grasp the full meaning of the operation known as 'welding.'"

It is, however, possible to obtain evidence of interchange of molecular motion, as has been so abundantly shown by Spring, even at the ordinary temperature, while, in the case of steel, it must take place far below incipient fluidity—indeed, at a comparatively low temperature, as is shown by the following experiment on the welding of steel. Every smith knows how difficult it is to weld highly carburized hard tool-steel, but if the ends of a newly-fractured $\frac{3}{16}$-inch square steel rod, *a* (Fig. 10), are placed

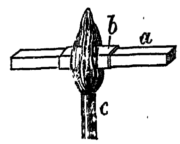


FIG. 10.

together and covered with platinum foil, *b*, so as to exclude the air, and if the junction is heated in the flame of a Bunsen burner, *c*, the metal will weld, without pressure, so firmly that it is difficult to break it with the fingers, although the steel has not attained a red-heat.[1]

The question now arises, What is the effect of the presence of other metals in steel, of which much has been heard recently? (1) Manganese. Osmond has shown that this metal enables steel to harden very energetically, as is well known. If much of it be present, 12 to 20 per cent., in iron, *no break whatever* is observed in the curve which represents slow cooling (see line marked "manganese steel" (Fig. 7). That is, the iron never shows such a change as that which occurs in other cooling masses of iron. Then you will say such a material should be hard however it is cooled. So it is. There is one other important point of evidence as to molecular change connected with the addition of manganese to submit to you. Red-hot iron is not magnetic. Hopkinson[2] has shown that the temperature of recalescence is that at which iron ceases to be magnetic. It may be urged that β iron cannot therefore be magnetized. Steel containing much manganese cannot be magnetized, and it is therefore fair to assume that the iron present is in the β form. Hadfield[3] has given metallurgists wonderful alloys of iron and manganese in proportions varying from 7 to 20 per cent. of manganese. This core of iron round which a current is passing, attracts the sphere of iron, but if nothing is changed, except by replacing the core of iron with a core of Hadfield's steel, it is impossible to make a magnet of it. [Experiment shown.]

Prof. Ewing, who has specially worked on this subject, concludes that, "no magnetizing force to which the metal is likely to be subjected in any of its practical applications would produce more than the most infinitesimal degree of magnetization" in this material.

It has been seen that quantities of manganese above 7 per cent. appear to prevent the passage of β iron into the α form. In smaller quantities manganese seems merely to retard the conversion, and to bring the two loops of the diagram nearer together.

Time will not permit me to deal with the effect of other elements on steel. I will only add that tungsten possesses the same property as manganese, but in a more marked degree. Chromium has exactly the reverse effect, as it enables the change of hard β iron to α soft iron to take place at a higher temperature than would otherwise be the case, and this may explain the extreme hardness of chromium steels when hardened in the same way as ordinary steels.

There are a few considerations relative to the actual working of steel with which I can deal but briefly, notwithstanding their industrial importance. The points *a* and *b*, adopted in the celebrated memoir of Chernoff to which

[1] "Études Métallurgiques," par Osmond, p. 6 (Paris: Dunod, 1888.)
[2] Trans. American Soc. Civil Engineers, xvi., 1887, p. 324.

[1] Trans. American Society Mechanical Engineers, ix., 1888, p. 155.
[2] Proc. Roy. Soc., xlv., 1889, pp. 318, 445, and 457.
[3] Proc. Inst. Civil Engineers, xciii. Part iii., 1888.

I have referred already, change in position with the degree of carburization of the metal. It is useless to attempt to harden steel by rapid cooling if it has fallen in temperature below the point (in the red) *a*, and this is the point of "recalescence" at which the carbon combines with the iron to form carbide-carbon: it is called V by Brinell. In highly carburized steel, it corresponds exactly with the point at which Osmond considers that iron, in cooling slowly, passes from the β to the α modification. Now with regard to the point *b* of Chernoff. If steel be heated to a temperature above *a*, but below *b*, it remains fine-grained however slowly it is cooled. If the steel be heated above *b*, and cooled, it assumes a crystalline granular structure whatever the rate of cooling may be. The size of the crystals, however, increases with the temperature to which the steel has been raised.

Now the crystalline structure, which is unfavourable to the steel from the point of view of its industrial use, may be broken up by the mechanical work of forging the hot

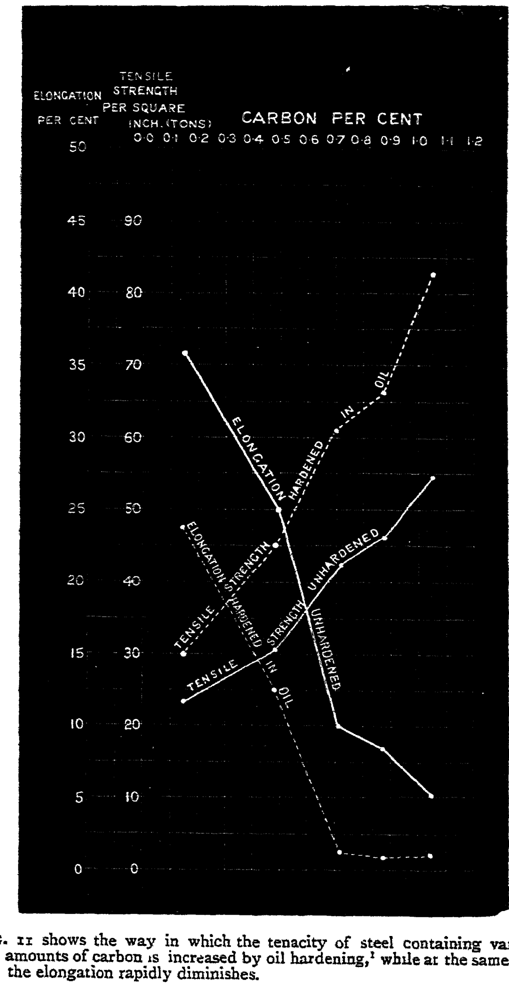


FIG. 11 shows the way in which the tenacity of steel containing varying amounts of carbon is increased by oil hardening,[1] while at the same time the elongation rapidly diminishes.

mass; and the investigations of Abel, of Maitland, and of Noble, have shown how important "work" on the metal is. When small masses of hot steel are quenched in oil, they are hardened just as they would be if water were used as a cooling fluid. With large masses, the effect of quenching in oil is different. Such cooling of large hot masses appears to break up this crystalline structure in a manner analogous to mechanical working. If the mass of metal is very large, such as a propeller shaft, or tube of a large gun, the change in the relations between the carbon and the iron, or true "hardening" produced by such oil treatment is only effected *superficially*—that is, the hardened layer does not penetrate to any considerable depth, but the innermost parts are cooled more quickly than they otherwise would have been, and the development of the crystals, which would have assumed serious proportions during slow cooling, is arrested. It depends on the size of the quenched mass, whether the tenacity of the metal is or is not increased, but its power of being elongated is considerably augmented. This prevention of crystallization I believe to be the great merit of oil quenching, which, as regards large masses of metal, is certainly not a true hardening process.

[1] This was well shown in Prof. Akerman's celebrated paper on "Hardening Iron and Steel," Journ. Iron and Steel Institute, 1879, Part ii. p. 504.

There has been much divergence of view as to the relative advantages of work on the metal, and of oil-hardening, but I believe it will be possible to reconcile these views, if the facts I have so briefly stated be considered.

The effect of annealing remains to be dealt with. In a very complicated steel casting, the cast metal probably contains much of its carbon as hardening carbon, and the mass which has necessarily been poured into the mould at a high temperature is crystalline. The effect of annealing is to permit the carbon to pass from the "hardening" to the "carbide" form, and, incidentally, to break up the crystalline stucture, and to enable it to become minutely crystalline. The result is that the annealed casting is far stronger and more extensible than the original casting. The carbide-carbon is probably interspersed in the iron in fine crystalline plates, and not in a finely divided state. It would obviously be impossible to "work"—that is, to hammer—complicated castings, and the extreme importance of obtaining a fine crystalline structure by annealing, with the strength which results from such a structure, has been abundantly demonstrated by Mr. J. W. Spencer, whose name is so well known to you all in Newcastle.

The effect of annealing and tempering is in fact very complicated, and I can only again express my wish that it were possible to do justice to the long series of researches which Barus and Strouhal have conducted in recent years. They consider that, annealing is demonstrably accompanied by chemical change, even at temperatures slightly above the mean atmospheric temperature, and that the "molecular configuration of glass-hard steel is always in a state of incipient change, . . . a part of which change must be of a permanent kind." Barus says "that during the small interval of time within which appreciable annealing occurs, a glass-hard steel rod suddenly heated to 300° is almost a viscous fluid."[1] Barus considers that glass-hard steel is constantly being spontaneously "tempered" at the ordinary temperature, which, he says, "acting on freshly quenched [that is hardened] steel for a period of years, produces a diminution of hardness about equal to that of 100° C., acting for a period of hours."

The nature of the molecular change is well indicated in the long series of researches which led them to conclude that in steel "there is a limited interchange of atoms between molecules under stress, which must be a property common to solids, if, according to Maxwell's conception, solids are made up of configurations in all degrees of molecular stability."

Barus and Strouhal attach but little importance to the change in the relations between the carbon and the iron during the tempering and annealing of hard steel. They consider that in hardening steel the "strain once applied to steel is locked up in the metal in virtue of its

[1] Phil. Mag., xxvi., 1888, p. 209.

viscosity"; tempering is the release of this molecular strain by heat.

Highly carburized steels harden very energetically by very slight modifications in thermal treatment, and it will be evident that a very hard material is unsuitable for industrial use if the conditions of its employment are such as to render it desirable that the material should stretch. To turn to very "mild" steel which does not harden, it is certain that, although wrought iron passes almost insensibly into steel, there can be no question that not merely the structural but the molecular aggregation of even steel containing only $\frac{2}{10}$ per cent. of carbon is profoundly different from that of wrought iron. Formerly, as Sir F. Bramwell pointed out in a lecture delivered at the Royal Institution in 1877, "by the year 1830 . . . from small beginnings in Staffordshire and at Birkenhead sprang a wonderful *wrought-iron* navy, but steel was a luxury: it was made in small portions sold at high prices, as much as a shilling or eighteenpence a pound. It was employed for swords, cutlery, and tools, needles and other purposes where the quantity used was but trifling, and where the importance of the superior material was such as to justify the large expenditure incurred. It was felt in those days that steel was worth paying for because it was trusted; indeed its trustworthiness had passed into a proverb"—"as true as steel."

The class of steel which was formerly employed, as I have just indicated, for weapons and tools belonged to the highly carburized, readily-hardening class. It was the "mild steel" containing but little carbon which was destined to replace wrought iron, and when attempts were made to effect the general substitution of steel for iron, fears as to its character and trustworthiness unfortunately soon arose, so that from about the year 1860 until 1877 steel was viewed with suspicion. We can now explain this. Doubts as to the fidelity of steel, even when it was obtained free from entangled cinder, arose from ignorance of the fact that, on either side of a comparatively narrow thermal boundary, the iron in steel can practically exist in two distinct modifications. The steel was true enough, but from the point of view of the special duties to be intrusted to it, its fidelity depended on which modification of iron had to be called to the front. Artificers attempted to forge steel after it had cooled down below the point *a* of Chernoff, at which recalescence occurs, and they often attempted to work highly carburized steel at temperatures which were not sufficiently low.

Steels may be classified from the point of view of their industrial use according to the amount of carbon they contain, and I have attempted to arrange in this trophy certain typical articles, grouped under certain definite percentages of carbon ranging from $\frac{2}{10}$ to $1\frac{1}{2}$ per cent. [This was a trophy 18 feet square, with various typical articles of steel arranged in order according to the amount of carbon they contained. I am greatly indebted to Mr. J. W. Spencer, of Newcastle, who kindly lent me the fine series of specimens of which the "trophy" is built up.] Each class merges into the other, but the members at either end of the series vary very greatly. It would be impossible to make a razor which would cut from boiler plate; and conversely, a boiler made of razor steel would possibly fracture at once if it were superheated and subjected to any sudden pressure of steam. Speaking generally, if the steel contains, in addition to carbon, $\frac{1}{10}$ per cent. of manganese, each class of steel, as at present arranged, would have to be shifted a class backwards towards the left of the trophy.

At the present day, instead of steel being manufactured and used in small quantities, about 4,000,000 tons are annually employed in this country. Let us see how it is used. A steel fleet, the finest fleet in the world, has recently assembled at Spithead. The material of which it was made contained $\frac{15}{100}$ to $\frac{2}{10}$ per cent. of carbon, and when steel faces are used for the armour plates, the material contains $\frac{7}{10}$ to $\frac{8}{10}$ per cent. of carbon.

It has been pointed out that the crews of the fleet at Spithead numbered no less than 21,107 men. This it has been shown is "a remarkable figure, considering the great economy in men which prevails in a modern navy as compared with the navy of Nelson's day. A hundred years ago the normal requirements of a fleet were one man to a little over four tons, but now, thanks to the part played by steel and hydraulic power, we require but one man to every seventeen tons. Thus it may roughly be said that an aggregate of 20,000 men at the present day corresponds to an aggregate of 80,000 men in the days of Nelson." The latest type of battle-ship weighs, fully equipped, about 10,000 tons, there being about 3400 tons of steel in the hull, apart from her armour, which, with its backing, will weigh a further 2800 tons.[1]

From the use of steel in the Royal Navy and in the mercantile marine, let us pass on to its most notable use in construction. If the President of the French Republic was justified in appealing, in a recent speech, to the Eiffel Tower as "a monument of audacity and science,"[2] what are we to say of the Forth Bridge, the wonders of which will be described by Mr. Baker on Saturday? By his kindness I am able to place in the position in the trophy justified by the carbon it contains, a plate from the Forth Bridge, which fell from a height of some 350 feet, and, being of excellent quality, doubled itself on the rocks below. A single span of the Forth Bridge is nearly as long as two Eiffel Towers turned horizontally and tied together in the middle, and the whole forms a complicated steel structure weighing 15,000 tons, erected without the possibility of any intermediate support, the lace-like fabric of the bridge soaring as high as the top of St. Paul's. The steel of which the compression members of the structure are composed contains $\frac{23}{100}$ per cent. of carbon and $\frac{69}{100}$ per cent. of manganese. The parts subjected to extension do not contain more than $\frac{18}{100}$ per cent. of carbon.[3]

Time will not permit me to pass the members of each class in review. I can only refer to very few. Steel for the manufacture of pens contains about $\frac{9}{10}$ per cent. of carbon, and 16 to 18 tons of steel are every week let loose on an unoffending world in the shape of steel pens.

Steel rails contain from $\frac{3}{10}$ to $\frac{4}{10}$ per cent. of carbon, and, in this class, slight variations in the amount of carbon are of vital importance. An eminent authority, Mr. Sandberg, tells us that in certain climates a variation of $\frac{1}{10}$ per cent. in the amount of carbon may be very serious. The great benefit which has accrued to the country from the substitution of more durable steel rails for the old wrought-iron ones may be gathered from the figures which Mr. Webb, of Crewe, has given me, which show that "the quantity of steel removed from the rails throughout the London and North-Western system by wear and oxidation is about 15 cwt. an hour, or 18 tons a day."

Gun-steel contains $\frac{3}{10}$ to $\frac{5}{10}$ per cent. of carbon, and it may contain $\frac{8}{10}$ per cent. of manganese. It is in relation to gun-steel that oil-hardening becomes very important. The oil-tank of the St. Chamond Works (on the Loire) is 72 feet deep, and contains 44,000 gallons of oil, which is kept in circulation by rotary pumps, to prevent the oil being unduly heated locally when the heated mass of steel is plunged into it.

Now with regard to projectiles. To quote some recent remarks of Lord Armstrong,[4] "the heaviest shot used in the *Victory* was 68 pounds, while in the *Victoria* it will be 1800 pounds; and, while the broadside-fire from the

[1] Address by Mr. Baker, Section G, British Association Report, 1885, p. 1182.
[2] *Times*, August 19, 1889.
[3] Journal of the Iron and Steel Institute, 1888, ii. p. 94.
[4] *Times*, August 3, 1889.

Victory consumed only 325 pounds of powder, that from the *Victoria* will consume 3000 pounds. The most formidable projectiles belong to the highly carburized class of steel. Shells contain 0·8 to 0·94 per cent. of carbon, and, in addition, some of these have 0·94 to 2 per cent. of chromium. The firm of Holtzer shows, in the Paris Exhibition, a shell which pierced a steel plate 10 inches thick, and was found, nearly 800 yards from the plate, entire and without flaw, its point alone being slightly distorted. Compound armour-plate with steel face, which face contains 0·8 per cent. of carbon, is, however, more difficult to pierce than a simple plate of steel.

[A prominent feature in the "trophy," among the class of highly carburized steels which contain over $\frac{7}{10}$ per cent. of carbon, was a fine suspended wire $\frac{8}{100}$ of an inch diameter, of remarkable strength, supporting a weight of $2\frac{1}{2}$ cwt., or a load of nearly 160 tons to the square inch. The strength of the same steel *undrawn*, would not exceed 50 tons to the square inch. A similar wire manufactured by the steel company of Firminy attracted much attention in the Paris Exhibition by supporting a shell weighing 1800 lbs., or a load of 158 tons per square inch.]

Lastly, I will refer to the highly carburized steel used for the manufacture of dies. Such a steel should contain 0·8 to 1 per cent. of carbon, and no manganese. It is usual to water-harden and temper them to a straw colour, and a really good die will strike 40,000 coins of average dimensions without being fractured or deformed; but I am safe in saying that if the steel contained $\frac{1}{10}$ per cent. too much carbon, it would not strike 100 pieces without cracking, and if it contained $\frac{2}{10}$ per cent. too little carbon, it would probably be hopelessly distorted, and its engraved surface destroyed, in the attempt to strike a single coin.

The above examples will be sufficient to show how diverse are the properties which carbon confers upon iron, but as Faraday said, in 1822, "It is not improbable that there may be other bodies besides charcoal capable of giving to iron the properties of steel." The strange thing is that we do not know with any certainty whether, in the absence of carbon, other elements do play the part of that metalloid, in enabling iron to be hardened by rapid cooling. Take the case of chromium, for instance: chromium-carbon steels can, as is well known, be energetically hardened, but Busek[1] has recently asserted that the addition of chromium to iron in the absence of carbon does not enable the iron to be hardened by rapid cooling. So far as I can see, it is only by employing the electrical method of Pepys that a decision can be arrived at as to the hardening properties of elements other than carbon.

A few words must be devoted to the consideration of the colours which, as I said (see *ante*, p. 11), direct the artist in tempering or reducing the hardness of steel to any determinate standard. The technical treatises usually give—not always accurately, as Reiser[2] has shown—a scale of temperature ranging from 220° to 330°, at which various tints appear, passing from very pale yellow to brown yellow, purples, and blues, to blue tinged with green, and finally to grey. Barus and Strouhal[3] point out that it is possible that the colour of the oxide film may afford an indication of the temper of steel of far greater critical sensitiveness than has hitherto been supposed. It is, however, at present uncertain how far time, temperature, and colour are correlated, but the question is being investigated by Mr. Turner, formerly one of my own students at the School of Mines.

That the colours produced are really due to oxidation was shown by Sir Humphry Davy in 1813,[4] but the nature of the film has been the subject of much controversy. Barus points out that "the oxygen molecule does not penetrate deeper than a few thousand times its own dimensions,[1] and that it probably passes through the film by a process allied to liquid diffusion. The permeable depth increases rapidly with the temperature, until at an incipient red heat the film is sufficiently thick to be brittle and liable to rupture, whereupon the present phenomenon ceases, or is repeated in irregular succession.

Looking back over all the facts we have dealt with, it will be evident that two sets of considerations are of special importance: (1) those which belong to the relations of carbon and iron, and (2) those which contemplate molecular change in the iron itself. The first of these has been deliberately subordinated to the second, although it would have been possible to have written much in support of the view that carburized iron is an alloy of carbon and iron, and to have traced with Guthrie the analogies which alloys, in cooling, present to cooling masses of igneous rocks, such as granite, which, as the temperature of the mass falls, throws off "atomically definite"[2] bodies, leaving behind a fluid mass of indefinite composition, from which the quartz and feldspar solidify before the mica. This view has been developed with much ability in relation to carburized iron by Prof. Howe, of Boston, who even suggests mineralogical names, such as "cementite," "perlite," and "ferrite," for the various associations of carbon and iron.

I am far from wishing to ignore the interest presented by such analogies, but I believe that the possibility of molecular change in the iron itself, which results in its passage into a distinctive form of iron, is at present the more important subject for consideration, not merely in relation to iron, but as regards the wider question of allotropy in metals generally.

Many facts noted in spectroscopic work will have, as Lockyer has shown, indicated the high probability that the molecular structure of a metal like iron is gradually simplified as higher temperatures are employed. These various simplifications may be regarded as allotropic modifications.

The question of molecular change in solid metals urgently demands continued and rigorous investigation. Every chemist knows how much his science has gained, and what important discoveries have been made in it, by the recognition of the fact that the elements act on each other in accordance with the great law of Mendeleeff which states that the properties of the elements are periodic functions of their atomic weights. I firmly believe that it will be shown that the relation between small quantities of elements and the masses in which they are hidden is not at variance with the same law. I have elsewhere tried to show[3] that this may be true, by examining the effect of small quantities of impurity on the tenacity of gold.

In the case of iron, it is difficult to say what property of the metal will be most affected by the added matter. Possibly the direct connection with the periodic law will be traced by the effect of a given element in retarding or promoting the passage of ordinary iron to an allotropic state; but "the future of steel" will depend on the care with which we investigate the nature of the influence exerted by various elements on iron, and on the thermal treatment to which it may most suitably be subjected.

Is it not strange that so many researches should have been devoted to the relations between carbon, hydrogen, and oxygen in organic compounds, so few to the relations of iron and carbon, and hardly any to iron in association with other elements? I think that the reason for the comparative neglect of metals as subjects of research arises

[1] *Stahl und Eisen*, ix. 1889, p. 728.
[2] "Das Härten des Stahles," p. 78 (Leipzig, 1881). See also Lœwenherz, *Zeitschrift für Instrumentenkunde*, ix., 1889, p. 322.
[3] *Bull. U.S. Geo. Survey*, No. 27, 1886, p. 51.
[4] Sir Humphry Davey, *Thomson's Ann. Phil.*, i., 1813, p. 131; quoted by Turner, Proc. Phil. Soc., Birmingham, vi., 1889, part 2.

[1] *Bull. U.S. Geo. Survey*, No. 35, 1886, p. 51.
[2] *Phil. Mag.*, June 1884, p. 462.
[3] Phil. Trans. Roy. Soc., clxxix., 1888, p. 339.

from the belief that methods which involve working at high temperatures are necessarily inaccurate; but the school of Ste. Claire-Deville has shown that they are not, and there are signs among us that our traditional love for the study of metals is reviving. Of course it cannot be that chemists and physicists are afraid "that science will be degraded by being applied to any purpose of vulgar utility," for I trust that I shall at least have shown that the empire over matter, and the true advancement of science, which I suppose is the object of all research, may be as certainly secured in the field of metallurgy as in any other.

PROF. WEISMANN'S "ESSAYS."

PROF. WEISMANN'S suggestions are, with reason, universally recognized as being most important and valuable; nevertheless certain questions treated of by him seem to me to require further solution, and at present to constitute difficulties which oppose themselves to an entire acceptance of his hypotheses.

Death in the Metazoa is, according to him, due (new translation, Clarendon Press, p. 21) to the cells of their tissues having ceased to be able to reproduce themselves—in "the limitation of their powers of reproduction." Such a cessation may be an inevitable result of an excessive amount of work or efficiency on their part, and "the advantages gained by the whole organism" might, as he says (p. 61), "more than compensate for the disadvantages which follow from the disappearance of single cells."

But granting all this, how did such a process begin? Some Metazoon must have been the first to die through this failure of reproduction in its component tissue-cells. Yet if the Protozoa were, and are (as Prof. Weismann represents), naturally immortal, the first Metazoa must have been entirely composed of immortal cells, and therefore themselves potentially immortal. Granted that cell-aggregations become every now and then accidentally dissolved, that would be "accidental death." Why should natural death arise, and, if it did, what advantage could ensue from the failure of cell-reproduction? It could not benefit the race, because as yet there was no race, but only individual clusters of naturally immortal cells which had happened to divide imperfectly. The Professor tells us (p. 29) it is "conceivable that all cells may possess the power of refusing to absorb nutriment, and therefore of ceasing to undergo further division." But how and why should a cell begin, for the very first time, to practice this abstinence? That it should do so, is, of course, like many other things "conceivable," but to my judgment it does not appear credible. Of course when once we have a race of mortal organisms propagating by germ cells, it is easy enough to understand how such a race would be benefited by the death of the "useless mouths" belonging to it, and therefore by the cessation of the tissue-reproduction which leads to such death. The difficulty lies in the natural death of the very first Metazoa which ever lived. Here, as in so many cases, it is "the first step" which tries us. How, from this perennial race of microscopic immortals, are we to obtain our first Metazoon *naturally* mortal?

By the hypothesis, each component cell consists of a form of protoplasm which has the power of growing and dividing. It is not easy to see how the mere coalescence of such cells can lead any one, or any set, of such cells to acquire an altogether new power—that of reproducing the whole complex organism of which it has come to be a part? The Professor tells us (p. 27) that probably "these units soon lost their primitive homogeneity. As the result of mere relative position, some of the cells were especially fitted to provide for the nutrition of the colony, while others undertook the work of reproduction." Referring to *Magosphæra planula*, he says (p. 75):—"Division of labour would produce a differentiation of the single cells in such a colony: thus certain cells would be set apart for obtaining food and for locomotion, while certain other cells would be exclusively reproductive." But how can the fact of a cell happening to fall into a position "especially fitted" for the performance of a certain function, lead to its performing this function? Supposing that the physical influences of the environment have modified the arrangement, or cohesion, size, or number of molecules in a cell, or modified their molecular motions, how can such influences give it a power, not of reproducing its thus "acquired" characters, or the characters of the cell before it becomes thus differentiated, but of reproducing the whole organism whereof it forms a part? Is it credible that any impacts and reactions thus occasioned should produce so marvellous a result? I do not know any phenomena in Nature which could warrant us in entertaining such a belief.

Of course, if we were dealing with races of creatures sexually reproduced, it is conceivable enough that, out of multitudinous, indefinite, minute accidental changes in the arrangements of the molecules of their germs, favourable arrangements might be selected in the struggle for life. But we are here concerned with nothing of the kind, but with the first appearance of the earliest Metazoa reproduced. If we meditate on the conditions affirmed by the Professor to have produced that origin, it will, I think, be clear that no hypothesis suggested by him will answer the question how any of the cells of the first coherent colonies came to reproduce, not such cells as their ancestors (or, rather, the earlier living portions of their very selves) had by countless processes of fission produced, but a whole "cell-colony," such as that whereof they had, by the hypothesis, for the first time come to form a part.

With respect to the immortality of Monoplastides and the question of death generally, he (the Professor) makes various remarks which do not appear to be satisfactory. The process of spontaneous fission, he says (p. 25), "cannot be truly called death. . . Nothing dies, the body of the animal only divides into two similar parts possessing the same constitution." Where such a perfect similarity exists we may say not only that there is no death, but also that there is no birth. In some of the Monoplastides, however, the relationship between parent and offspring does exist, but this, of course, need not necessarily involve death; as we see in higher species and in our own. But the fact that death does not take place during, or soon after, fission, does not prove that death never naturally occurs at all, and that the cell can balance its metabolism indefinitely. Very likely it may be able so to do, but this can hardly be affirmed to be an absolute certainty. What may be certainly affirmed is that reproduction by fission does not entail death to the degree that sexual reproduction entails it. But reproduction by gemmation may equally fail to entail death; as we see in the parthenogenetic Aphis and many Hydrozoa.

In *Euglypha* we can, as Prof. Weismann admits (p. 64), recognize the daughter cell (which is for a time without a nucleus, and we also find a very marked distinction between the segments of transversely dividing Infusorians; where one has to form a new mouth and the other a new anus.

After all that can be urged, then, in contrasting the multiplication by fission of Monoplastides with reproduction in the life-cycle of Polyplastides, there seems to me to be more of a true reproductive process in the former than the Professor is disposed to allow. In some *Heliozoa* and *Ciliata* we have all the complexity of indirect nucleus division by karyokinesis, while in *Euglypha* we have cell division without any antecedent separation of the nucleus into two parts. Of course it is easy enough to understand how a mere augmentation in bulk may overcome cohesion, how internal molecular arrangement may cause cleavage along definite lines, and, perhaps, even how such cleavage

may be insured through an increase of mass in proportion to a relatively diminishing surface nutrition. But such a division would be much simpler than a process of karyokinesis, and certainly than the formation of a new mouth and a new anus. Here there is no question of a part (p. 73) growing "to resemble the whole," comparable to the regrowth, by crystallization, to replace a fragment broken from a crystal. We have a whole which divides itself in such a way as to initiate and carry out a progressively increasing *difference*—a difference between the two parts dividing, and a difference (but a different kind of difference) between each such part and the previously existing whole.

Passing from the consideration of the immortality of Monoplastides to the mortality of Polyplastides, I cannot see my way to accept the Professor's definition (p. 114) of death: "An arrest of life, from which no lengthened revival, either of the whole or any of its parts, can take place," nor can I agree to his assertion (*loc. cit.*) that death "depends upon the fact that the death of the cells and tissues follows upon the cessation of the vital functions as a whole." If we cut up a *Begonia* plant or a *Hydra* into small parts, such an individual *Hydra* or *Begonia* cannot surely be considered as still alive, because fresh *Hydræ* or *Begoniæ* may spring from such fragments. Similarly with higher organisms, it would be preposterous to say that a man was not dead because a *post-mortem*, inferior kind of life—such as can alone be manifested in very lowly structures—was still persisting in the cells of his tissues!

No doubt, as the Professor says, we cannot have death without a corpse, but the tissues and cells of the corpse may still retain a certain sort of life without the corpse being any the less a corpse on account of that circumstance.

But if life of some sort may be, as we agree, affirmed of such cells, can we deny it absolutely (since no one comprehends it) even to the molecules of the cells? But body-tissues of lower Vertebrates may retain such life for a very long time. If, then, such a Vertebrate be devoured by another animal, who would venture to affirm that it is impossible that some of the micellæ or tagmata, or at least the molecules of some of the cells of the creature devoured may not pass, while still retaining a sort of life, into the tissues of the devourer? Even tagmata must be small enough to traverse the tissues, and can the possibility that they may enter into their composition while still living be dogmatically denied? May we not affirm the certainty of the death of the animal devoured till we are sure of the impossibility of the survival of any of the molecules of its cells?

No doubt the Professor would refer us to *Magosphæra* as presenting phenomena (so far as regards its cells) which support his view. He says (p. 126):—"The dissolution of a cell colony, with its component living elements, can only be death in the most figurative sense, and can have nothing to do with the real death of the individuals; it only consists of a change from a higher to a lower stage of individuality. . . . Nothing concrete dies in the dissolution of *Magosphæra*; there is no death of a cell colony, but only of a conception." But surely it cannot be the same thing "to exist in a coherent interrelated mass bound together by a common jelly," and "to exist in separate parts, living independently without interrelations, and not bound together by a common jelly." If there is here "death of a conception," there must be an external objective death corresponding therewith. *Magosphæra* is a very lowly organism, and its life can be very little better than that of a Monoplastid, because its structure is very little more complex. It is not wonderful, then, that there is very little difference between its existence and the existence of its *post-mortem* surviving cells. Yet the difference must be allowed to be, however diverse in degree, like that in the higher animals. Let us suppose that half a dozen higher animals could be so divided that no two cells remained in contiguity, yet that every cell could retain a *post-mortem* life such that by reuniting they could build up other individuals. Would it be reasonable to affirm that the higher animals thus segmented had not been *killed*, or that when their cells had reunited—possibly in very different combinations—the individual animals were the same ones as before? An extreme illustration often best seems to bring out the force and significance of a principle.

The *Orthonectides*, referred to (p. 126) by the Professor in controversy with Götte, hardly illustrate the question here discussed, but we note with much interest and satisfaction that he is inclined to regard them as arrested larvæ, Leuckart having found them[1] greatly to resemble the new-born young of *Distoma*, as Gegenbaur has found that the Dicyemids are like a stage in the development of the Platyhelminthes. If this interpretation is, as it probably is, correct, we have here an interesting example of what we find in such *Batrachians* as *Axolotl* and *Triton alpestris*. I am inclined to look at *Menobranchus*, *Proteus*, and *Siren* as larval forms which have now altogether ceased to assume what was once the adult stage of their existence.[2]

Prof. Weismann's hypothesis concerning heredity is certainly the best which has yet been proposed, but I have not met with any reference to that proposed by Sir Richard Owen forty years ago.[3] It is now out of date, and his references are not of course expressly to "germ-plasm," but to the contents of germ-cells. Nevertheless, there is an undeniable resemblance between the two hypotheses, and any interested in Prof. Weismann's would do well to read over Owen's small volume on the same problem.

But the complexity of Prof. Weismann's hypothesis is such as to approach, if it does not even exceed, that of pangenesis itself.

He tells us (p. 191): "Every detail of the whole organism must be represented in the germ-plasm by its own special and peculiar arrangement of the groups of molecules," and (p. 146) that "the number of generations of somatic cells which can succeed one another in the course of a single life, is predetermined in the germ." Moreover none of these circumstances can be explained by any difference of quality,[4] but must be exclusively due to the size, number, and arrangement of the component parts. Now, if we consider what must be the complexity of conditions requisite to determine once for all in the germ the precise number of all the succeeding cells of epithelial tissue, including every one of the rapidly succeeding cells of glandular epithelium, and every blood corpuscle of the whole of life; to necessitate also every modification of structure which may successively appear in polymorphic organisms, which change again and again profoundly between the egg and the imago; to arrange, at starting, the successive very complex changes of arrangement which must be necessary to build up reflex mechanisms

1 "Zur Entwicklungsgeschichte des Leberegels," *Zool. Anzeiger*, 1881, p. 99.

2 In this connection may be noted a passage which occurs on p. 266 of Prof. A. C. Haddon's excellent introduction to the study of embryology. Sollas is there quoted as saying that a longer mature life is possessed by those forms which are "saved from the drudgery of a larval existence." It would be interesting to know whether *Rana opisthodon* is longer lived than its congeners, since it has no tadpole stage of life.

3 See his work "On Parthenogenesis" (Van Voorst, 1849). There we read:—"Not all the progeny of the primary impregnated germ-cell are required for the formation of the body in all animals. Certain of its derivative germ-cells may remain unchanged and become included in the body which has been composed of their metamorphosed and diversely combined or confluent brethren; so included, any derivative germ-cell *or the nucleus* of such may commence and repeat the same processes," &c. (p. 5). At p. 68 he speaks of "the retention of some of the primary germ-vesicles." Finally, on p. 72, he says:—"How the retained spermatic force operates in the formation of a new germ-process from a secondary, tertiary, or quaternary derivative germ-cell or *nucleus*, I do not profess to explain; neither is it known how it operates in developing the primary germ-mass from the impregnated germ-vesicle of the ovum. In both we witness centres of repulsion and of attraction antagonizing to produce a definite result."

4 P. 101, where the existence of "quality" is denied.

capable, not only of compelling complex instinctive actions occurring at one time of life, but of so successively changing as to be able successively to make necessary the successively occurring very different instinctive actions of different periods of life, as *e.g.* in *Sitaris*. But this is by no means all. The arrangement of the molecules must be such as not only to effect all this, but also all the constitutional pathological inherited modifications which are to arise at different periods of life, and all the capabilities of reaction upon stimuli of every cell, of every tissue, and every predisposition an organism may possess—"predisposition" and "capacity" being nothing more than names for a certain collocation of particles so built up as inevitably to fall down into other collocations—upon shock and impact—the original collocation again being such as to insure not only that the first ensuing collocation from impact shall be of an appropriately definite kind, but that its definiteness shall be such as to insure that all the succeeding varied collocations from successive impacts shall also be appropriately definite. I confess I do not believe that such a collocation of particles is possible.[1]

This, however, is, after all, only a portion of the difficulty from complication, necessarily involved in Prof. Weismann's hypothesis of germ-plasm. For we have to consider the modifying effect on the germ-plasm produced by its effecting those developmental changes which it is its own business to effect. After speaking of the great complexity of the germ-plasm in higher animals, he goes on (p. 191) to say :—"This complexity must gradually diminish during ontogeny, as the structures still to be formed from any cell, and therefore represented in the molecular constitution of the nucleoplasm, become less in numbers; . . . the complexity of the molecular structure decreases as the potentiality for further development also decreases, such potentiality being represented in the molecular structure of the nucleus."

According to the hypothesis, the whole organism at every stage of its existence is but a collocation of molecules of different sizes most complexly arranged. Amongst them, during development, are the portions of germ-plasm, everywhere building up the increasingly complex structures of the developing body, while they themselves are simultaneously decreasing in complexity of composition. Now, it seems somewhat difficult to conceive of such a mass, which may thus be said to both decrease and increase simultaneously in complexity, both centripetally and centrifugally, and yet to preserve its complexity both centrally and sporadically, as must be the case in order to effect sexual reproduction and such repair of tissues after injury, as the organism may be capable of. Prof. Weismann continues :—"The development of the nucleoplasm during ontogeny may be, to some extent, compared to an army composed of corps which are made up of divisions, and these of brigades, and so on. The whole army may be taken to represent the nuceloplasm of the germ-cell : the earliest cell-division (as into the first cells of the ectoderm and endoderm) may be represented by the separation of the two corps, similarly formed, but with different duties : and the following cell-divisions by the successive detachment of divisions, brigades, regiments, battalions, companies, &c. ; and as the groups become simpler so does their sphere of action become limited. It must be admitted that this metaphor is imperfect in two respects : first, because the quantity of the nucleoplasm is not diminished, but only its complexity ; and, secondly, because the strength of an army chiefly depends upon its numbers, not on the complexity of its constitution." A better illustration of the Professor's conception would seem to be that of an army very complexly organized sending off successively regiments of different kinds, but always retaining in the centre a few men of all arms, and always being recruited by rustics (the food of the germ-plasm), who become organized by the central reserve of all arms retained for that purpose.

But how, according to this or any other conceivable illustration, are we to understand the germ-plasm becoming simplified by forming tissues and organs, and then regaining its complexity so as to be able to effect the various reparative growths which constantly take place after non-fatal injuries? Or if we are to deem that the germ-plasm only regains a portion of its complexity—one portion in one place, another in another—how can we conceive of the germ-plasm being so divided that each part of the body has just that portion of germ-plasm which is needed for its reproduction, in spite of that being the very portion which we might expect to have been exhausted, since it is it which has built up that part of the body.

Moreover, all these processes of succession, progression, simplification, and possible recomplication, of the germ-plasm itself, must, according to the hypothesis, have been laid down and necessitated in the first original collocation of the molecules of the germ. This seems to me to exceed the bounds of credibility.[1]

But if the hypothesis of germ-plasm be deemed one involving too much complexity for belief—that is, if the conditions supposed by it are deemed inadequate to explain the results of sexual ontogeny—the hypothesis seems yet more unsatisfactory with respect to processes of reparative growth and reproduction by gemmation. This is a subject the Professor has not yet expressly treated, and therefore some suggestions with respect to its difficulties may be welcome to him, as showing what elucidations some minds seem to require. He, however, tells us (pp. 197, 211, and 322) that such processes of growth are due to the presence of germ-plasm, and of course not so to hold would be to abandon his hypothesis. It is, however, difficult to understand how we can thus account for the reproduction of a human elbow with a joint structurally and functionally much as the old one (see "On Truth," pp. 170–171). Are we to understand that germ-plasm in all its complexity was there? If so, is it universally diffused through the organism as well as present in the sexual glands, and why does it not produce rather an embryo than an elbow-joint? If *not*, how comes it that the germ-plasm present happened to have the complexity needed to effect that which was, anatomically and physiologically, effected? With respect to germination generally, the Professor says (p. 322) :—"The germ-plasm which passes on into a budding individual, consists, not only of the unchanged idioplasm of the first ontogenetic stage (germ-plasm), but of this substance altered so far as to correspond with the altered structure of the individual which arises from it, viz. the rootless shoot which springs from the stem or branches. The alteration must be very slight, and perhaps quite insignificant, for it is possible that the difference between the secondary shoots and the primary plant may chiefly depend upon the changed conditions of development,[2] which takes place beneath the earth in the latter case and in the tissues of the plant in the former."

[1] Prof. Weismann sees clearly enough the fatal complexity of the parallel hypothesis of Nägeli, who would explain all this by "conditions of tension and movement." "How many different conditions of tension," our author remarks (p. 182), "ought to be possessed by one and the same idioplasm, in order to correspond to the thousand different structures and differentiations of cells in one of the higher organisms? In fact, it would be hardly possible to form even an approximate conception of an explanation based upon mere conditions of tension and movement."

[1] The term "Zielstrebig," as one used to denote a practically teleological process which is not really teleological, is a remarkable example of the mode in which we are led to regard the invention of a new name as an *explanation*.

[2] The remarkable readiness with which the fertile mind of Prof. Weismann excogitates hypotheses on hypotheses to explain away difficulties is rather remarkably shown by the way in which he tries to obviate the objection to his view as to parthenogenesis, which arises from the fact that in the bee the same egg will develop into a drone or not, according as it has or has not been fertilized. This would seem to emphatically contradict his doctrine, that the one cause of parthenogenesis is the greater amount of germ-plasm which exists in parthenogenetic eggs than in ordinary ones. He meets this by suggesting (p. 237) that if the spermatozoon reaches the egg it may, under the stimulus of *internal causes, grow to double its size*, thus obtaining the dimensions of the segmentation nucleus." What may *not* be thus explained?

Surely this is a very inadequate and even misleading statement of the matter. It is surely inconceivable that a portion of protoplasm should be affected in these diverse but most definitely diverse ways by the environment of earth and plant-tissues respectively. The radicle and plumule are formed (*e.g.* in the bean) while still surrounded by the tissues of the parent plant, but no radicle is formed in a growth by gemmation. Even if in all cases a radicle was formed, which radicle became largely developed under the stimulus of earth-environment, it would be difficult to understand why it should atrophy or metamorphose itself within those very plant-tissues under the influence of which it was itself first formed.

Again, as regards the Begonia leaf, if it is such germ-plasm as Prof. Weismann conceives of, which determines the development of such a leaf into a plant, what can be supposed to make it different from the germ-plasm of the seed? However complex may be the germ-plasm of Begonia, it must be a definite complexity. The germ-plasm cannot be simultaneously built up in two different ways. But a molecular arrangement which compels growth from a seed cannot possibly be the same as a molecular arrangement which compels growth from a leaf. The initial stages of the two processes are quite different.

Certainly the influence of the environment is sometimes very surprising; but these surprising results hardly, at least at first sight, seem to harmonize with Prof. Weismann's views. Thus the effect of the movements of the young of *Cynips*, newly hatched from an egg deposited in the tissues of a plant (p. 302), is to cause it to produce a gall—a result "advantageous to the larva but not to the plant." It causes "an active growth of cells" around the larva, much to that larva's advantage. Now surely it is too much to ask us to believe that the germ-plasm of the plant, in the first instance, before even, say, a single *Cynips* had visited it, had in the complex collocation of its molecules, an arrangement such as would compel the plant which was to grow from it, to grow these cells and form a gall as just mentioned.[1] However this may be, the production of the gall is certainly a curious effect of the action of the environment on an outgrowth from germ-plasm, conceived of as Prof. Weismann conceives of it.

But the question of the actual or possible influence of the environment suggests some further difficulties which can hardly fail to occur to any critical reader of what Prof. Weismann says concerning the inheritance of acquired characters. Although he absolutely denies that changes induced in the *soma* by the action of the environment, can be transmitted to a succeeding generation, he yet allows (p. 98) that the germ-plasm itself may be modified through the action of the environment on the *soma* increasing its nutrition, and such modifications, on his hypothesis, would be inherited. But if it is true, as stated, that oysters transported to the Mediterranean become rapidly modified, that the *Saturnia* imported to Switzerland from Texas become modified so as to transmit new characters in one generation, and that cats in Mombas, turkeys in India, and greyhounds in Mexico, have also been modified, their modifications being transmissible, it is very difficult to understand how such changed climatic conditions, or increased or diminished nutrition, could change the molecular structure of the germ-plasm in such a way as to compel the production in a second generation of modifications either so induced in the *soma* of the first, or of a nature appropriate to the conditions presented by a changed environment.

That the wild pansy does not change at once when planted in garden soil, and yet in the course of generations gains new characters which are propagated by seed, he explains (p. 433) by a modification of germ-plasm thus induced. But such an admission is enough to satisfy much of what is demanded by those who assert the inheritance of acquired characters. After all, such an inheritance must be due to the *soma*, since it is only through it that the germ-plasm can be modified.

If this effect on the germ-plasm itself is thus cumulative, may it not be partly due to a cumulative effect on the soma which transmits to the germ-plasm the actions which modify the latter? Can this be declared to be absolutely impossible? Anyhow, it is plain that effects of the environment on Polyplastides may be transmitted to succeeding generations. There are, however, still more striking phenomena amongst mammals which do not seem to accord with Prof. Weismann's theories. I refer to the production of offspring which resemble not their father, but the father of preceding offspring—as in the well-known case of Lord Zetland's brood mare, and the puppies of thoroughbred bitches which have once been coupled with a mongrel. How can the germ-plasm of the first father have been acquired by the offspring of a subsequent father? I have ventured to propose these questions, which must of course have occurred to many other naturalists, feeling sure that Prof. Weismann will be glad to have his attention drawn to a few points, a further explanation of which seems necessary for the acceptance of his most interesting hypotheses.

September 2. ST. GEORGE MIVART.

[1] It would be very interesting to know how "natural selection" (to the action of which, as everybody knows, Prof. Weismann constantly appeals) could have caused this plant to perform actions which, if not self-sacrificing (and there must be some expenditure of energy), are at least so disinterested. No doubt the Professor has an hypothesis to produce, though he only says (p. 302) here that "it would be out of place to discuss here the question."

NOTES.

THE Medals of the Royal Society have this year been awarded as follows:—The Copley Medal to the Rev. Dr. Salmon, F.R.S., for his various papers on subjects of pure mathematics, and for the valuable mathematical treatises of which he is the author; a Royal Medal to Dr. W. H. Gaskell, F.R.S., for his researches in cardiac physiology, and his important discoveries in the anatomy and physiology of the sympathetic nervous system; a Royal Medal to Prof. Thorpe, F.R.S., for his researches on fluorine compounds, and his determination of the atomic weights of titanium and gold; and the Davy Medal to Dr. W. H. Perkin, F.R.S., for his researches on magnetic rotation in relation to chemical constitution. Intimation has been received at the offices of the Royal Society that the Queen approves the award of the Royal Medals.

WE regret to learn that another officer of the Geological Survey of India has fallen a victim to the Indian climate. Mr. E. J. Jones, who joined the Survey in 1883, died of dysentery at Darjiling on October 15, at the age of thirty. Mr. Jones was an Associate of the Royal School of Mines, and having also studied chemistry at Zürich and Würzburg, he was a valuable member of the Survey, to the publications of which he contributed several geological and chemical papers.

To add to the many obligations under which he has laid Cambridge University, Prof. Sidgwick has offered to give £1500 towards the completion of the new buildings urgently required for physiology, on condition that the work is undertaken forthwith. The Financial Board has accordingly recommended a scheme by which this can be effected. The alliance between mental science and physiology which this gift represents is a bright feature of Cambridge studies at present.

THE University of St. Andrews is to be congratulated on an extraordinary piece of good fortune. The sum of £100,000 has been bequeathed to it by Mr. David Berry, who died last September. Mr. Berry was a native of Cupar, Fife, and in 1836 went to Australia, where he ultimately inherited the estate of his brother, Dr. Alexander Berry. The latter had been a

student of the St. Andrews University, and at the time of his death it was understood that he had left an unsigned will bequeathing a quarter of a million to his *alma mater*, but giving permission to his brother David to carry out the provisions as he might think proper. The legacy will not come into the possession of the University until 1894.

IN addition to the botanical appointments named last week, the following are announced from Russia:—Prof. Faraintzin having resigned his post of Professor of Botany in the University of St. Petersburg. Prof. Borodin has been appointed in his place. M. W. Palladin succeeds the late Prof. Pitra as Professor of Botanical Anatomy and Physiology in the University of Charkow; and is himself succeeded in the Botanical Chair in the Agricultural Academy at Nowo-Alexandria by M. Chmielewski. M. W. Rothert has been appointed Lecturer on Botanical Anatomy and Physiology at the University of Kasan.

IN the November number of the *Kew Bulletin* a curious correspondence is printed which illustrates very well the nature of some of the duties undertaken by the Kew officials. Towards the end of December 1876, Dr. Hooker received from the Colonial Office a letter inclosing a despatch in which the Governor of Labuan suggested that it might be well to promote in Labuan the cultivation of the African oil palm. A long correspondence followed, the result of which was that full and accurate information as to the palm oil industry was obtained from the Gold Coast, and transmitted to Labuan. Palm oil nuts were also obtained, and in due time planted in the fertile island of Daat, where no fewer than 700 healthy trees were soon raised. It recently occurred to Mr. Thiselton Dyer to make inquiry as to the later history of this interesting experiment. A despatch from the Acting Governor of Labuan to the Colonial Office, dated August 1, 1889, and forwarded to Kew, closes the correspondence. It is as follows:—"As reported in Mr. Treacher's despatch No. 72, of August, 26, 1878, it appears that 700 of these palms were raised in the island of Daat, and in due time produced nuts. No attempt, as far as I am aware, was ever made to manufacture any oil from the nuts, and last year the palms were all removed to make room for cocoa-nut trees. Daat, a dependency of this colony, is private property, and I venture to suggest that, should any further information be required by Mr. Thiselton Dyer, he should apply to the owner, Dr. Peter Leys, who is now in England, and who would no doubt be glad to supply it. The experiment, so far as I am in a position to judge, was a success."

THE authorities of the Royal Gardens, Kew, are always glad to aid any dependency of the Empire in introducing and establishing any new plant which promises to serve as the foundation of a new industry. The documents relating to the oil palm in Labuan show how much work may be involved in the carrying out even of a simple scheme of this nature, and how disappointing the results may be. "The enterprise," says the *Bulletin*, "is suggested; it is considered; a plan for carrying it out has to be matured; all the necessary incidental information has to be collected; and then the plan is carried into execution. Sometimes it fails the first time, and then a second attempt has to be made, and so on till success is secured. All that then remains is to wait for the result; and this, in any appreciable shape, will in most cases not be reached for years. But in the interval Governors and officials change. It may be, though it is not always so, that the ardour with which the experiment was launched evaporates with the individual whom it inspired. A new Colonial Government *régime* may regard with apathy and even hostility the work of its predecessor, and the whole enterprise may fail into oblivion till some chance inquiry on the same subject leads to the digging out of the file of papers containing its record from the Kew archives."

THE remaining contents of the *Kew Bulletin* relate to Phylloxera regulations at the Cape, Ramie or Rhea, and the collecting and preserving of fleshy Fungi.

THE Manchester Field Naturalists' Society has formed a special committee, with Mr. Leo Grindon, the President of the Society, as botanical referee, and Mr. C. J. Oglesby, as convener, for the purpose of determining which trees, shrubs, and flowers will succeed in the squares and streets of the city. The opinion prevails that, notwithstanding the unfavourable climatic conditions, several forest trees, climbers, and hardy plants would grow if special care were taken in planting and tending them. The planting of the quadrangle at Owens College, of the infirmary esplanade (in the centre of the town), and of several churchyards, has been attended with success.

THE following money-grants have been lately made by the Berlin Academy of Sciences:—£75 to Prof. Brieger, for continuation of his researches on the ptomaïnes; £60 to Dr. Krabbe, for investigation of the Cladoniaceæ of the Hartz; £30 to Dr. von Dankelmann, for utilization of meteorological observations at Finschhaven in New Guinea; £20 to Dr. Assmann, for measurements of air-temperature on the Säntis; £100 for publication of Prof. G. Finsch's work on Torpedineæ; £50 for publication of a memoir by Dr. Heiden, on the development of *Hydrophilus piceus*; £100 to Dr. Strehlmann, in Zanzibar, for prosecution of his faunistic researches in East Africa; £125 to Prof. Lepsius, of Darmstadt, for preparation of his geological map of Attica; £50 to Prof. Conwentz, for investigation of silicified wood in the island of Schonen; £75 to Dr. Fleischmann, of Erlangen, for researches in development; and the same to Dr. Zacharias (Silesia), for micro-faunistic studies.

THE first meeting of the one hundred and thirty-sixth session of the Society of Arts will be held on Wednesday, November 20, when the opening address will be delivered by the Duke of Abercorn, Chairman of the Council. Before Christmas there will be four ordinary meetings, in addition to the opening meeting. The following arrangements have been made:—November 27, Dr. J. Hall Gladstone, F.R.S., "Scientific and Technical Instruction in Elementary Schools"; December 4, Dr. Armand Ruffer, "Rabies and its Prevention"; December 11, Mr. H. Trueman Wood, "The Paris Exhibition"; December 18, Sir Robert Rawlinson, "London Sewage."

A NOVEL and interesting application of science to art may now be seen at the Arts and Crafts Exhibition, where Mrs. Watts Hughes shows specimens of what she calls "voice figures" (Catalogue, No. 723). These are practically Chladni's figures produced in a viscid medium. Semi-fluid paste is spread on an elastic membrane stretched over the mouth of a receiver. A single note "steadily and accurately sung" into the receiver throws the paste into waves and curves. The patterns formed are either photographed immediately after production, or are transferred as water-colour impressions while the membrane is still vibrating. Fanciful names, *e.g.* "wave, line, flower, tree, fern," are given to these; the effect, especially in transparencies, is very beautiful. Some of the forms would repay the study of physicists as well as of artists; the most interesting are perhaps the "daisy forms," in which we are told that "the number of petals increases as the pitch of the note which produces them rises." The apparatus employed is not exhibited, and the descriptive label is not very clear, but we understand that Mrs. Hughes would be most pleased to explain the matter to anyone scientifically interested in it: her address is 19 Barnsbury Park, N.

For determination of the air-temperature at great heights, the Berlin Society for Ballooning (we learn from *Humboldt*) is going to try a method of Herr Siegsfeld, who uses a thermometer, which, by closure of an electric circuit when certain temperatures are reached, gives a light-signal. Small balloons, each containing such a thermometer, will be sent up by night, and the light will affect photographically a so-called "photo-theodolite," while the height then attained will be indicated in a mechanical way. It is hoped that more exact formulæ for the decrease of temperature with height may thus be obtained.

The rapid decrease in the number of kangaroos is beginning to attract the attention of scientific Societies in Australia. From the collective reports of the various stock inspectors it was estimated that in 1887 there were 1,881,510 kangaroos. In 1888 the number fell to 1,170,380, a decrease of 711,130. The chief obstacle to the adoption of measures for the effectual protection of the kangaroo is his vigorous appetite. One full-grown kangaroo eats as much grass as six sheep; and graziers—who as a class are not, it is to be feared, readily accessible to the influence of sentiment—find that the food eaten by this interesting animal might be more profitably utilized otherwise. In a communication on the subject, lately submitted to the Linnean Society of New South Wales, Mr. Trebeck suggested that the National Park might be used for the preservation not only of kangaroos but of very many members of the Australian fauna and flora.

At the monthly meeting of the Royal Society of Tasmania on September 9, the President (His Excellency Sir Robert G. C. Hamilton) said he desired to bring before the Society a matter relating to the young salmon at the Salmon Ponds. These were the undoubted product of the ova brought out by Sir Thomas Brady, which had been stripped from the male and female fish and artificially fertilized, and the utmost care had been taken to keep them apart from any other fish bred in the ponds. He recently visited the ponds, accompanied by the Chairman of the Fisheries Board, the Secretary, and two of the members, when they carefully examined a number of the young salmon, among which they were surprised to find marked differences existing, not only in size, but in their characteristics. It has often been held that the *Salmonidæ* caught in Tasmanian waters cannot be true *Salmo salar* because so many of them have spots on the dorsal fin, and a tinge of yellow or orange on the adipose fin, but nearly half of the young salmon they examined, which had never left the ponds, had these characteristics. Again, many of them were almost "bull-headed" in appearance—another characteristic which is not supposed to distinguish the true *Salmo salar*. He would suggest to the Chairman of the Fisheries Board, whom he saw present, that the Secretary should be asked to make a formal report of the result of this visit, and to obtain some specimens of the young fish, which could be preserved in spirits, and perhaps sent to Sir Thomas Brady to be submitted for the consideration and opinion of naturalists at home.

At the same meeting of the Tasmanian Royal Society, Mr. James Barnard read a remarkably interesting paper on the last living aboriginal of Tasmania. It has hitherto been generally believed that the aboriginal Tasmanians are extinct. Mr. Barnard, however, contends that there is still one survivor—Fanny Cochrane Smith, of Port Cygnet, the mother of six sons and five daughters, all of whom are living. She is now about fifty-five years of age. Fanny's claims to the honour of being a pure representative of the ancient race have been disputed, but Mr. Barnard makes out a good case in her favour. He himself remembers her as she was forty years ago, when there were still about thirty or forty natives at Oyster Cove; "and certainly at that time," he says, "I never heard a doubt expressed of her not being a true aboriginal."

The Caucasus is a region of great interest in the study of prehistoric times, and a fresh impulse was lately given to its exploration, by Beyern's discovery of an extensive burial-ground south of Kura (in the district of the Anticaucasus). At the recent annual meeting of the German Anthropological Society, Dr. Virchow gave some account of this bed (which Beyern has named after General Repkin). The region is rich in ores, but bronze articles are absent; for, while copper is plentiful, there is no tin. On the other hand, various ornaments of pure antimony have been met with; also antimony buttons (or knobs), like those of Beni-Hassan in Egypt. The ground is largely of volcanic nature, and many articles of obsidian (chiefly knives and arrow-heads) have been found in the graves. One curious find was that of a skeleton having an arrow-head of obsidian in one of the leg-bones, partly overgrown by a callus. The metallic girdles in this burial-ground have figures of animals engraved on them; in the Koban ground, such figures are confined to the clasp, but this, in the Repkin ground, is wanting.

Prof. Edwin J. Houston contributes to the November number of the Journal of the Franklin Institute a short paper on a hail-storm at Philadelphia, October 1, 1889. After noting various points common to most hailstones, he refers to a characteristic which he had never before observed. "On some of the hailstones," he says, "though not in the majority of them, well-marked crystals of clear transparent ice projected from their outer surfaces for distances ranging from an eighth to a quarter of an inch. These crystals, as well as I could observe from the evanescent nature of the material, were hexagonal prisms with clearly-cut terminal facets. They resembled the projecting crystals that form so common a lining in geodic masses, in which they have formed by gradual crystallization from the mother-liquor. They differed, however, of course, in being on the outer surface of the spherules."

In *Das Wetter* for October, Dr. W. J. van Bebber discusses a paper, by the late Prof. Loomis, on the rainfall of the earth. The following are noted as some of the conditions favourable to rain: (1) an unsettled state of the atmosphere, caused by unusually high temperature, with great humidity, a condition which occurs when the pressure is below the average value; (2) cold northerly or westerly winds on the west side of a depression, by which the winds on the east side receive a stronger impulse; (3) proximity to mountains, the ocean or large lakes; (4) deep depressions of small area and steep gradients. With regard to the rainfall which accompanies barometric depressions, it is found that in the United States, south of latitude 36° N., a rainfall of 2·5 inches occurs oftener on the east side than on the west side of a depression in the ratio of 2·6 : 1; on the eastern side of the Rocky Mountains, a rainfall of 9 inches occurs more frequently on the east than on the west of a barometric minimum, in the ratio of 6·2 : 1. In the North Atlantic Ocean, the ratios of large rain areas on the east and west sides of a depression are as 2·6 : 1; while in Europe a rainfall of 2·5 inches in twenty-four hours on the east and west sides of a depression occurs in the ratio of 2 : 1. The rainfall with a falling or rising barometer is also investigated.

We have received the fifth and last part of vol. i. of M. Fabre's comprehensive "Traité Encyclopédique de Photographie" (Paris: Gauthier-Villars, 1889). The subject of lenses is considered in great detail, and the theory and use of diaphragms are fully gone into. The relation of the time of exposure to the subject and lens employed is also considered, and studios, dark rooms, and their various accessories are fully described and illustrated. From both the theoretical and practical point of view the work still bears out its original promise of becoming the most complete one on the subject.

A SECOND edition of Prof. Tait's "Light" (A. and C. Black) has been issued. The author says that in revising the work he has made use of various notes jotted down from time to time on his own copy, mainly as the result of questions asked, or of difficulties pointed out, by students who were reading the book with care. Suggestions of this kind he has found to be almost always of value, as they tend to make the book better suited to the wants of the class of readers for whom in particular it was designed.

PERSONS interested in ferneries and aquaria will find much to attract them in a little volume entitled "Ferneries and Aquaria: a Complete Guide to their Formation, Construction, and Management," by George Eggett, Sen. This is one of a series of "practical guide-books" issued by Messrs. Dean and Son.

THE third volume (new series) of the *Reliquary* (Bemrose and Sons) has been issued. It opens with an interesting illustrated article on two Assyro-Phœnician shields from Crete, by the Rev. Joseph Hirst. Mr. John Ward contributes three illustrated papers of scientific value—on Rains Cave, Longcliffe, Derbyshire; on relics of the Roman occupation, Little Chester, Derby; and on recent diggings at Harborough Rocks, Derbyshire.

MESSRS. DULAU AND CO. have sent us a "Catalogue of Zoological and Palæontological Works." It includes works on Reptilia and Amphibia, and on Pisces.

THE atomic weight of palladium has been redetermined by Dr. E. H. Keiser (*Amer. Chem. Journ.*). Among all the atomic weights at present adopted by chemists, that of palladium has been one of the most imperfectly determined, for the discrepancy between the results of the various previous investigations is most unsatisfactory. In 1826, Berzelius obtained the value 113·63 from a consideration of the proportion in which palladium combines with sulphur. Two years later, the same distinguished chemist derived a much lower value from analyses of potassium palladious chloride, $2KCl . PdCl_2$; known quantities of this salt were heated in a current of hydrogen, and the residuary potassium chloride and reduced palladium weighed. Recalculated by Profs. Meyer and Seubert, utilizing all the refined corrections of the present day, these analyses yield the value 106·2—a number which is almost identical with the atomic weight obtained by Dr. Keiser. In 1847, however, Quintus Icilius also investigated the subject, and, from determinations of the loss in weight which potassium palladious chloride undergoes when heated in a current of hydrogen, obtained the value 111·88. No other determinations having since been attempted, and the number 112 or 113 being certainly too high from considerations of the position of palladium among the metals, the number 106·2 obtained from Berzelius's second analysis recalculated by Meyer and Seubert has been universally adopted. To place the subject out of all doubt, Dr. Keiser has re-examined it from a totally different standpoint. The double chlorides of palladium and the alkalies, such as $2KCl . PdCl_2$ and $2NH_4Cl . PdCl_2$, are found to be unsuitable for atomic weight determinations; they retain water of decrepitation with great tenacity, and, after drying, are too hygroscopic for accurate weighing. On the other hand, the yellow crystalline salt, palladammonium chloride, $Pd(NH_3)_2Cl_2$, is a much more suitable substance. It is eminently stable, can be obtained in a state of practically perfect purity, contains no water of crystallization, does not retain water after drying in a desiccator, and the dried salt is not hygroscopic. Weighed quantities of it contained in a platinum boat were introduced into a combustion tube and heated in a stream of pure hydrogen. The hydrogen was rapidly absorbed, changing the bright yellow colour into black, metallic palladium and ammonium chloride being formed. The absorption of hydrogen occurred so readily that it was only necessary to warm one end of the boat when the heat of the reaction was found sufficient to complete the reduction of the whole. $Pd(NH_3)_2Cl_2 + H_2 = Pd + 2NH_4Cl$. After raising the temperature so as to volatilize the ammonium chloride, the finely divided palladium adhered together in the form of a porous bar having the shape of the boat. It was allowed to cool before weighing until just below a red heat in the current of hydrogen so as to prevent oxidation, and afterwards the hydrogen was displaced by dry air to prevent its occlusion. Two series of determinations were made, the salt for the second series being prepared from the reduced palladium of the first. The mean of eleven experiments in the first series gave the number 106·352, and of eight in the second series 106·350. The maximum value obtained was 106·459, and the minimum 106·286. The mean result 106·35 practically confirms that obtained by recalculating the results of Berzelius's second analyses.

IN our note in these columns three weeks ago (vol. xl. p. 655), upon pinol, the new isomer of camphor, it was pointed out that the nitrosochloride of pinol forms with β-naphthylamine an interesting base, $C_{20}H_{24}N_2O_2$, isomeric with quinine. This base, however, is not the first isomer of quinine which has been prepared, for an artificially prepared base of the same empirical formula was described by Dr. Kohn, of University College, Liverpool, in the Journal of the Chemical Society for 1886, p. 500.

THE additions to the Zoological Society's Gardens during the past week include three Rhesus Monkeys (*Macacus rhesus* ♂ ♂ ♂) from India, presented respectively by Colonel Cuthbert Larking, Mr. James T. Wilson, and Mrs. Charles Sainsbury; a Hairy-rumped Agouti (*Dasyprocta prymnolopha*) from Guiana, presented by Mr. Henry E. Blandford; a Common Polecat (*Mustela putorius*) from Norfolk, presented by the Earl of Romney; a Northern Mocking Bird (*Mimus polyglottis*) from North America, presented by Miss E. Breton; two White Pelicans (*Pelecanus onocrotalus*), a Crested Pelican (*Pelecanus crispus*) from Roumania, a Common Boa (*Boa constrictor*), a Neck-marked Snake (*Geoptyas collaris*) from Panama, a Mocassin Snake (*Tropidonotus fasciatus*) from North America, deposited; two Common Siskins (*Chrysomitris spinus*), two Twites (*Linota flavirostris*), two Lesser Redpoles (*Linota rufescens*), four Snow Buntings (*Plectrophanes nivalis*), two Knots (*Tringa canutus*), a Bar-tailed Godwit (*Limosa lapponica*), British, a Rosy-billed Duck (*Metopiana peposaca* ♂) from South America, purchased.

OUR ASTRONOMICAL COLUMN.

OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at 10 p.m. at Greenwich, November 14 = 1h. 36m. 45s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) { G. C. 385	—	—	1 35 30	+50 50
{ G. C. 386	—	—	1 36 29	+50 51·5
(2) 57 Ceti	6	Yellowish-red.	1 54 36	−21 16
(3) ζ Ceti	3	Yellow.	1 45 32	−10 55
(4) δ Cassiopeiæ ...	3	Bluish-white.	1 18 36	+59 40
(5) 7 Schj.	7·0	Reddish-yellow.	1 10 5	+25 11
(6) R Pegasi	Var.	Red.	23 1 7	+ 9 57
(7) V Tauri	Var.	Reddish.	4 45 50	+17 21

Remarks.

(1) This is one of Herschel's double nebulæ. Dr. Huggins notes that both components give a gaseous spectrum, but could only be certain of the presence of the chief nebula line near 500, although 495 was strongly suspected. He notes, also, that there

is a faint continuous spectrum at the preceding edge of No. 386. The point chiefly requiring attention at present is the character of the line near 500. Many recorded observations describe this line as having a fringe of light on the more refrangible side, whilst others state that it is perfectly sharp on both edges. Low dispersion only should be employed in making this observation. The observation of continuous spectrum in a special part of the nebula 386 is also worthy of attention; the spectrum should be examined for maxima of brightness, as in the case of the nebula in Andromeda.

(2) Dunér records this as a star of Group II. (see below), but states that the spectrum is very feebly developed. The star is probably, therefore, either just condensing into a fully-developed star of Group II., or is just passing into Group III. If the former, there will practically be nothing but very narrow bands, and if the latter, absorption *lines* will accompany the bands. In the earlier stages of this group, the bands in the blue are strongest, whilst in the later stages red bands are strongest, and this point should also receive attention. As a check, the colour of the star should be noted at the time of observation.

(3) This star belongs to either Group III. or to Group V., and the criteria (see p. 20) should be observed in order to determine which.

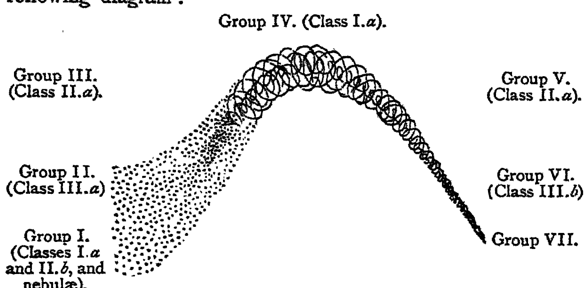
(4) According to Vogel, the spectrum of this star is of the same type as α Lyræ, *i.e.* Group IV. The relative intensities of the metallic lines and those of hydrogen, which vary from star to star, should be noted for future classification of the stars of this group according to temperature.

(5) This is a star of Group VI. Dunér describes the spectrum as consisting of four zones, the zones being the bright spaces between the dark carbon flutings. The presence of slight traces of carbon absorption in the solar spectrum indicates that stars of this group only differ in temperature from stars like the sun. The passage from one group to the other will probably be found to be very gradual, and the widths of the carbon flutings and the presence or absence of other absorptions should therefore be noted.

(6) Period given by Gore as 382 days, and magnitude at maximum (November 13) as 6·9–7·7. The spectrum has not yet been recorded, and the present maximum may, therefore, conveniently be taken advantage of.

(7) Period given by Gore as 168 days, and magnitude at maximum (November 15) as 8·3–9. Spectrum not yet recorded.

Note.—Lockyer's classification will, in future, be exclusively used, so that there will be no necessity for a double reference. The relation of this to Vogel's classification is shown in the following diagram:—



Lockyer's Temperature Curve.

The temperature increases from Group I. to Group IV., and then decreases to Group V. On the ascending side of the "temperature curve" we have probably to deal with condensing meteoritic swarms; and, on the descending side, with gradually condensing masses of meteoritic vapours.

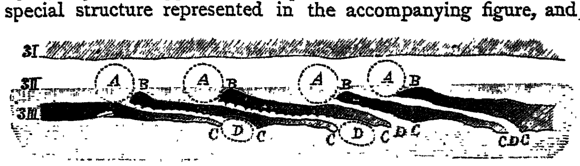
A. FOWLER.

LARGE-SCALE CHARTS OF THE CONSTELLATIONS.—Mr. Arthur Cottam has projected a series of thirty-six most excellent charts of the constellations from the North Pole to between 35° and 40° of south declination, and showing stars in half magnitudes down to 6½ by disks of various sizes. Although the primary object in constructing these charts was to make them companions to Webb's "Celestial Objects for Common Telescopes" and Smyth's "Cycle of Celestial Objects," their scope has been considerably enlarged, and a number of double, multiple, and variable stars have been laid down which are not included in either of the above-mentioned works. The Earl of Crawford's (Dun Echt) summary of F. G. W. Struve's Dorpat Catalogue included 2248 double and multiple stars, and of them, 2130 are shown upon these charts. In addition to this, 275 of the double stars discovered by Mr. S. W. Burnham have been mapped, this being the whole of those included in his first four catalogues, and a selection from his other catalogues. The maps have been drawn to a scale of one-third of an inch to a degree, which is a much larger scale than any hitherto published, and as each map includes but a small portion of the heavens, there is practically no distortion, whilst the epoch being 1890, the positions will hold good, without any serious errors, for fifteen or twenty years beyond that date. The projection is conical, or, in those charts which extend any distance both north and south of the equator, cylindrical. Hence it will be easy to lay down any additional objects that may be required. There is no doubt that these charts will be eminently useful, one of their great advantages being that they will enable possessors of telescopes mounted on altazimuth stands or without circles to find with ease a large number of interesting objects, and thus will help to extend the knowledge of the heavenly bodies and to popularize the most fascinating of sciences. We may say that the publisher of these charts is Edward Stanford, Cockspur Street, S.W., and that the first issue is limited to 200 sets, many of which have been already subscribed for.

BARNARD'S COMET, II. 1889, MARCH 31.—The following ephemeris is given in *Astronomische Nachrichten*, No. 2931:—

1889.	R.A. h. m. s.	Decl. ° '	1889.	R.A. h. m. s.	Decl. ° '
Nov. 6 ...	1 8 54 ...	−16 30·2	Nov. 22 ...	0 28 2 ...	−17 25·4
7 ...	5 49 ...	−16 37·2	23 ...	26 3 ...	−17 25·7
8 ...	2 49 ...	−16 43·6	24 ...	24 8 ...	−17 25·6
9 ...	0 59 53 ...	−16 49·5	25 ...	22 17 ...	−17 25·2
10 ...	57 1 ...	−16 54·9	26 ...	20 29 ..	−17 24·7
11 ...	54 13 ...	−16 59·8	27 ...	18 45 ...	−17 23·9
12 ...	51 29 ...	−17 4·1	28 ...	17 5 ...	−17 22·8
13 ...	48 50 ...	−17 8·1	29 ...	15 28 ...	−17 21·6
14 ...	46 15 ...	−17 11·6	30 ...	13 55 ...	−17 20·0
15 ...	43 44 ...	−17 14·8	Dec. 1 ...	12 25 ...	−17 18·3
16 ...	41 17 ...	−17 17·4	2 ...	10 58 ...	−17 16·3
17 ...	38 55 ...	−17 19·7	3 ...	9 34 ...	−17 14·3
18 ...	36 36 ...	−17 21·5	4 ...	8 13 ...	−17 12·0
19 ...	34 21 ...	−17 22·9	5 ...	6 56 ...	−17 9·7
20 ...	32 11 ...	−17 24·0	6 ...	5 41 ...	−17 7·1
21 ...	30 5 ...	−17 24·9	7 ...	4 29 ...	−17 4·4
22 ...	28 2 ...	−17 25·4	8 ...	3 20 ...	−17 1·5

THE STRUCTURE OF JUPITER'S BELT 3, III.—This dark band appears under ordinary conditions to be made up of two parallel bands, but Dr. Terby (*Astronomische Nachrichten*, No. 2928) says this appearance of parallelism is the result of the special structure represented in the accompanying figure, and



Structure of Jupiter.

that, therefore, the band 3, III., is composed of a lot of dark bands inclined in the same direction. The circular parts A are distinguished by Dr. Terby as emitting a sort of diffused light of an entirely different character from the white equatorial spots, properly so called; these luminous balls seem always to occur at the interval between two of the inclined bands, and touching what is generally their darkest part, B. The brilliant white spots D also appear at the dissolution of two successive bands, and occupy by preference their northern extremities. When the definition was very good, Dr. Terby observed that the interval between two of these fragmentary bands had the appearance of a series of globules, as shown in the figure. The structure appears so general and regular that it may be the means of adding considerably to our knowledge of the physical constitution of this planet.

GEOGRAPHICAL NOTES.

AT the first meeting of the session of the Royal Geographical Society, the paper was on Cyprus, by Lieut.-General Sir Robert Biddulph, G.C.M.G., C.B. The island of Cyprus is the third largest in the Mediterranean, being inferior in size only to Sicily and Sardinia. Its area is 3584 square miles. Its principal

features are two mountain ranges, running pretty well parallel to each other from east to west. The northernmost of these two ranges extends almost the whole length of the island from Cape Kormakiti on the north-west to Cape St. Andrea at the end of the horn-like promontory which stretches for 40 miles from the north-east of the island. This promontory is called the Carpas, and the low mountain chain running through it is called the Carpas range. The westernmost and higher portion of the northern range is called the Kyrenia range, and rises to an altitude of 3340 feet. This range is of a remarkably picturesque outline, in some parts extremely rugged. It is mostly a single ridge without any remarkable spurs, and its summit is about two miles from the northern coast. It can be crossed in many places. The chief mountain peaks of this range are Kornos, 3105 feet; Buffavento, 3140; and Pentedaktylos, 2400. The last named is a remarkably shaped rock in the centre of the Kyrenian range, owing its name to its shape, the word Pentedaktylos signifying in Greek "five-fingered." Beneath this rock there rushes out southward from the mountain side, at an altitude of 870 feet, a torrent of water, which never ceases to flow summer or winter, and which, descending into the great plain in the centre of the island, carries its fertilizing streams to the lands of several villages, its course marked by mills, gardens, and trees, until its water is exhausted by various irrigating channels. A similar stream of water gushes from the northern side, about 12 miles west of the Kyrenia Pass. Smaller streams descend on either side of the range at various places; their waters are used for irrigation in the valleys. The southern range of mountains is of a much more extensive nature than the northern range. The easternmost point of this range is the mountain of Santa Croce, so called from the church of the Holy Cross which stands on its summit. This mountain, which is 2260 feet in height, is of a peculiar shape. Beginning then from this point the southern range rapidly rises to considerable altitudes, finally culminating in Mount Troodos, the highest point in Cyprus, being 6406 feet above the sea-level. The other chief peaks in the southern range, are Adelphe, 5305 feet; and Machera, 4674 feet. But it is not only in altitude that the Troodos range is distinguished; numerous spurs run down to the north and south, and as we proceed further west these radiate out to greater distances, so that half way between Troodos and the sea, the mountain range is not less than 20 miles wide. Here there are very considerable forests, many miles in extent, rarely visited save by wandering flocks and by wood-cutters, and affording shelter to the moufflon, or wild sheep of Europe, some 200 or 300 of which still roam over these hills. On the map it will be seen that numerous rivers descend from both sides of the southern range. These are mostly dry in summer, but after rain their waters descend with violence, filling up the river-beds in the plains, carryiny away trees and cultivated patches, and often rushing in a turbid stream into the bays of Famagusta and Morphou. Between the two mountain ranges there lies a great plain called the Mesaorea, which is the most fertile part of Cyprus, growing large crops of wheat, barley, and cotton. It was evidently once the bottom of the sea, for in many parts are large beds of marine shells—gigantic oysters and others—all clustered in masses. A noticeable feature of this plain is the number of flat-topped plateaux of various sizes, where the rock seems to have resisted the action of the water. The tops of these plateaux are clothed with short herbage, affording a scanty provision for flocks, and are usually from 100 to 200 feet above the plain. The rivers which descend from the hills carry down large quantities of alluvial soil, and this forms in the eastern part of the Mesaorea a rich deposit, something similar to the Delta of the Nile. The two rivers which mainly contribute to this plain are the Pediæus and the Idalia, the former taking its rise from the northern slopes of Mount Machera, and the latter from the eastern slopes of the same mountain. The beds of these rivers have, however, become so choked up with alluvial deposit towards the end of their course, that their waters overflow the plain and mingle together, so that their separate mouths can with difficulty be distinguished. The normal condition of these rivers is to be without water, but whenever there is a heavy rainfall in the mountains, the river "comes down," as it is called, and runs for one, two, or more days. It occasionally happens that the water descends with great suddenness and violence, causing disastrous floods. Considerable supplies of water for irrigation purposes are obtained by sinking wells. A long chain of wells are sunk at distances of five or six yards apart, and being connected by underground galleries, a channel is thus formed which conveys the water to a reservoir constructed at the foot of the last well, and it is thence raised to the surface by a water-wheel; or in some cases the level of the ground admits of the channel being brought out on the surface. In this way the town of Nicosia is supplied with excellent water, which is brought in two aqueducts from a distance of some miles. Larnaca and Famagusta and other towns have similar aqueducts. Closely connected with the water supply is the forest question. Sir Robert Biddulph then entered into detail with reference to the denudation of Cyprus of its forests, and the great locust-plagues which have been so successfully treated since the British occupation.

THE FLORA OF CHINA.[1]

SINCE the last meeting of the British Association, two additional parts of the "Index Floræ Sinensis" have been published, bringing the enumeration of known, and the description of new, species as far as the *Loganiaceæ*. The Committee now, therefore, look forward with some confidence to the completion of their labours at no distant date.

Further extensive and valuable collections have been received from China in aid of the work, more especially from Dr. Augustine Henry, late of Ichang. The novelty and richness of the material obtained by this indefatigable botanist far exceeds any expectations the Committee could have formed. It is to be regretted that his duties as an officer of the Chinese Imperial Maritime Customs have necessitated his removal to Hainan. It is probable, however, that he had practically exhausted the immediate neighbourhood of Ichang, and that without opportunities of travelling over a wider radius, which the Committtee regret they were unable to procure for him, he would not have been able to add much of material novelty to the large collections already transmitted by him to Kew.

The Committee have met with the kindest sympathy and assistance in their labours from Dr. C. J. de Maximowicz, of the Académie Impériale of St. Petersburg, who has long been engaged on the elaboration of the collections made by Russian travellers in China, and from M. Franchet, of the Muséum d'Histoire Naturelle at Paris, who is describing and publishing the extremely rich collections made by the French missionaries in Yunnan.

The Committee have received striking proofs of the appreciation of their labours by botanists of all countries. They permit themselves to quote the following passage from a letter received early in the present year from Baron Richthofen, than whom no one is more competent to estimate the value of work connected with the scientific exploration of China:—

"It is of great value to have, now, a Flora of China, embodying all the species known from that country. You have evidently succeeded at Kew in getting a very complete collection. At the same time, in looking over the localities mentioned in the book, it strikes me that large portions of China are still unexplored botanically. There remains a splendid field for a good collector in the Tsingling Mountains, the province of Sz'chuen, and chiefly its elevated region west of Ching-tu-fu. Work in those parts will be greatly facilitated by the solid foundation laid through the work of Forbes and Hemsley."

The Committee derive an independent existence as a Sub-Committee of the Government Grant Committee of the Royal Society. They are at present in possession of sufficient funds to enable them to carry on the work. They do not therefore ask for their reappointment at the hands of the British Association.

SCIENTIFIC SERIALS.

American Journal of Science, October.—Assuming that the earth's crust rests on a layer of liquid as a floating body, Mr. Le Conte here offers an explanation of normal faults. The crust is supposed to be raised into an arch, by intumescence of the liquid, caused by steam or hydrostatic pressure; it is thus broken by long more or less parallel fissures into oblong prismatic

[1] Third Report of the Committee, consisting of Mr. Thiselton-Dyer (Secretary), Mr. Carruthers, Mr. Ball, Prof. Oliver and Mr. Forbes, appointed for the purpose of continuing the preparation of a Report on our present knowledge of the Flora of China.

blocks, which, on relief of the tension by escape of lava or vapour, are readjusted by gravity, in new positions. The blocks may be rectangular in section, but are more likely to be rhomboidal or wedge-shaped; giving level tables with fault cliffs (as in the plateau region) in the one case, and tilted blocks with normal faults (as in the basin region) in the other. The author considers the Sierra and Wahsatch to have been formed by lateral crushing and folding; and the region between to have been arched, broken, and readjusted, as described, in the end of the Tertiary.—Two determinations of the ratio of the electromagnetic to the electrostatic unit are furnished from the Johns Hopkins University; one made this year, by Mr. Rosa, by Maxwell's method of measuring a resistance, the other ten years ago, by Messrs. Rowland, Hall, and Fletcher, by measuring a quantity of electricity electrostatically, and then measuring it electromagnetically with a galvanometer. The former gives $v = 2{\cdot}9993 \times 10^{10}$ centimetres per second; the latter, $2{\cdot}9815 \times 10^{10}$ centimetres. It seems certain, according to Mr. Rosa, that v is within a tenth per cent. of 300 million metres per second.—Mr. Long continues his account of the circular polarization of certain tartrate solutions; and his experiments point to a law that the rotation of a double tartrate may be made to approach that of a neutral tartrate of either of the metals present, by addition of a salt of that metal (the effects being apparently explained by substitution).—Mr. Eldridge proposes a new grouping and nomenclature for the middle Cretaceous in America.—There are also papers on the gustatory organs of the American hare (Mr. Tuckerman); on the output of the non-condensing engine, as a function of speed and pressure (Mr. Nipher); and on some Florida Miocene (Mr. Langdon).

SOCIETIES AND ACADEMIES.

London.

Physical Society, November 1.—Prof. Reinold, F.R.S., President, in the chair.—The following communications were read:—On a new electric-radiation meter, by Mr. W. G. Gregory. The meter consists of a long fine platinum wire attached to a delicate magnifying spring of the Ayrton and Perry type, and stretched within a compound tube of glass and brass. At the junction between the wire and spring a small mirror is fixed. When the tube is placed parallel to a Hertz's oscillator in action, the mirror is turned in a direction indicating an extension of the wire. The arrangement is so sensitive that an elongation of $\frac{1}{200000}$ of a mm. can be detected, and when placed at a distance of 4 metres from the oscillator the apparent extension is such as would correspond to a change of temperature of 0°·003 C. By its aid the author has roughly verified Hertz's statements that at considerable distances the intensity of radiation varies as the inverse distance; but before he can proceed further it is necessary to greatly increase the sensibility of the apparatus; and with a view of obtaining some suggestions in this direction, he exhibited it before the Society. Prof. Perry asked if the E.M.F. required to produce the observed results had been calculated; he also believed that the sensibility might be increased by using copper instead of platinum wire, and replacing the spring by a twisted strip. Mr. Blakesley inquired whether the effect of increasing the capacity of the ends of the wire had been tried. Mr. Boys said that if the observed effect was due to rise of temperature he would like to see it measured thermally. He also thought the effect might be due to extension caused by rapid electric oscillations in some such way as the elongation of an iron bar caused by magnetization. In answer to this, Prof. S. P. Thompson said the matter had been investigated experimentally, but with negative results. Prof. Herschel suggested the use of a compound spring such as is used in Breguet's metallic thermometers. In reply, Mr. Gregory said he had estimated the E.M.F. by observing that a Leclanché cell through 50 ohms produced about the same result. No improvement in sensitiveness was obtained by using copper wire or by increasing its capacity, and attempts to measure the rise of temperature by an air thermometer had been given up as hopeless. The President, in thanking the author for his paper, congratulated him on the ingenuity and courage displayed in producing an apparatus to measure such microscopic quantities as are here involved.—On a method of driving tuning-forks electrically, by Mr. Gregory. In order to give the impulses about the middle of the stroke, the fork is arranged to make and break the primary circuit of a small transformer, the secondary circuit of which is completed through the electromagnet actuating the fork. The prongs of the fork are magnetized and receive two impulses in each period. Another device was suggested, where the prongs respectively operate contacts which successively charge and discharge a condenser through the coils of the actuating magnet. Prof. S. P. Thompson said the methods, if perfect, would be of great service, and suggested that a fork so driven be tested optically by comparison with a freely vibrating one. He regarded the mercury contacts used as objectionable, for their capillarity and adhesion would probably cause the impulses to lag behind the appointed epochs. Prof. McLeod remarked that Lissajous' figures gave a satisfactory method of testing the constancy of period, and could be readily observed without using lenses, and in reference to liquid condensers suggested by the author for his second device, said that platinum plates in sulphuric acid were found to disintegrate when used for this purpose. He thought lead plates would prove suitable. Prof. Jones, who read a paper on a similar subject in March last, said he now used bowed forks, with which to synchronize the speed of the disk there described, and the frequency is determined by causing the disk to complete the circuit of his Morse receiver once each revolution.—On a physical basis for the theory of errors, by Mr. C. V. Burton. After pointing out that the law of error for any particular measurement depends on the nature of the conditions governing such measurement, the author considers several simple cases, and deduces their curves of error. A kinematic method of combining two or more independent errors, each following known laws, is then described and applied, and the general formula obtained leads to Laplace's law of error in the case of an infinite number of similar errors. Referring to Most Advantageous Combinations of measures, it is shown that the method of least squares is only a particular solution of the general equation, and is derived by assuming the individual errors to conform to Laplace's law. Subjective errors are next considered, and in conclusion the author says that "the law of error in a set of observations depends on the nature of each special case, and what may be called the probable law of error is determined by our knowledge of the conditions. The combination of three or more sources of error of comparable importance gives in general a law not seriously differing from that of Laplace, so that the method of least squares will be practically the most advantageous, except where a single source of error with a very different law is predominant above all the rest."—A note on the behaviour of twisted strips, by Prof. J. Perry, F.R.S., had been prematurely announced by mistake, and he accordingly gave only a brief outline of the paper. In a previous communication, Prof. Ayrton and the author enunciated a working hypothesis in which the strips were imagined to be split up into pairs of filaments, each pair acting as a bifilar suspension. The resulting formula for the rotation produced by a given load did not agree with experiment, and quite recently the author had recognized why the formula was incorrect. The bifilar law they had assumed was only true for small twists, but he now saw another method of treatment by which he hoped to verify the formula derived from experiment before the next meeting. Prof. Fitzgerald reminded Prof. Perry of a method of attacking the problem suggested by the speaker some time ago, in which each filament was supposed to be wrapped round a smooth cylinder; and said that on working it out the formula was found to be very complicated. Mr. Trotter thought the pairs of strips might be regarded as twisted ladders, and Mr. Gregory said this suggestion reduced the problem to a series of bifilar suspensions which had already been worked out.—On electrifications due to contact of gases and liquids, by Mr. J. Enright. For some time past the author has been studying the electrical phenomena attending solution, by connecting an insulated vessel in which the solution takes place with an electrometer. As a general rule, no effect is observed if nothing leaves the vessel, but when gases are produced and allowed to escape the vessel becomes charged with + or − electricity, depending on the nature of the liquid from which the gas passes into the air. As an example, when zinc is placed in hydrochloric acid, the deflection of the electrometer is in one direction whilst the liquid is chiefly acid, but decreases and reverses as more and more zinc chloride is produced. From such observations the author hopes to obtain some information relating to atomic charges. Owing to the lateness of the hour, the latter portion of the paper and the discussion on it were postponed until next meeting.

Paris.

Academy of Sciences, Nov. 4.—M. Des Cloizeaux, President, in the chair.—Instrument for measuring the coefficient of elasticity of metals, by Mr. Phillips. This is a large spiral spring and balance wheel, the former made of the metal to be examined. —*Rôle* and mechanism of the local lesion in infectious diseases, by M. Ch. Bouchard. Whereas in absolute immunity, there is, after inoculation, neither general infection nor local lesion, and in total absence of immunity, general infection, often without local lesion, in relative, normal, immunity there is local lesion mostly without general infection; in the last case, as experiment shows, it is not the local lesion that causes the immunity, but *vice versâ*. Inoculating vaccinated and unvaccinated rabbits with pyocyanic Bacillus, the author found, in the former, rapid appearance of leucocytes, all having many Bacteria, which were soon resolved into granulations, and in sixteen hours were quite gone; while the free Bacteria soon decreased in number. In the other animals, few leucocytes, no Bacilli in them, and free Bacteria multiplying.—Statistics of preventive treatment of rabies, from February 9, 1888, to September 15, 1889, at the Pasteur Institute of Rio de Janeiro (Dr. Ferreira dos Santos), by the Emperor of Brazil. Of 156 who underwent full treatment, only one died, and not certainly from rabies; this gives a mortality of 0·64 per cent.—On the velocity of wind at the top of the Eiffel Tower, by M. A. Angot. Three months' observations give a mean of 7·05 m. as compared with 2·24 m. at the Central Meteorological Office (21 m. from the ground). While at low stations there is a minimum at sunrise and a maximum at 1 p.m., the Eiffel (like mountains) showed a minimum about 10 a.m. and a maximum at 11 p.m. (while at midday there was but a slight upward bend of the curve).—On phenyl-thiophene, by M. A. Renard. This is prepared by passing through an iron tube, heated to dark redness, vapours of toluene and of sulphur, and distilling the condensed product. Analysis gave the formula $C_6H_5—C_4H_3S$. With bromine, nitric acid, and sulphuric acid, substitution products are obtained.—Researches on digitaline and tanghinine, by M. Arnaud. By heating digitaline with baryta-water to 180° for several hours, it combines with water yielding the compound $C_{31}H_{52}O_{11}$, from which the formula $C_{31}H_{50}O_{10}$ is deduced for digitaline. The formula of tanghinine, similarly deduced, is $C_{27}H_{40}O_8$. This formula differs from that of Schmiedeberg for digitaline, viz. $C_{21}H_{32}O_7$.—Studies on the embryology of the axolotl, by M. F. Houssay. He describes the mechanics of segmentation, the origin and development of the peripheral nervous system, and the morphology of the head.—On the cytoplasm and the nucleus in Noctiluci, by M. G. Pouchet. Flemming's chromatine seems to be formed of two substances, chromatoplasm and hyaloplasm; and the proportion of the former increases as gemmation proceeds; hence the more and more lively colour of the segmented nuclei.—On the parasitic castration of *Typhlocyba* by a Hymenopterous larva (*Aphelopus melaleucus*, Dalm.), and a Dipterous larva (*Atelenevra spuria*, Meig.), by M. A. Giard. In *T. hippocastani*, the eight terminal branches of the penis are reduced to six, four, or three. A pair of curious invaginations on the ventral surface of the body are also shortened.—Action of serum of diseased or vaccinated animals on pathogenic microbes, by MM. Charrin and Roger. Operating with the pyocyanic Bacillus and rabbits, they found the serum of vaccinated animals more adverse to growth of the Bacillus than normal serum, but somewhat less than that of the diseased animals.—Contribution to the semeiological and pathogenic study of rabies, by M. G. Ferré. Inoculating by trepanation, and with stronger virus than before, they found that the respiratory acceleration appeared on the fourth instead of the fifth day; the respiratory centres being invaded correspondingly sooner. The symptoms could not be attributed to thermal elevation, the maximum of this occurring later.—Statistics of preventive inoculations against yellow fever, by Dr. Domingos Freire. From 1883 to 1889, there were 10,524 persons inoculated in Brazil; and the mortality was 0·4 per cent. The deaths of non-vaccinated during the four epidemics were over 6500.—On the modifications in normal gaseous exchanges of plants by the presence of organic acids, by M. L. Mangin. He injected malic, citric, and tartaric acids into leaves of Japanese prick-wood, bay rose, and lilac, and found these leaves to behave like Cacteæ and Crassulaceæ. In the dark, the volume of carbonic acid liberated is greater than that of oxygen absorbed; and in the light, there is emission of oxygen without correlative absorption of carbonic acid.—On the existence of numerous zeoliths in the gneissic rocks of Upper Ariège, by M. A. Lacroix.

DIARY OF SOCIETIES.

London.

THURSDAY, November 14.

Mathematical Society, at 8.—Isoscelian Hexagrams: R. Tucker.—On Euler's ϕ-Function: H. F. Baker.—On the Extension and Flexure of a Thin Elastic Plate: A. B. Basset, F.R.S.

Institution of Electrical Engineers, at 8.—On the Lighting of the Melbourne Centennial International Exhibition: K. L. Murray.

FRIDAY, November 15.

Physical Society, at 5.—On the Electrification due to the Contact of Gases and Liquids: J. Enright.—On the Effect of Repeated Heating and Cooling on the Electrical Resistance and Temperature Coefficient of Annealed Iron: H. Tomlinson, F.R.S.—Notes on Geometrical Optics, Part II.: Prof. S. P. Thompson.

Institution of Civil Engineers, at 7.30.—The New Harbour and Breakwater at Boulogne-sur-Mer: S. C. Bailey.

MONDAY, November 18.

Aristotelian Society, at 8.—Scepticism: S. Alexander.

TUESDAY, November 19.

Institution of Civil Engineers, at 8.—Water-Tube Steam-Boilers for Marine Engines: John I. Thornycroft.

Royal Statistical Society, at 7.45.—Opening Address by the President, Dr. T. Graham Balfour, F.R.S.

WEDNESDAY, November 20.

Geological Society, at 8.—On the Occurrence of the Striped Hyæna in the Tertiary of the Val d'Arno: R. Lydekker.—The Catastrophe of Kantzorik, Armenia: M. F. M. Corpi. Communicated by W. H. Hudleston, F.R.S.—On a New Genus of Siliceous Sponges from the Lower Calcareous Grit of Yorkshire: Dr. J. G. Hinde.

Royal Meteorological Society, at 7.—Second Report of the Thunderstorm Committee—Distribution of Thunderstorms over England and Wales, 1871-87: William Marriott.—On the Change of Temperature which accompanies Thunderstorms in Southern England: G. M. Whipple.—Note on the Appearance of St. Elmo's Fire at Walton-on-the-Naze, September 3, 1889: W. H. Dines.—Notes on Cirrus Formation: H. Helm Clayton.—A Comparison between the Jordan and the Campbell-Stokes Sunshine Recorders: F. C. Bayard.—Sunshine: A. B. MacDowall.—On Climatological Observations at Ballyboley, Co. Antrim: Prof. S. A. Hill.

Society of Arts, at 8.—Opening Address by the Chairman, the Duke of Abercorn, C.B.

University College Chemical and Physical Society, at 4.30.—Pyridine and the Alkaloids: Dr. N. Collie.

CONTENTS.

THURSDAY, NOVEMBER 21, 1889.

ROCK METAMORPHISM.

Chemical and Physical Studies in the Metamorphism of Rocks, based on the Thesis written for the D.Sc. Degree in the University of London, 1888. By the Rev. A. Irving, D.Sc.Lond., B.A., F.G.S. (London: Longmans, Green, and Co. 1889.)

DR. IRVING is well known as a writer on Bagshot beds. He appears in a new light as the propounder of theories dealing with the metamorphism of rocks. His ideas on this subject are classified under three heads: paramorphism, metatropy, and metataxis. *Paramorphism*, according to the author, includes those changes within in the rock-mass, involving changes in the chemical composition of the original minerals and the formation of new minerals; *metatropy* denotes changes in the physical character of rock-masses; and *metataxis*, mechanical changes, such as the development of cleavage. Changes brought about by the introduction of a new, or the removal of an old mineral (*e.g.* dolomitization) are treated under the head of *hyperphoric change*.

The author writes, he tells us, for those who are willing to look at geological phenomena "in the light of physical and chemical ideas." To all others his dissertation "must read rather like romance than sober science." He is not far wrong when he complains that the chemical side of geology has been neglected since the time of Bischof. The reason for this is to be found in the fact that geologists have been too busily engaged in reaping golden harvests in the demesnes of palæontology and stratigraphy to be much tempted by the allurements of chemical geology. With the resuscitation of petrology, however, the chemical constitution of rocks begins again to present problems of great interest and importance. But the author turns his chemical knowledge to bad account, we think, in applying it to the elaboration of sweeping generalizations. The views he puts forward may or may not be founded on sound chemical and physical axioms; but mere test-tube reactions will not suffice to explain the operations of Nature in the vast laboratory of the universe. The phenomena of metamorphism represent the net result of numerous and often antagonistic forces; and are not always simple reactions that may be expressed by a neat chemical equation.

Dr. Irving appears to be highly gifted with what he terms a "scientific imagination," the meteoric flights of which carry him far above the solid ground of fact or even justifiable theory. An instance of this faculty of the author's will be found on p. 66, where he seeks to explain the origin of foliation in Archæan rocks by the influence of "solar and lunar tides upon the non-consolidated magma in the Archæan and pre-Archæan (*sic*) stages of the earth's evolution." He proceeds:—

"In such an unequally viscous mass there would be tension, contortion, and shearing to any extent during the tidal pulsations which the magma was suffering. . . . Portions already solidified, or nearly so, by segregation or otherwise, as time went on, would by their *vis inertiæ* present obstacles around which a fluxion structure would develop itself in the contiguous portions of the yielding magma, giving us perhaps in some cases 'Augengneiss.' The local tension of parts of the viscous lithosphere, especially near the crests of the waves, would imply stretching and consequent lowering of temperature, a circumstance favourable to local solidification. Who shall say that in the later and feebler struggles of this kind, as secular cooling went on, and the magma approached nearer and nearer to the conditions required for consolidation, some of these tidal waves may not have become *in situ* sufficiently rigid to outline some of the earliest lines of elevation?"

This is speculative enough in all conscience. On p. 29, the author discusses the influence of the salts dissolved in sea-water on submarine lava-flows, and suggests that serpentinization and the conversion of orthoclase into albite are the result of some process of "submarine paramorphism" effected by this agency. This, again, is pure hypothesis, there being no facts to support such a view.

There is a flavour of pedantry in the use of such expressions as "burnt hydrogen" for water (p. 64), or in such sentences as "orthoclase is probably the embryonic silicate of the terrestrial lithosphere" (p. 67). As the old lady is said to have remarked of the word Mesopotamia, there is something especially comforting and satisfying about this last sentence.

The pages bristle with "hard words," some of which are new to science. "Vitreosity" has an uncanny sound; "apophytic" is curious; and "dehydrodevitrification" is as inelegant as it is long. Indeed, so technical is the author's language that a clear understanding of his meaning involves constant reference to his definitions. Unfortunately such reference is rendered impracticable by the absence of an index.

The book bears witness to Dr. Irving's extensive acquaintance with foreign chemical and geological literature; references to foreign sources being abundant, sometimes superfluous. Indeed, there is more evidence of the author's acquaintance with literature than with facts derived from original observation. Good ideas may here and there be picked out; and the work no doubt contains some plausible explanations of geological phenomena; but of this we are assured, that the science of geology will not be advanced by those who spend their time in manufacturing wide-reaching generalizations or attractive theories in the library, but rather by those who are content to labour, with the hammer in the field, the microscope in the cabinet, and the balance in the laboratory at the ofttimes wearisome task of unravelling details.

This book may be placed in the same category as Sterry Hunt's "Chemical and Geological Essays." Such books can be recommended to those with a taste for speculation and rumination. To others they may be productive of mental confusion and headache.

HAND-BOOK OF DESCRIPTIVE AND PRACTICAL ASTRONOMY.

Hand-book of Descriptive and Practical Astronomy. By G. F. Chambers, F.R.A.S. Part I. The Sun, Planets, and Comets. (Oxford: Clarendon Press, 1889.)

THE avowed aim of the author of this work, since the publication of the first edition in 1861, has been to keep its pages up to date—to make it a sort of *vade mecum* to astronomers; and, regarded as a book en-

deavouring to effect a compromise between purely elementary works on astronomy and advanced treatises, it is worthy of some praise. With the many remarkable developments of astronomical science during the last quarter of a century, the bulk of the original volume has been somewhat increased by additions, and it has now been decided henceforth to publish the work in three divisions, viz.—

(1) The sun, planets, and comets.
(2) Instruments and practical astronomy.
(3) The starry heavens.

The first division of the work is now before us, and viewed as a handy book of reference it has many commendable features; but all that could be said in its praise would be the reiteration of comments upon former editions.

The most important application of spectroscopy to astronomy is too well known to need any enlarging upon. It may be said to be almost entirely a creature of the last quarter of a century, but by far the greater amount of this spectroscopic work has been directed to the sun, whilst many new and important discoveries have been made in connection with it. In pre-spectroscopic times a spot on the sun was only that, and nothing more; and a solar prominence was a stupendous flame, the observation of which was only possible at eclipses. Nothing was known of their constitution; and, in fact, all we now know of the physical and chemical condition of the sun has been gained by spectroscopists. However, it is not necessary here to consider the enormous work that has been done in this direction, but it is our duty most emphatically to protest against a compilation such as the one before us—purporting to be a completely revised account of astronomical labours and advances, and yet rendering terribly conspicuous by its absence everything that relates to spectroscopy. It is like a book on locomotion leaving out all about railways because they were not prominent when the first edition was published. The pictorial representations of the corona, the solar prominences, the surface of the sun and the spots upon it, are well discussed in their respective sections, but no room has been given to an examination of their constitution by means of the spectroscope; and indeed, as far as this book is concerned, the whole work that has been done in connection with solar physics might have been left undone.

But these remarks apply not only to the chapters relating to the sun; those on the planets and comets respectively are in the same incomplete condition. Without the spectroscope, the source of luminosity of a comet was far beyond human ken, and its whole constitution was a matter of considerable doubt; with this instrument, however, much has been added to our knowledge—the comet's light has been analyzed, and the whole sequence of changes, as it goes from aphelion to perihelion and back again, is now understood. Yet the spectroscope might never have been turned to these bodies, or indeed utilized in any way, if the utility and importance of the work done were measured by the brief notice with which the author has seen fit to dispose of it, and the following may be said to be the reason for his grievous omissions:—

"The study of the sun has during the last few years taken a remarkable start, owing to the fact that, by the aid of the spectroscope, we have been enabled to obtain much new information about its physical constitution. The subject being, however, a physical rather than an astronomical one, and involving a great amount of optical and chemical details, it cannot conveniently be discussed at length in a purely astronomical treatise, though something will be said concerning it later on in the portion of this work dedicated to spectroscopic matters."

This explanation, however, only aggravates the fault. The importance of the work that has been done is assented to, but, instead of including that part of it relating to the sun in a chapter on that body, instead of considering the spectroscopy of comets as inseparable from a chapter devoted to their discussion, the author has relegated the whole work to an unpublished section devoted to astronomical instruments. Such an arrangement is undoubtedly wrong. A chapter on the sun must contain all that is known about that body, if it strives to be at all complete; similarly, a chapter on comets cannot approach completion unless their spectra are considered; thus this work cannot lead the general public to a just appreciation of the many advancements that have been made. The most elementary text-books rightly include the spectroscopic labours and discoveries, whereas this so-called hand-book, although aiming at being an historical account of the work that has been directed to the sun, planets, and comets respectively, leaves a vast array of facts out of consideration altogether.

There are a few minor faults, one of which is the figure relating to Foucault's pendulum experiment for determining the rotation of the earth. The author appears to have discarded the method of suspension adopted by Foucault, and the pendulum is sketched as if rigidly attached to a beam. The accompanying text also leaves this most important experimental detail out of consideration.

But apart from these points, the work is worthy of some commendation. An addition has been made to the chapter on comets, viz. a method of determining the elements of the orbit of a comet by a graphical process. The catalogue of comets whose orbits have been computed has also been brought up to date, and similar additions have been made to the chapters on periodic and remarkable comets. Doubtless the book will prove to be what it has been heretofore—a handy reference to some astronomical facts.

ELECTRICAL UNDERTAKINGS.

Proceedings of the National Electric Light Association at its Ninth Convention, 1889. Vol. VI. (Boston, Mass., U.S.: Press of *Modern Light and Heat*, 1889.)

WE have before us, in this volume, an account of the proceedings of the National Electric Light Association in the United States during the Convention held at Chicago on certain days in February 1889.

This body is one which, in the United States, has been brought into existence by the growing necessities and rapid expansion of the electric light and power industry. Probably its nearest English analogue is the Iron and Steel Institute. It is essentially a commercial association, and its aims may be said to be comprised within the limits of the exchange of practical information

amongst its members, and of such joint action as will further the use and success of these electrical trades. Hence its objects are not, exactly speaking, scientific, at least in the usual sense of the word, and the intermixture of genuine desire to exchange veritable experience, with a certain element of effort to push into notice particular personal "interests," renders a discriminating mind necessary in dealing with its Reports. At the time of writing, when the work of practically providing London with distributed electric current is being carried on with energy in diverse directions, and the various Electric Supply Companies are laying down mains and establishing stations, this Report serves a useful purpose of enabling us to judge the present state of the industry in the country where, of all others, it has had the most unhindered development.

In his opening address, the President, Mr. S. A. Duncan, gave some figures which are significant of the immense extent to which the electric lighting business has now progressed in the United States. The total number of arc lights in daily use is about 220,000; of incandescent lamps, some 2,500,000. There are approximately 5700 central stations and isolated plants, supplying electric current to single buildings or groups, or sections of towns. There are 53 electric railways in operation, and 44 in progress, on which 378 electric tram-cars travel over 294 miles of track. The total capital employed and sunk in these various undertakings is probably not under fifty millions sterling. When we consider that this is the growth of ten years, we are bound to admit, not only that this youngest of the applied sciences is of vigorous growth, but that its commercial basis must be sound. The Proceedings of the Convention take the form of a series of Reports on various points of interest which are drawn up by individuals or Committees, and then discussed by the whole body.

One of the important questions which in this meeting received consideration was that of underground conductors. It has been evident for a long time that arc-light wires, telephone, telegraph, fire-signal, and incandescent-lamp wires cannot be permitted to increase without limit in the form of overhead conductors. In the early days of the telephone and arc light the inconvenience of overhead wires did not present itself as a formidable one; but, with their rapid growth, the dangers to life and property arising from an indiscriminate collection of electric wires strung on poles or attached to roofs in large cities became apparent. Hence has arisen a demand that they shall be put underground.

Unfortunately this is not so easy in practice as it seems. The distributing companies in many cases desire to avoid the cost of making the exchange in those cases in which they are operating overhead wires. The expense of an underground system of conductors is from five to ten times that of aërial lines. Moreover, the various methods suggested for sub-laying the conductors in streets and roads have all peculiar merits and demerits. Mr. Edison, as is well known, places the copper conductors in steel pipes, insulating them with a bituminous compound, and lays these like gas-pipes in the streets. This system has been operated for years in New York, Milan, Boston, and Chicago, with a high degree of success. Other inventors have advocated a conduit system; others, again, the use of bare copper conductors insulated in a subway. It is thus seen that the necessary experience for satisfactorily laying down underground systems of conductors for the conveyance of large electric currents is only slowly being collected.

The city of Chicago has one of the most completely developed systems of underground conductors for arc-light wires. There are some seventy-eight miles of underground cable conveying currents under a pressure of 1000–1800 volts. The members of the Convention not unnaturally exhibited considerable differences of opinion on this question of underground conductor systems. A Committee appointed for the purpose had issued a circular to about 1066 managers of central stations and lighting systems and others, with the object of eliciting their opinions on the subject of underground conductors. Out of this number 130 returned very full answers to the various questions, and the diversity of opinion seems very great. It is difficult, however, to believe that the process of collecting information was that which would lead to the best results, and although the various views put forward in the discussion on the Report are interesting, they do not indicate a solidarity of opinion on any one point. It is perfectly certain, however, that in England electric conductors for systems of town lighting by electricity will have to be placed underground, and it is also equally certain that those responsible for this work will have to exercise the greatest discretion and take the fullest advantage of existing experience. The question of the fire risks of electric lighting also occupied the attention of the members. In the United States, as with us, the opinion based on experience is that when the work of installing the electric light is carried out under all known proper precautions, and by the best guidance, there is greater safety in it than in gas illumination, but that when these known precautions are disregarded then danger ensues. Minor questions, such as the disruptive discharges in lead cables and fuel oil, attracted briefer attention. The importance of such a gathering in guiding the experience of those who are fostering an industry like that of electric lighting, in which invention advances by leaps and bounds, is very great. We in England, thanks to the revision of the Electric Lighting Act, are now entering on a period of great electrical activity, and already it has been found that the commercial side of electrical engineering requires the association of those engaged in it for mutual advice and joint action, and the London Chamber of Commerce has now an active Electrical Section which fulfils to some extent the functions of the National Electric Light Association in America.

J. A. F.

DIANTHUS.

Enumeratio Specierum Varietatumque Generis Dianthus. Auctore F. N. Williams, F.L.S. Pp. 23. (London: West and Newman, 1889.)

ONE of the things most wanted by species-botanists at the present time is a set of monographs of a number of the familiar large genera of Polypetalous Dicotelydons. The natural orders of Polypetalæ were

monographed by De Candolle in the "Prodromus" between 1824 and 1830, and the scattered material relating to many of the orders and genera has not since been brought together and codified. As instances of genera now involved in great confusion for want of a more recent elaboration, we may cite Ranunculus, Viola, Papaver, Alyssum, Draba, Dianthus, Geranium, Galium, and many others. The present paper is, unfortunately, not a monograph of Dianthus, but only a list of the known species classified into groups, accompanied by general remarks on the structure of the different organs in the genus, and on their range of variation, so that, though it is interesting and useful as far as it goes, it still leaves very much to be desired. Although, on the one hand, Caryophyllaceæ are dried for the herbarium very easily, and suffer little in the process, yet Dianthus is a very difficult genus for botanists to deal with and to understand. There are 230 species for a monographer to characterize. The range of variation between the extreme types is not great, and some of the commoner species (*e.g. D. Seguieri*, *plumarius*, and *Carthusianorum*) are very variable, the consequence being that, one often sees them named in gardens very incorrectly, forms of *plumarius* especially, which is hardy and spreads readily, doing duty for many totally distinct species.

Dianthus is a genus quite characteristic of temperate and sub-temperate climates. It has its head-quarters in Europe and Western Asia. There are several species at the Cape; a few are Himalayan, Chinese, and Japanese; none reach Australia, New Zealand, or the Andes; and only one just touches the extreme north-western tip of the American continent. There are two principal sub-genera: Caryophyllastrum, of which the carnation may be taken as the type, which is far the largest; and Armeriastrum, or Carthusianastrum, of which the flowers are numerous and clustered, as in the sweet-william. There is a third small sub-genus, intermediate between Tunica and the true pinks, which is classified by Bentham and Hooker with Tunica, and by Mr. Williams, following Linnæus and Koch, as a third sub-genus of Dianthus. Within the bounds of the genus, Mr. Williams finds his primary characters—those which mark groups—in the form of the calyx, the nature of the margin of the lamina of the petals, the presence or absence of a beard at the junction of the blade and claw of the petals, filaments, and styles, the shape of the leaf, and the disposition of the flowers; and his secondary characters—those which distinguish species—in the number and shape of the bracts of the epicalyx, the form of the lamina of the petals and their apposition, the character of the calyx-teeth, the form and structure of the capsule, the form and structure of the seeds, and the disposition of the fascicles of veins in the leaves of the barren shoots and flowering stems. His groups and species do not differ materially from those given in his paper in the *Journal of Botany* for 1885, p. 340. The list would have been more useful if he had stated the native country of each species, and added a reference to where it was first described. We hope, however, that he will see his way to publish, before long, the monograph of which this is a mere outline sketch.

J. G. B.

OUR BOOK SHELF.

Magnetism and Electricity. By Arthur W. Poyser, M.A. (London: Longmans, Green, and Co., 1889.)

SINCE the amount of knowledge that is supposed to constitute an elementary scientific education increases every year, there is sufficient justification for the publication of a series of science manuals designed to meet the growing requirements of the Science and Art Department examinations, and this work is an excellent representation of such a series. Apart, however, from the value of this book as an examination manual, it possesses considerable merit. The matter contained in it is just about as much as would cover the course usually taken in a year's school work; the explanatory text is couched in the clearest language, and the experiments described are capable of being easily brought to a successful termination. Also the 235 illustrations will be of considerable assistance to the student, whilst the many exercises and examination questions interspersed throughout the book may be useful tests of his knowledge. The text-books that in their day have been eminently successful, if unrevised, must be supplanted by others which take a more extended view of the subject; hence it is that this book will compare most favourably with any written for the purpose of imparting a rudimentary knowledge of magnetic and electrical phenomena and the laws by which they are governed.

The Engineer's Sketch-book. By Thomas Walter Barber. (London: E. and F. N. Spon, 1889.)

ENGINEERS and draughtsmen generally keep note-books in which are jotted down most things they wish to particularly remember, accompanied by rough sketches when necessary. The author of this book is no exception to the rule. He tells us he has made many notes and sketches during his experience as an engineer, and has often found the want of such a collection for reference. This volume consists of about 1936 sketches, classified under different headings, of devices, appliances, and contrivances of mechanical movements. The book is certainly unique in its way, and will prove useful to those who have machinery to design, who may require suggestive sketches of mechanical combinations to accomplish some desired end. The author truly remarks that a sketch properly executed is to a practical man worth a folio of description. Hence the descriptions given are generally mere names, with occasionally a concise statement of purpose. Each sketch bears a number, and on the opposite page this number is to be found with the description, &c.,—a very good arrangement.

These sketches are clearly printed, and are probably executed from scale drawings in most cases. Taken as a whole, they fairly represent what they profess to do. Sketch 1636, however, is supposed to represent a Ramsbottom safety valve, but it gives a radically wrong impression of this valve. The lever is shown resting on the two valves certainly, but the spring is attached to the lever at a point considerably above the assumed straight line joining the points resting on the valves—an impossible position. Again, one of the two points of the lever resting on the valves is usually loose and connected with the lever by a pin. The sketch shows the lever and the two projecting points made solid. This example is the most unpractical sketch discovered in the book, and should be rectified in a future edition. A fairly good index adds to the usefulness of the volume. There is ample evidence of careful work on the part of the author, and he is to be congratulated on writing a book which will probably be of use to many engineers and those connected with the profession.

N. J. L.

A Life of John Davis. By Clements R. Markham, C.B., F.R.S. (London: George Philip and Son, 1889.)

THIS is the first volume of what promises to be a series of great value and interest. The object of the series, as explained by the editors, is to provide a biographical history of geographical discovery. Each of the great men who "have dared to force their way into the unknown, and so unveiled to us the face of mother earth," will form the subject of a volume; and an attempt will be made, not only to present a vivid picture of the character and adventures of these heroes, but to estimate exactly the scientific value of their work. If the scheme is carried out in a manner worthy of the stirring tales to which it relates, the series will be a source of much wholesome pleasure to all who care to understand how our present knowledge of the earth's surface came to be built up, and who are capable of appreciating the splendid qualities, moral and intellectual, of all who have won for themselves a place in the list of illustrious explorers. The subject of the present volume could not have been intrusted to a more suitable writer than Mr. Clements Markham. He tells in a simple and natural style the tale of Davis's life, displaying at every stage of the story full and accurate knowledge, and summing up clearly the achievements which entitle the discoverer of Davis Straits to be ranked "among the foremost sea-worthies of the glorious reign of Queen Elizabeth." Two admirable chapters are devoted to the following-up of the work of Davis, and in an appendix the author gives all necessary information as to authorities. Mr. Markham has done his work well, and it will be no easy task for the writers of the succeeding volumes to maintain the series at the same high level.

The Brook and its Banks. By the Rev. J. G. Wood. (London: The Religious Tract Society, 1889.)

The Zoo. Second Series. By the Rev. J. G. Wood, (London: Society for Promoting Christian Knowledge, 1889.)

THE first of these two books was written for the *Girls' Own Paper*, and a few chapters of it have been printed in that periodical. Now the complete work is issued separately, and it will no doubt be welcomed by many readers who have already profited by the late author's well-known writings. The reader is supposed to be conducted along the banks of an English brook, and to learn, as he advances, the characteristics of the living creatures which are to be found by the way. The idea is carried out brightly, and—we need scarcely say—with ample knowledge. There are many illustrations, and they add considerably to the interest of the text.

"The Zoo" contains an account of animals of the weasel tribe, the seal tribe, the rodent family, and various kinds of oxen. The descriptions are clear, compact, and lively, and cannot fail to interest the young readers for whose benefit the book was originally planned. Mr. Harrison Weir contributes a number of excellent illustrations.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Protective Coloration of Eggs.

THE following letter records a very interesting observation which is new to me, and I should be glad to hear if any similar fact has been noted before. If not, it would be very interesting if those who have the opportunity would, in the coming spring, seek for as many nests as possible of the red-backed shrike, and see if they can find any correlation between the colours of the eggs and the lining material of the nest.

Parkstone, November 1. ALFRED R. WALLACE.

"*Merchant Taylors' School, Crosby, Liverpool,*
"*October* 15, 1889.

"DEAR SIR,—I wish to bring before your notice an observation of mine relative to the purpose of colour in animals.

"The red-backed shrike (*Lanius collurio*). Colour of eggs—*either* pale blue or green, white ground with zone of spots at larger end; *or*, *pink ground* with reddish spots.

"*Observation.*—The colour of the lining substance of the nest—such as roots—assimilates to the colour of the eggs, being dirty gray material when the eggs are to be pale (blue or green) white, but being of red-brown roots, &c., when the eggs are to be pink.

"Evidence for above statement. About sixteen years ago I was a lad of fifteen, an enthusiastic birds'-nester, living at Maidstone, and found several (I forget how many) nests, and noticed this; and it so puzzled me—because I could not make out how the bird knew what coloured lining to select, because she made her nest *before* she laid her eggs—that I have never forgotten it. In those days I had never heard of 'The Origin of Species,' nor did I trouble myself about evolutionary theories, knowing nothing about them, so that there was no *predisposing cause* in me to make a wrong observation. Yet I remember it was only a *school-boy's observation*, and therefore it needs confirmation.

"*Assume the fact.* Protective, obviously. Yet, how does the bird know? We know birds build nests from *observing* other nests, and not by instinct wholly.

"(*a*) Have we here incipient species, in which the young, emerging from *pink eggs*, remember their own infancy in a reddish nest?

"(*b*) Has the sight of the red lining an influence over the mother to tinge the eggs pink—*i.e.* would a shrike brought up in a *pink cage* be more likely to lay pink eggs? or a gray rabbit in a black or white hutch have a greater proportion of black or white variants in her litter?

"(*c*) A mere coincidence; too few observations.

"Will you forgive one who intends to be amongst your audience on October 29 and 30, if not prevented, thus trespassing on your time—time which, spent in research, is so valuable to the whole scientific world? Yet, I *do think* my boyhood's observation is worth recording, if only to direct other observers.

"*E.g.* has the amount of white quartzite veins in a cliff, or chalk, *any* influence in the percentage of white, as against blue, eggs of the common guillemot?

"Believe me, yours faithfully,
"(Rev.) FRED. F. GRENSTED."

Science and the India Civil Service Examinations.

THE position of science candidates in the Civil Service competitions is largely in the hands of the science examiners. In some cases they have practically struck their subject out of the schedule by requiring, or by acquiescing in, the demand for a standard of knowledge far beyond the proportion of marks assigned. Even in the last India Civil Service competition the first two men in chemistry only scored 196 and 195 respectively, whilst the first two in German, out of the same maximum, gained 359 and 353. If the eminent men of science who undertake these examinations would see that science had fair play, many more candidates would be encouraged to study it. Whatever the private views of the Civil Service Commissioners may be, their absolute justice and honourable impartiality are unassailable. Even if they did not altogether concur in the opinions of the examiners, they would give their arguments careful consideration, and see that all interests should be duly regarded.

It will not advance the claims of science to weight them with the very doubtful proposition that "the Universities of England and India" are the only places where "well educated" men are to be found. Many most distinguished men of science have not had the advantage of a University degree in early life. No one would venture to class them for this reason in "an inferior order of men." HENRY PALIN GURNEY.

2 Powis Square, W., November 15.

The Physics of the Sub-oceanic Crust.

In the new edition of his "Physics of the Earth's Crust," Mr. Fisher has made a great advance on his former position, for he sees his way to explain the formation of mountain chains, and all the phenomena of compression which are so strikingly exhibited in the crust of the earth, without depending on his former theory of columnar expansion, and without falling back on the contraction hypothesis.

He believes that the existence of a liquid substratum beneath a thin crust is consistent with the physical conditions of the universe; and argues that no appreciable tide would be produced in it if the liquid magma consisted of an intimate association of fused rock and dissolved gases. He further concludes that this magma is not an inert or motionless liquid, but one in which convection currents are constantly bringing up heat from below, and leading to frequent internal displacements of mass.

In this hypothesis he finds a means of explaining the movements of the earth's crust. Whether Mr. Fisher's position can be maintained must be decided by those who are accustomed to deal with the physical problems involved, but geologists will be glad if it should prove that the objections to the existence of a liquid substratum have been successfully met, for they have always found a difficulty in explaining geological phenomena without having recourse to the supposition of a liquid layer.

One of the most important chapters in the book is that on the sub-oceanic crust, and it is on this that I propose to offer a few remarks, taking it for granted that a truly liquid substratum with a play of convection currents does really exist.

Mr. Fisher's object is to ascertain the thickness and density of those parts of the crust which lie beneath the oceans, and to see whether in these respects they differ from the continental portions. This he does by making a series of assumptions, and considering how far the results are compatible with known facts and conditions. This process involves the dismissal of certain hypotheses, but although he eventually finds one which fulfils the requisite conditions, it does not follow that no other equally satisfactory hypothesis can be found. Consequently his results though interesting cannot be regarded as final. The suppositions he is obliged to introduce before obtaining satisfactory results are, that the density of the subtratum beneath the continental and the sub-oceanic portions of the crust is different, and that the sub-oceanic crust consists of two layers of different densities.

It is conceivable, however, that the lower part of the crust is *everywhere* denser than the upper part, and consequently that two layers of continental crust should be introduced into the problem; whether this hypothesis would likewise fulfil the conditions, and whether it would lead to the same results as that which Mr. Fisher adopts, could only be ascertained by trial. Mr. Fisher informs me that he has not made this trial, and that every additional assumption introduced increases the great labour of the calculations.

Let us assume, however, that no other hypothesis would satisfy the conditions so well as that which he has adopted, and let us see to what conclusions it leads. Mr. Fisher derives from it the following important results:—

(1) That the sub-oceanic crust dips more deeply into the substratum than the continental crust.

(2) That its lower part is more dense than the substratum.

(3) That the density of the liquid substratum is less beneath the oceans than beneath the continents.

This last result leads to the conclusion that the differences of density in the substratum must give rise to ascending and descending convection currents, and that the ascending currents will rise beneath the oceans while the descending currents will occur beneath the continents. "That the former occupy so much larger an area is," he says, "no more than we might expect, because to whatever immediate cause they may be due, they are ultimately the result of secular cooling. . . . The descending being merely return currents will be confined to the smaller area, but on that account they will move the more rapidly."

Finally he says that these conclusions confirm the theory of the permanence of oceans, "because it is difficult to conceive how the subjacent crust, once more dense, can have subsequently passed into the less dense condition which would be requisite to render it continental." I venture to think he is hardly justified in making this unqualified statement, and purpose to show that his results only confirm the theory of the permanence of oceans in a limited and partial manner.

In the first place, if chapters xvii. and xxiv. are read carefully, it will be obvious that Mr. Fisher uses the terms oceanic and sub-oceanic in a special sense. On p. 233 he classes areas having less than two vertical miles of water as "extensions of the elevations that produced the continents," and even those with depths of two to three miles of water he regards as "sometimes connected with and prolongations of the first." In other words, he looks upon the shallower parts of the great oceans from a continental coast-line to a depth of at least 2000 fathoms as extensions of the continental elevations.

Again, on p. 331 we find him saying that New Caledonia and the Seychelles are not properly speaking oceanic islands, because the first is a prolongation of the submerged ridge which connects New Zealand with North Australia, and because the latter belongs to an extension of the Madagascar ridge into the Indian Ocean. Now a reference to the physical chart of the oceans given in the "Narrative of the Cruise of the *Challenger*" (vol. i.) shows that the 1000-fathom line completely encircles New Caledonia and the adjacent islands, and that the submerged ridge which he speaks of would be a very narrow one unless we regard it as extending to the line of 2000 fathoms; but this line includes also the Solomon Islands, the Fijis, and the Friendly Islands, so that if New Caledonia cannot be considered as an oceanic island neither can the other islands just mentioned, though no one would reject them from that category on other grounds. Similarly, the Seychelles and Amirantes are surrounded by water of more than 1000 fathoms, and are usually regarded as oceanic islands. The same may be said of Barbados, where stratified Neozoic rocks are found.

The contour-line of 1000 fathoms has, I think, been generally taken by recent writers as the approximate limit of the continental elevations, the space outside this being regarded as oceanic; the islands which rise from depths of over 1000 fathoms would on this view be necessarily classed as oceanic, and as a matter of fact all such islands come within the terms of Sir A. Wallace's definition of an oceanic island except that a few of them are not entirely of volcanic or coralline composition. To exclude all the islands which rise from within the 2000-fathom limit would necessitate the division of oceanic islands into two classes, the definition of which would be difficult.

I am not saying that such a distinction would be incorrect, or that Mr. Fisher has no right to assign larger limits to the continental elevations and narrower limits to the oceans: I only desire to show that he takes a special view, and that he declines to regard islands which rise from less than 2000 fathoms as specimens of the sub-oceanic crust. His discussion of the probable structure of the *sub-oceanic* crust deals therefore with areas which are covered by water of three miles or more in depth—that is to say, from about 2500 to 5000 fathoms, and the comparison which he makes between patches of sub-continental and sub-oceanic crust is really between a piece of continental land and a piece below an area of deep ocean at a considerable distance from the continents.

With regard to this point, I have had the advantage of a further explanation from Mr. Fisher; writing to me he says:—"My sub-oceanic patch may be anywhere under the ocean, but you must remember that all the quantities are subject to change except c, ρ, μ, σ, as δ diminishes; *i.e.* as the ocean grows shallower toward the coast-lines, the thicknesses and densities merge into those at the sea-level, the second layer of the sub-oceanic crust at the same time thinning away to nothing. You are quite right in thinking that in a general way in discussing the sub-oceanic crust I am dealing with the crust at a considerable distance from the continents. . . . I do not profess to explain the structure of the crust of the earth in those parts which appear to have sometimes been land and sometimes sea. I should, however, guess that having been at times land the crust there resembles the present continental crust. Still the equations (p. 242) must apply to these parts if only we knew what assumptions to make."

Since, therefore, there are regions of sub-oceanic crust the structure of which may resemble that of the continental crust rather than that beneath the central parts of the oceans, it is clearly of importance to consider the position and extent of these regions. Let us first take that part of the Pacific Ocean in which New Caledonia is situate; if we are to regard it as a submerged plateau which may once have been continental land, it acquires a special interest. The contour of 2000 fathoms which unites New Caledonia to Australia and New Zealand extends from the north coast of New Guinea by the Solomon Islands to Samoa, and then bends southward to New Zealand, but curves out again

so as to include the Chatham and Antipodes Islands, some 600 miles to the south-east of New Zealand. Southward it has a connection with the Antarctic continent, but a deep gulf of over 2000 fathoms runs far up outside the east coast of Australia. The area within the 2000-fathom line measures about 2500 miles across its northern portion, and has an extreme length of about 3600 miles from its northern border to the south end of New Zealand.

If this large area is not to be regarded as strictly oceanic—that is to say, if the physical structure of the crust beneath it differs from that of the crust beneath the deeper ocean outside it—and if its geological history is different from that of this deeper oceanic area, and is comparable with that of a continent, then a very important modification is introduced into the theory of the permanence of oceans and continents.

We learn that an area now covered with oceanic deposits may not have been always ocean, and this is precisely what Lyell and his followers have always maintained; for if so large a part of the Pacific may have been land (say in the Cretaceous period), there has been what most geologists would consider to be a change from continental to oceanic conditions; and if, being such a transmutable region, it may eventually be raised again till large parts of it become land surfaces, round which shallow water deposits could be formed, it would exhibit strata of deep-sea origin (usually called oceanic) intercalated between formations of the ordinary continental type.

Another region where similar transmutations appear to have taken place is that of the West Indian Islands with the adjoining area of the Caribbean Sea and a portion of the Western Atlantic. Of this region the structure of Barbados is an illustration. That island conforms to the ordinary definition of an oceanic island; it is separated from South America and the rest of the Antilles by water of over 1000 fathoms, and the scanty fauna which it possesses is not such as would have been introduced by any former land connection. Its geological structure is simple but striking: there are no volcanic rocks, but a basal series of sandstones and clays that are similar to the older Tertiaries of Trinidad, and may be regarded as testifying to a former northern extension of the South American continent; above these are oceanic deposits, consolidated radiolarian and foraminiferal oozes, which appear to be of very late Tertiary age (Pliocene or Pleistocene). Capping the whole are raised coral reefs. Here, therefore, is part of a continental (or shallow sea) area which has sunk into oceanic depths during the Tertiary period, has received a burden of oceanic deposits, and has risen again to be invested with a formation of essentially shallow water origin. Certainly geologists have no proof of greater geographical changes than this, though Europe affords evidence of quite as great a change, for in the area of the European chalk we have an instance of similar oceanic conditions to those under which the Barbados earths were deposited; yet this area was continental land before the Cretaceous period, and has again become so since that period.

The other oceanic areas which have less than 2000 fathoms of water over them are the Arctic Ocean, the southern part of the Indian Ocean, and part of the North Pacific between America and Kamchatka. It would appear then that we may claim these regions, together with the Caribbean area and a large part of the Western Pacific, as areas which have been interchangeable with the present continental surfaces.[1]

Mr. Fisher does not discuss the subterranean structure of the shallow ocean areas, but in his letter already quoted he inclines to think that the crust beneath them is similar to the continental crust, and this view is borne out by the structure of certain oceanic islands; but though the density and general structure of the crust may be similar to that of the continents, the condition of the liquid substratum may not be exactly the same, or rather there may be differences in the force and direction of the convection currents which traverse the substratum.

In chapter xxiv. Mr. Fisher does briefly consider the condition of the substratum in the tracts that lie between the continents and the [deep] oceanic regions. Having shown that, if the density of the substratum is less beneath the ocean than beneath the land, the convection currents must rise beneath the oceans and descend beneath the continents, he points out that there must be a certain space between the lines of ascent and descent where the currents will move more or less horizontally. In this horizontal movement he finds a force capable of exerting strong pressure on the continental crust. Now in some parts of the world the space along which these horizontal currents move may be narrow, but in others it is probably broad: thus, on the east side of the Pacific, where the change from ocean depths to mountain heights is rapid, this space is doubtless small, but on the west side of the same ocean, as we have seen, there is a broad intervening area of shallow ocean, and beneath this the currents that move westward may continue to be mainly horizontal till they reach Australia.

[1] The ridges in the Central and Southern Atlantic do not come within the category of shallow oceans.

The behaviour of convection currents is so little understood that one cannot predicate much about them; there would probably be a certain play of ascending and descending currents beneath the broad semi-oceanic area as well as horizontal currents, and very slight changes may cause these to vary in volume and to alter their positions; such a region is therefore likely to be in a state of unstable equilibrium, and its upheaval or further subsidence would depend on the balance that is established between the three sets of currents in the liquid substratum beneath it.

Another question suggests itself—namely, whether the oceans have always been as deep as they are now. According to Mr. Fisher's results, the mass of the sub-oceanic crust is greater than that of the sub-continental crust, but he gives reasons for thinking that its thickness is not greater, and if this is so, then its density must be greater; and it is from this he deduces the permanency of the oceans, because it is difficult to conceive of the denser crust becoming less dense, which would be necessary before any part of it could be converted into a continent. But though this difficulty certainly exists, it does not preclude the possibility of the sub-oceanic crust having been originally less dense than it is now; it may have been growing denser, and there may have been a corresponding increase in the size and depth of the oceans at the expense of the continents. His results, in fact, do not involve the permanency of the present continents, or of the present relative proportions of land and water surfaces. We are at liberty to imagine a time when there was much more land than there is at present, and when all the oceans were comparatively shallow; there being at this early period less difference in the comparative density of the sub-oceanic and sub-continental crust.

We may, in fact, postulate a secular increase in the size of the oceans and in the depth of the ocean basins corresponding to a secular increase in the density of the sub-oceanic crust; and possibly as a consequence a general increased stability of the whole crust.

The supposition of a secular increase in the depth of the oceans is in accordance with the evidence of geological history, for if there had been such an increase we should expect to find that oceanic deposits of the modern type were essentially Neozoic formations, and would not occur among Palæozoic rocks; and such appears to be the case. At present we do not know of the existence of any purely oceanic limestone that is older than the Cretaceous period; and among the Palæozoic rocks there are none which appear to have been formed at any great distance from continental land.

I think it has now been shown that Mr. Fisher's conclusions do not give unqualified support to the theory of the permanence of oceans, but that, on the contrary, they are consistent with two important limitations of the theory—limitations which had already been suggested by geologists before the publication of Mr. Fisher's book. Thus, Prof. Prestwich has expressed the opinion[1] "that it is only the deeper parts of the great ocean-troughs that can claim the high antiquity which is now advocated for them by many eminent American and English geologists"; and I have suggested the probability that "the tendency of all recent geographical changes has been to deepen the ocean-basins, and to raise the mountain-peaks to higher and higher elevations."[2]

It is therefore satisfactory to find that the results of purely physical and mathematical reasoning, on the one hand, and of a consideration of the geological evidence, on the other hand, are so closely in accord. The importance of this agreement consists in the way it opens for the reconciliation of two opposing geological schools: an important limitation is imposed on the Lyellian belief in the past interchange of oceanic and continental areas; while the extreme view, held by Dana and others, that there has been no such interchange at all, may be equally far from the truth; the probability being that truth lies midway between the two extremes.

It is also worthy of note that the hypothesis of a secular increase in the depth of the oceans and the heights of the moun-

[1] "Geology," vol. ii. p. 547.
[2] "The Building of the British Isles," p. 334.

tains brings the whole succession of past geological changes within the scope of a general theory of geographical evolution.

A. J. Jukes-Browne.

The Composition of the Chemical Elements.

My excuse for troubling your readers with this well-worn theme is that a definite hypothesis is possible, which, should it be fully borne out by the facts, appears to afford a remarkably complete explanation of the periodic law, as set forth in Prof. Mendeleeff's table.

The periodicity exhibited by this table is double, alternate series presenting members which have high or low atomic volumes, are fusible or infusible, &c.

Should the elements be really simple atoms, it would be impossible to account for this fact without introducing occult differences of quality, from which it has been all along the aim of chemical science to free itself. Undoubtedly periodical variations in the size and shape of the atoms might account for the dual periodicity of their properties, but nothing satisfactory can be gleaned from such an explanation. Besides, we are accustomed to regard differences of properties in compounds as dependent on composition, even should their molecular weights be similar. It may also be urged that, if the elements are supposed single, their properties should vary with increase of weight in some continuous manner, and not sway to and fro so remarkably. I am aware that Prof. Mendeleeff himself does not take this view (cf. Chem. Soc. Journ., October 1889), but it is one that is widely spread, and is held by other eminent chemists.

It is, however, possible to push too far such analogies as that of a series of organic compounds. Important differences exist between such a series and that of a natural family of the elements: for example, the specific refraction equivalents are not at all analogous in the two cases. Specific heat determinations show that, as a rule, an element moves as a single solid mass. But these considerations need prove nothing more than that we must be prepared to deal, in the case of the elements, with affinities of a different order—perhaps brought into play by vastly different conditions—from those found in ordinary compounds.

If the elements are assumed to be composite radicles, then, in stating their hypothetical composition, there is material ready to hand. The famous principle known as "Occam's razor" applies here as elsewhere. Hypothetical elements should only be introduced where other considerations are plainly in favour of the suppositions involved.

The elements form natural families of two groups each, six of them having for their types the following: Li, Be, B, C, N, and O.

Since the properties of the typical element run all through the members of a family, then (on the hypothesis that properties depend upon composition) we should expect it to be found in the formulæ of the remainder.

The hypothesis here advanced is, that the periodicity of the properties of the elements is due to the dependence of the properties of each element upon those of the typical element of the family to which it belongs, together with the mode of its combination with oxygen. In other words, that the elements, with the exception of the first six, are, in a qualified sense, compound oxygen radicles.

The reasons for the adoption of oxygen are: (1) the remarkable coincidence of the figures for each family upon this hypothesis; (2) that the atomic weights of the oxygen family of elements are whole multiples of that of oxygen; (3) the relations disclosed between the numbers of atoms composing the elements, which cannot be other than the result of law; and (4) the fact that all the elements combine with oxygen, which is also the most plentiful element in Nature.

Supposing any natural family complete, its two groups are given by the following formulæ, R being its typical element:—

$$R\begin{cases}\text{Group }(a): RO_2,\ RO_5,\ RO_8,\ RO_{11},\ RO_{14}.\\ \text{Group }(b): RO,\ R_2O_3,\ R_2O_6,\ R_2O_9,\ R_2O_{12}.\end{cases}$$

The seventh and eighth families are very incomplete, but may be represented in the same way.

It will be noted that the numbers of atoms in these formulæ are as follow:—

$$1\begin{cases}3,\ 6,\ 9,\ 12,\ 15.\\ 2,\ 5,\ 8,\ 11,\ 14.\end{cases}$$

The common difference in each group being 3, and the numbers 4, 7, 10, and 13 being absent.

The resemblance of these figures to the atomic weights of the ten typical elements (including four hypothetical ones) is very close. One is almost tempted to regard them as the primitive forms of the combination of matter, and to return to Prout's hypothesis.

The existence of *four* elements between H and Li is indicated as well by the gap which exists between them as by this hypothesis. That Fe, Co, Ni, &c., have formulæ commencing with R_2, is shown by the fact that they recur regularly in the series having these formulæ, their comparative infusibility and low atomic volume indicating also this composition, as well as the fact that, if it were otherwise, the rule observable in the first six families would be broken through. It is, again, hardly possible to suppose that the seventh family, the halogens, should contain the electropositive hydrogen, although the latter would then lose its unique position, and in this case the *difference* between the calculated values of Ag and I (18·9) agrees very nearly with that between those observed (18·87), the *ratio* of these latter being very exactly determined by Stas. This, however, is a matter which may well be left undecided for the present. Should fluorine be a fundamental element, the halogen series will break the rule which holds for at least six out of the remaining seven families.

The following table is constructed on the lines of Mendeleeff's. The seventh and eighth families are placed first in order, and the calculated and observed atomic weights are placed underneath their respective formulæ. Want of data is indicated by blanks, but the rarer metals are omitted, although they mostly correspond to the formulæ R_2O_9. It will be noted that the arrangement gives Mn, Fe, Co, Ni, and Cu an intelligible position in the series.

It is not to be expected that the calculated and observed figures will perfectly agree, although in some thirty cases the average variation is 0·5 of a unit. The chief variations occur in two series, in which, however, the natural order is preserved, viz. Ti, V, and Cr, with an average error of 4·5, and all the elements containing O_{12}, from tungsten to bismuth, in which the mean difference is 9. It will be noted that this difference holds even in the case of the eighth family, in which the formulæ contain the hypothetical R^{ii}, R^{iii}, and R^{iv}, showing that the errors arise from a common cause. The atomic weights, since the discovery of the periodic law, have not been decided upon without reference to one another. This whole series is separated by a huge gap from the rest of the atomic weights, which is only filled in at intervals by the less common metals of the earths, &c., and consequently an error in one of them would certainly affect the whole. Similarly, the differences of 4 between the observed atomic weights of Ca and Sc, and Sc and Ti, are anomalous.

On the other hand, the coincidences exhibited by the table cannot be the work of chance, and, considering the inexactitude of the determinations of many of the atomic weights, the fact that the average of the differences between the observed and calculated numbers in the large majority of the elements is only one unit, and that the remainder appear to arise from a single cause, is remarkable, especially when we consider the facts which are brought to light by this mode of representation. The law that elements essentially similar differ only by an atomic weight of O_3, or its multiple, surely deserves attention. When, again, the difference between the two groups of any natural family, and the periodicity of the properties of the elements, are exhibited as the result of composition, the conclusion becomes apparent that we have in the hypothesis at least a guide for future research.

The atomic volumes of the groups commencing with RO are smaller than in those commencing with RO_2. These correspond to the "even" and "odd" series of Mendeleeff. Other properties follow, thus affording a possible clue as to *how* the characteristics of the elements depend upon their composition.

Without trespassing further upon your valuable space, I will conclude by quoting Dr. Gladstone (Pres. Address, Chemical Section, Brit. Assoc., Southport, 1883):—

"The remarkable relations between the atomic weights of the elements and many peculiarities of their grouping, force upon us the conviction that they are not separate bodies created without reference to one another, but that they have been originally fashioned, or built up from one another, upon some general plan. This plan we may hope to understand better; but if we are ever to transform one of these supposed elements into another, or to split up one of them into two or three dissimilar forms of matter, it will probably be by the application of some method of analysis hitherto unknown."

	VII.	VIII.			I.	II.	III.	IV.	V.	VI.
3	H I R^{i} 3 F = $R^{i}O$ 19 19	R^{ii} 4 — $R^{ii}O$ — —	R^{iii} 5 — $R^{iii}O$ — —	R^{iv} 6 — $R^{iv}O$ — —	Li 7 Na = LiO 23 23	Be 9 Mg = BeO 24 25	B 11 Al = BO 27 27	C 12 Si = CO 28 28	N 14 P = NO 31 30	O 16 S = O_2 32 32
4	Cl = $R^{i}O_2$ 35·5 35	$R^{ii}O_2$ —	$R^{iii}O_2$ —	$R^{iii}O_2$ —	K = LiO_2 39 39	Ca = BeO_2 40 41	Sc = BO_2 44 43	Ti = CO_2 48 44	V = NO_2 51 46	Cr = O_3 52·5 48
5	Mn = $R^{i}_2O_3$ 55 54	Fe = $R^{ii}_2O_3$ 56 56	Co = $R^{iii}_2O_3$ 58·5 58	Ni = $R^{iv}_2O_3$ 58·5 60	Cu = Li_2O_3 63 62	Zn = Be_2O_3 65 66	Ga = B_2O_3 70 70	Ge = C_2O_3 72 72	As = N_2O_3 75 76	Sc = O_5 79 80
6	Br = $R^{i}O_5$ 80 83	$R^{ii}O_5$ —	$R^{iii}O_5$ —	$R^{iv}O_5$ —	Rb = LiO_5 85 87	Sr = BeO_5 87 89	Y = BO_5 90 91	Zr = CO_5 90·5 92	Nb = NO_5 94 94	Mo = O_6 96 96
7	— $R^{i}_2O_6$ — 102	Ru = $R^{ii}_2O_6$ 103·5 104	Rh = $R^{iii}_2O_6$ 104 106	Pd = $R^{iv}_2O_6$ 106 108	Ag = Li_2O_6 108 110	Cd = Be_2O_6 112 114	In = B_2O_6 113·5 118	Sn = C_2O_6 118 120	Sb = N_2O_6 120 124	Te = O_8 126·5 128
8	I = R O_8 126·5 131	$R^{ii}O_8$ —	$R^{iii}O_8$ —	$R^{iv}O_8$ —	Cs = LiO_8 133 135	Ba = BeO_8 137 137	La = BO_8 139 139	Ce = CO_8 141 140	Di = NO_8 145 142	— O_9 — 144
9	— $R^{i}_2O_9$ — 150	— $R^{ii}_2O_9$ — 152	— $R^{iii}_2O_9$ — 154	— $R^{iv}_2O_9$ — 156	— Li_2O_9 — 158	— Be_2O_9 — 162	Er = B_2O_9 166 166	— C_2O_9 — 168	— N_2O_9 — 172	— O_{11} — 176
10	— $R^{i}O_{11}$ — 179	$R^{ii}O_{11}$ —	$R^{iii}O_{11}$ —	$R^{iv}O_{11}$ —	— LiO_{11} — 183	— BeO_{11} — 185	— BO_{11} — 187	— CO_{11} — 188	— NO_{11} — 190	W = O_{12} 184 192
11	— $R^{i}_2O_{12}$ — 198	Ir = $R^{ii}_2O_{12}$ 192·5 200	Pt = $R^{iii}_2O_{12}$ 194·5 202	Os = $R^{iv}_2O_{12}$ 195 204	Au = Li_2O_{12} 196 206	Hg = Be_2O_{12} 200 210	Tl = B_2O_{12} 204 214	Pb = C_2O_{12} 206·5 216	Bi = N_2O_{12} 210 220	— O_{14} — 224
12	— $R^{i}O_{14}$ — 227	$R^{ii}O_{14}$ —	$R^{iii}O_{14}$ —	$R^{iv}O_{14}$ —	— LiO_{14} — 231	— BeO_{14} — 233	— BO_{14} — 235	Th = CO_{14} 232 236	— NO_{14} — 238	Ur = O_{15} 240 240

Mirion Terrace, Crewe, October 25.

A. M. STAPLEY.

Is Greenland our Arctic Ice Cap?

THE result of Dr. Nansen's journey across Greenland, establishing, as it practically does, that this Arctic continent is covered by a huge ice cap, promises to be a matter of some interest in several ways.

Among other things it may possibly yield a clue as to the cause of the south polar cap of Mars being so very excentrically placed.

Since the time of the elder Herschel this has been a subject of speculation, and various ingenious suggestions have been put forward by astronomers to account for the presumed anomaly.

Webb, in his "Celestial Objects," p. 147, tells us that Herschel found that the caps were not opposite each other; and says himself that "one would expect that they might have been diametrically opposite."

"Mädler and Secchi found the north zone concentric with the axis, but the south considerably excentric"; and "it has been suggested by Beer and Mädler that the poles of cold may not coincide with the poles of rotation."

Later on, at p. 148, he tells us that "Secchi found the appearances at the poles irreconcilable with the idea of circular caps, and was forced to adopt the supposition of complicated and lobate forms. Schiaparelli alludes to the possibility of a mass of *floating ice*."

Apparently it was taken for granted that the *ice* or snow caps of Mars, should not only be truly circular in form, but centrally placed over the axis of rotation, like the cloud caps of Jupiter and Saturn.

But it seems to me that Dr. Nansen's journey will go a long way towards solving this problem, by demonstrating that Greenland is practically one of our two polar ice caps. On our South Pole we have one, more or less centrally placed over the axis of rotation, and which certainly does not float about, having two large active volcanoes on it. It corresponds fairly well to the northern pole of Mars. But on our North Pole—as far as we can see—there is no large permanent ice cap, and in its place we have an irregular, extensive polar basin.

Roughly speaking, we may say that the character of the Arctic and Antarctic ice bears this out, for in the south we see the immense flat-topped bergs of 2000 feet thickness, and several miles long, which are obviously portions of the southern ice cap broken adrift. In the north we see a preponderance of floe, or thin field-ice, a few flat-topped bergs near Franz Joseph Land (Young), and the angular bergs of the Atlantic, mainly from West Greenland (Greely).

If our Arctic basin is deep and has few islands in it, it stands to reason that a permanant ice cap could not form, or become anchored, there; the floe would be perpetually broken up by storms and tides, carried away, and melted. A floating ice cap would be impossible. The presence of a polar continent—even excentrically placed—would seem to be necessary, as in the case of Greenland. This would indicate the solution for the supposed anomaly, *re* the position, of the south polar cap of Mars, and for the lobate appearances remarked by Secchi in 1858.

If the foregoing remarks are at all likely to be correct, Dr. Nansen's journey may have quite unexpectedly solved for us an interesting astronomical problem, and thereby afforded another clue to the condition of Mars, a proof almost of partial glaciation.

I believe that M. Fizeau regards the so-called "canals" as evidence of the "movement and rupture" of a glacial crust.

But if this crust is formed on, and attached to, any extensive *land* surface (such as Greenland, say), it is not easy to account for such enormous ruptures, and the lateral movement.

If the canals are looked on as huge lanes of open water in a floating ice-pack, they would vary in size and form almost daily.

Sibsagar, Assam, India, September 25. S. E. PEAL.

Globular and other Forms of Lightning.

MR. A. T. HARE's account in NATURE, vol. xl. p. 415, of a flash of globular lightning seems to illustrate so well the explanation which I gave, many years ago, of the formation of fire-ball lightning, that the following extract from my pamphlet "On Atmospheric Electricity" (London, Hardwicke, Piccadilly, 1863) and the remarks which I have appended to it, may perhaps not be without interest at the present time. The pamphlet is not now on sale. The quotation is from pp. 45–46; I omit a few references:—

"A slip of tin-foil was formed into a hollow cylinder, and thrust tightly into one end of a glass tube which was about $\frac{7}{10}$ inch in external diameter, and the glass was not very thick. A brass ball was fixed to the end of the glass tube, and the tin-foil extended from the ball to the distance of about $12\frac{1}{2}$ inches from it, and all the tin-foil was inside the glass tube. The remainder of the glass tube served for an insulating support to the part which held the tin-foil. On electrifying the ball, the electricity is conveyed by the tin-foil to the inside surface of the lined part of the glass tube; and at the same moment the outside of this part of the tube is electrified inductively, and with the same sort of electricity as that with which the interior of the tube is charged. The part of the tube which held the tin-foil was supported horizontally. There was also a copper hook which could be set on any part of the outside of the lined portion of the glass tube.

"The copper hook was set at a distance of $7\frac{1}{2}$ inches from the brass ball on the end of the tube, and was connected with the outside of a Leyden-jar which was charged so as to be nearly able to give a spark $\frac{1}{2}$ inch long between two other brass balls each of which was $1\frac{1}{4}$ inch in diameter. The knob of the jar was next brought to the ball on the end of the glass tube; the discharge readily passed over the $7\frac{1}{2}$ inches of the electrified outer surface of the glass tube. Sometimes the spark could pass when the hook was at $8\frac{1}{2}$ inches from the ball. When the hook was placed at a distance of $12\frac{3}{4}$ inches from the ball, the spark passed between the ball and the hook with a much lower charge in the jar than was necessary to produce a spark $\frac{3}{4}$ inch long between the pair of balls before mentioned.

"These experiments show that the length of an ordinary electric spark, can be much increased by causing the spark to pass over an electrified surface. Instances of this are seen in the spontaneous discharge of Leyden-jars, and in the long sparks which flash over the revolving glass of the electrical machine.

"Let a ball be attached to the prime conductor of the electrical machine so that the ball may give electrical brushes to the air. Much longer sparks may be drawn from the ball along the path of the brushes than from the other parts of the prime conductor. The brush discharge electrifies the air in the neighbourhood of the ball, and the spark is longer because it passes near to, or through, a mass of previously charged particles.

"It is well known that atmospheric electricity not unfrequently forms an electric fire-ball which moves but slowly, and which, on striking an object, explodes and produces all the usual effects of a flash of lightning. Sir William Harris writes:—'Now, it is not improbable that, in many cases in which distinct balls of fire of sensible duration have been perceived, the appearance has resulted from the species of brush or glow discharge already described, and which may often precede the main shock.' And Dr. Noad says of the electrical fire-ball that 'it is no doubt always attended by a diffusely-luminous track; this may, however, be completely eclipsed in the mind of the observer by the great concentration and density of the discharge in the points immediately through which it continues to force its way.' A more perfect explanation can, as I suppose, be given by the aid of the experiments of this chapter.

"A thunder-cloud may produce both the electric glow and the electric brush, at the end of one of its cloudy branches. And since electricity passes freely along a charged surface, therefore the glowing discharge by electrifying the air in front of the aërial conductor, adds continually to the length of the conducting column, and so the electrical fire-ball advances. Little drops of water, or any other conductive matter which the column finds in its course, must facilitate the transmission of the electricity to the fire-ball; and without doubt, too, the electricity of the column continues to spread laterally, and so it increases the conductive capacity of the column. The electricicity travels through the electrified column as a series of luminous disruptive discharges; but the light is brightest at the head, because there the diameter of the column is least, and the discharge is most closely packed; and because there the air is unelectrified, and consequently opposes so great resistance to the passage of the electricity. As soon as the fire-ball has arrived at a conducting mass on the earth, the aërial conductor has been completed, and a flash of lightning may instantly follow along the path of the fire-ball."

Since the Leyden-jar, with a charge somewhat less than that required to give a spark $\frac{1}{2}$ inch long between the $1\frac{1}{4}$-inch brass

balls, gave a spark about 8 inches long over the excited glass tube; and since the Leyden-jar, with a charge much lower than that required to produce a spark ¾ inch long between the two brass balls, was sufficient to give a spark about 13 inches long over the excited glass tube; it was at once seen that the length of the spark over the excited glass tube, increases faster than the intensity of the charge of the Leyden-jar. Of course the law which connects the length of the spark over the excited glass tube, with the intensity of the charge of the Leyden-jar, can only be determined by experiment. It is, however, to be noticed that, from the experiments of Harris and others, the length of a spark in air of a Leyden-jar varies directly with the intensity of the charge—that is, with the quantity of electricity in the jar as measured by any such contrivance as the unit-jar. And further, that the length of the spark over the excited glass tube depends (1) on the length of the spark which the charge of the Leyden-jar can produce between the 1¼-inch brass balls; and also (2) on the degree of electrification of the glass tube; and that both these two quantities—namely, (1) and (2)—increase together. From these considerations, I should expect to find that the length of the spark over the excited glass tube increases in some way with the square of the intensity of the charge of the Leyden-jar—that is, with the square of the potential.

I dare say that the sparks over the excited glass tube, would become very brilliant by using an induction coil to charge the Leyden-jar. But to produce the maximum effect, the glass tube should, I think, be lined, as in the following experiment, with tin-filings instead of the tin-foil.

A piece of hard German glass tube was taken, and one end closed at the blow-pipe and the other end bordered to receive a cork. After these operations, the tube was found to be just 2 feet 2⅜ inches long; the external diameter of the tube was 1⅛ inch, and the glass was $\frac{1}{20}$ inch thick. Next, the closed end of the tube was filled with tin-filings to the height of 6 inches, the filings having been condensed by tapping the end of the tube on a piece of wood. A brass rod, with a knob at one end and a screw having been cut on the other end, was screwed into a cork which nicely fitted into the glass tube, and, by means of the rod, the cork was thrust into the tube until it pressed upon the tin-filings, and since the point of the rod was sharp and projected beyond the cork, the end of the rod entered a little way into the tin-filings. The knob of the brass rod now stood just at the mouth of the glass tube, and the mouth of the tube also contained a cork through which the brass rod passed. Of the outside of the glass tube, the part surrounding the tin-filings was painted over with lac varnish, and, as soon as it became sufficiently sticky, a thin piece of tin-foil was wrapped around the tube so as to cover the tin-filings, and no more. Lastly, the remaining portion of the outside of the glass tube was painted over with lac varnish. To charge this tubular Leyden-jar, it was laid with the tinned end on one conductor and with the knob of the brass rod on the other conductor of a Wimshurst influence machine. I may mention, in passing, that the capacity of this tubular Leyden-jar was surprisingly great in comparison with its size; thus showing that Leyden batteries, both cheap and compact, can be made with the aid of glass tube and metallic filings. The capacity is no doubt due, more or less, to the uniform thinness of the glass, and to the close contact of the tin-filings and the glass. The specific inductive capacity of hard German glass does not seem to have been ascertained. But of course, for the construction of Leyden-jars, and also for the plates of the Wimshurst machine, glass of the highest available specific inductive capacity should be used. It may not be amiss to remark that, owing to the high specific inductive capacity of glass as compared with air, the efficiency of a Wimshurst machine is probably much more increased by diminishing the thickness of the stratum of air between the glass plates than by diminishing the thickness of the plates.

Now, the Leyden-tube produces a class of sparks which I do not think have been shown by any other Leyden-jar. The Leyden-tube was laid, as before mentioned, on the two conductors of a Wimshurst influence machine, and the discharging balls belonging to the conductors were set ½ inch apart. These two discharging balls were each 1⅝ inch in diameter. On turning the handle of the machine, the Leyden-tube continued, of course, to become charged and then to be discharged by the ½-inch spark between the discharging balls. But besides the main spark between the discharging balls, little streams of electricity appeared along the glass tube, and extended away from the tin-foil to a distance of 1½ inch or more. These sparks were, I think, best seen in a subdued daylight. They were very numerous with each discharge of the tube; I estimated the number of sparks in different discharges as varying between one and two dozens. The sparks were sinuous, very bright at the tin-foil, and tapering away to nothing at the further end. Some of the sparks, however, were not so bright as the others, and rather ruddy; they were probably inside the glass tube, and coloured by the varnish on the tube.

In the *Leisure Hour*, November 1888, p. 777 (56 Paternoster Row), there is a photographic picture of a lightning-blaze, wherein the bright ends of several of the flashes are seen to be sitting upon what appears to be rock, and the flashes bear a strong resemblance to the little sparks whose bright bases rest upon the edge of the tin-foil.

In the *Leisure Hour*, November 1886, p. 786, there is another representation of a flash of lightning from a photograph. In this instance, the flash is thick in the middle, but on approaching the earth, it tapers off to a fine point. Like as a river may be only a small stream at its source and by gathering water as it leads on to the sea, become a bulky stream at its mouth; so the sparks on the Leyden-tube gather up electricity from the Leyden-tube, and so brighten away to the tin-foil. But in this flash of lightning, the very reverse appears to take place. The flash is greatly weakened before it reaches the earth, through a transverse discharge to the air. For around the brighter portions of the flash, the air is shining, and streamers are darting earthwards from the flash into the air. At the upper part of the flash, there are also streamers acting manifestly as feeders from the cloud to the flash. The flash rather resembles a long spark from the prime conductor of an electric machine, than the spark of a Leyden-jar; but the prime conductor being metallic, can only imperfectly represent the much lower conduction of a cloud.

In the *Leisure Hour*, September 1889, p. 641, there is an engraving from a photograph of the so-called ribbon-lightning. This form of lightning is clearly produced by a succession of flashes following along the same path, combined with some slight motion given to the camera by the hand of the operator; as indeed is there pointed out. The question is, How comes it that the flash so repeatedly passes along the same path? The answer there given is that suggested by Mr. Cowper Ranyard, "That apparently the first flash would heat the air and slightly rarefy it, leaving a path of least resistance, along which subsequent discharges would flow as certainly as water follows the twists and turns of a pipe." It seems to me, however, that a far more important cause for making a second flash to pass along the path of its predecessor is to be found in the action of the transverse discharge, whereby a tubular mass of air becomes electrified around the path of the first flash; and through the electrified air, the flash readily passes, as previously shown. In the woodcut, the effulgence of the surrounding air and the streamers show that the lightning was distributing electricity along its path. The transverse discharge is perhaps never absent from the flash of lightning. In NATURE, vol. xl. p. 543, a flash of lightning which struck a windmill, is described as "a mass or network of flame, which threw off thousands of sparks like fireworks."

The discharging balls of the Wimshurst machine were set one inch apart, everything else remaining as before. The sparks now extended along the glass tube to a distance of about 3½ inches from the tin-foil. The general character of the sparks was the same as before, when the discharging balls were set half an inch apart.

The discharging balls were set 1⅜ inch apart. When the discharge occurred, the sparks extended along the tube to about 5½ inches from the tin-foil. The sparks were straighter, and not nearly so numerous as when the discharging balls were set at half an inch; they were also very much brighter, but like the others, they all tapered away to nothing. In this experiment, the Leyden-tube was charged to about the highest potential that the machine would give it; and the matter was not any further pursued. REUBEN PHILLIPS.

1 Bay View Terrace, Northam, Bideford, October 9.

"Darwinism."

WHAT my "laborious essay" "distinctly professes to be" is, as its title-page announces, "an *additional* suggestion on the origin of species"; and this additional suggestion is forthwith stated to be that of "*another factor* in the formation of species, which,

although quite independent of natural selection, is in *no way opposed* to natural selection, and may therefore be regarded as a factor *supplementary* to natural selection." This passage occurs in the most conspicuous part of the paper, viz. at the close of the introduction. In the next most conspicuous part—viz., at the close of the paper itself—it is said, "Without natural selection, physiological selection would be powerless to create any differences of specific type, other than those of mutual sterility, and trivial details of structure, form, and colour."

So much for distinct professions. But as I am tired of controverting the statement that I both intended and perpetrated an "attack" on Mr. Darwin's theory, I will not now burden your columns by supplying the context, or otherwise easily explaining the passages which Prof. Lankester quotes in support of this statement. On a future occasion, however, I hope to avail myself of a more fitting opportunity fully to display the relation in which my "laborious essay" stands to the work of Mr. Darwin; and then I trust it will be clearly seen that, whatever we may severally think about the "complementary principle" of physiological selection, at all events it is in no way hostile to the cardinal principle of natural selection.

Edinburgh, November 19. GEORGE J. ROMANES.

How not to Teach Geometry.

As I have come across an almost unforeseen development of the above heading, I take the liberty of bringing it before your readers. For myself, I may state that I have considered the "learn a proposition off by heart" method was sufficiently bad, but what is to be made of the method described in the following extract from a note which I recently received from my friend:—

"We have half of a proposition written on the board, and then we write it at home from memory; then the other half is written on the board, and we write that at home from memory. Then we have to learn the whole proposition at once, to be able to write or say it with different letters. We are not allowed to have a printed Euclid book—we are only allowed to have a book of Enunciations."

Of course this refers to Euc. i. 1.

I beg to commend the above extract to the Association for the Improvement of Geometrical Teaching. I do not know whether to add the name of the school where the above system is followed by one of the teachers.

HERBERT J. WOODALL.

Normal School of Science, South Kensington,
November 11.

P.S.—I should like to see opinions on the teaching described.

A Brilliant Meteor.

Is not the meteor seen from Warwick School on November 4 the same as that mentioned in the following from my daughter, written from the school at Brookfield, Wigton, Cumberland?

"On Monday night (November 4), at 7.55 p.m., when out on the playground viewing the stars, I saw a most beautiful meteor. It seemed to be very near, and was in sight for quite a long time. It appeared just over Skiddaw—that is to say, due south—and went towards the south-east. It had a long tail of light, and burst, and sent out beautiful colours, and disappeared near the horizon."

I may add that, last Sunday, November 10, at about 5.56 p.m., I saw here a very bright meteor pass from a point perhaps south-south-west, and altitude about 25°, to a point perhaps south by east, and altitude about 10° or 12°. It was brighter than Venus when the planet is at its brightest, I think; and it seemed to flash out still more brightly just before disappearing; but the colour did not change perceptibly from its former soft white light, and there was no appearance of bursting. At the time of disappearance, its train of light must have extended over several degrees.

WM. SCARNELL LEAN.

Ackworth, November 16.

THE CAUSES AND CHARACTER OF HAZE.

UNLIKE fog, haze commonly occurs in this country when the lower air is in a state of unusual dryness. It is not only a frequent accompaniment of a spell of fine dry weather, but may be, when in combination with certain other conditions, a sign of its approach. Night or morning fogs, and in winter persistent fogs, often signify a calm and settled condition of the air and the prevalence of fair weather. Heavy dews, especially in the autumn, likewise portend fine weather, but usually of shorter duration. Fogs appear usually in one of two conditions: either the air is nearly saturated up to a considerable height, or else is unusually dry, except in a stratum immediately above the ground. In the first case, radiation or condensation from some cause produces, by a slight lowering of temperature, a large precipitation of vapour; and in the second case, radiation from the earth's surface being excessive, owing to the diathermancy of the dry atmosphere, the stratum next the ground rapidly reaches its dew-point, fog is formed, and this fog continues to radiate to the clear sky and further to reduce temperature. Haze, on the other hand, appears often in weather distinguished by unusual dryness, on the surface as well as at a considerable altitude above the ground. The air remains for many days uniformly dry, the nights being nearly dewless, and the sky often free from clouds. The chief difference to be observed, then, is this, that fog requires saturation where it occurs, while haze seems to be favoured rather by a dry atmosphere.

Haze does not prevail on the continent of Europe or in the interior of North America to anything like the same extent as in England; nor, probably, in mid-ocean to the same extent as near the shores of northern countries. On the east coast of Scotland, and, indeed, over all North Britain, it is exceedingly common, especially in the spring, and during the prevalence of east wind, although with west winds the atmosphere is frequently clearer in summer than in Southern England. Over Southern England it is a common accompaniment of winds between east-south-east and north-east inclusive. It appears to prevail more on the eastern than on the western coasts when east winds are blowing. In Western Surrey, when the lower air moves from a westerly direction or is calm, the approach of east wind is announced by a light haze obscuring distant views, before the east wind has actually arrived on the spot of observation. This is not in all cases due to the descent of London smoke from a higher stratum, where the east wind first gains ascendancy, for the phenomenon may be observed in other localities. The haze produced on the first arrival of the east wind is thicker than that which remains when the east wind has gained a strong hold, and the neutral band where calm prevails between a south-west and a north-east current is marked by the thickest mist. In winter a dark fog frequently marks this neutral zone, often not more than one or two miles in breadth, and the zone moves eastwards or westwards according as the west or east wind exercises the strongest pressure. I have frequently observed this phenomenon with great distinctness. In winter, the approach of the equatorial after the prevalence of the polar current is often betokened by a damp fog and the contrary change by a dry fog; the same changes in summer are respectively marked by a great increase of transparency and by a spreading haze or mist. The following observations taken in Scotland illustrate the phenomena accompanying a change from west to east in August. St. Fillan's Hill is a small, steep, isolated volcanic cone about 300 feet in height, standing in the middle of the valley of the Earn, about two miles from the lower end of Loch Earn, in Perthshire. The air was clear, and a fresh westerly breeze was blowing when I was on the summit, about 5 p.m. The breeze suddenly began to slacken, and in about five minutes had dropped altogether. Then down the valley eastwards a blue haze began swiftly to climb the glens tributary to Strathearn, and the whole air eastwards grew obscure. The calm only lasted a little more than two minutes, and then suddenly a strong wind from the east set in, and soon the air, westwards as well as eastwards, was robbed of its transparency. The east wind

continued, and in a few minutes the tops of the hills, which rise precipitately from Strathearn to a height of about 2000 feet, were obscured with cloud-banners growing continuously and descending till in about two hours not only the hills above a level of about 1000 feet, but the whole sky, were covered with gray cloud. The duration of the neutral calm, from two to four minutes, seems to be about the usual time occupied by a moderate east wind in driving back the opposing current, according to my observations in the neighbourhood of London. In the suburbs south-west of London such a change is signalized in the neutral band of calm by a dense yellow haze, producing great darkness, the result of a banking up of smoke to some altitude, together with the condensation of aqueous vapour by the mixture of currents differing in temperature. With lighter winds about equal to each other in momentum, such a band often lasts much longer, and I have known a west wind prevail at Richmond simultaneously with an east wind in London, both without fog, while at Wandsworth, between the two, a calm continued for many minutes, with dense, almost nocturnally-black, smoke-fog, the pressure in each direction being apparently equal. Generally speaking, the mist thus produced at the junction of the two winds is exceedingly dense in winter, moderately dense in spring and autumn, and thinnest in summer, varying, in fact, from a black fog in the cold season to a mere haze in the warmest weather. Hence we have an ascertained condition for the production of haze—the mixture of two opposite winds. It may be here remarked that a very sudden squall of wind from the north, displacing an equatorial or south-westerly current, produces a somewhat similar dense wall of mist, which it soon drives away before it.

Haze very frequently prevails during a north-east or east wind in all parts of Great Britain; in the east of Scotland it is, perhaps, more marked than in other localities, and attends both wet and dry weather. A dense blue mist or haze brought by the east wind sometimes invests the landscape for days before a continuous downpour from that quarter. This haze extends far out to sea eastwards. The southern parts of England are less troubled than the northern by this disagreeable infliction, and the northern parts of France less still. In the eastern counties, and probably in other parts of England, the density of the haze seems to increase in some proportion to the dryness of the air, when only a slight wind blows. On thoroughly rainy days, such as the north-east wind sometimes brings to the London district, the amount of haze is below the average; and when the north-east wind is accompanied by snow-showers, as it often is in February and March, or by rain-showers later in the year, it is remarkably and conspicuously clear. I cannot remember any showery days with a steady north-east wind showing a true haze, beyond the influence of London, but have often observed the extraordinary clearness of such days, and the apparently dissipative action of the air on London smoke.

Generally, the density of the haze is less as the strength of the wind increases. A gale from the north-east is seldom accompanied by much haze inland, although on the east coast the combination is not uncommon. Haze appears to diminish as the north-east wind grows more established, and in winter a long period of this wind may be experienced without the continuance of haze. It is also important to observe that, when high upper clouds are seen to be moving from a direction between east and north inclusive, but especially from north-east, the air is usually clear, and a long continuance of the polar wind may be expected. It is a sign of the firm establishment of the north-east wind when high cirro-cumulus is seen passing over from that direction, whatever deviations may take place temporarily on the earth's surface. The extension of the north-east wind to a great altitude seems to deprive it of its accustomed haziness. When, on the other hand, thick haze accompanies the north-east wind, if upper clouds are in view, they are generally seen to be borne by a different current, and in winter the lower wind does not, in such conditions, often remain long in the same quarter. Hence we have the means of making forecasts with tolerable safety as follows:—

(1) If the lower air be clear, whether clouds at a high level be seen to move from the north-east or none be visible, the lower wind from north-east will probably last some days, perhaps some weeks.

(2) If the lower air be very thick and misty, the north-east wind is not strongly established, and is likely soon to be succeeded either by variable airs and calms, or by breezes from a different quarter.

In spring and summer, haze prevails sometimes for many days together, with a dry atmosphere, over the whole or a large part of Great Britain. The wind is either easterly or variable, the barometer high, temperature high by day and low by night, and the deposition of dew either small or heavy. The haze seems to be uniformly distributed through the atmosphere, and varies neither from one day to another, nor from day to night. The sky is pale blue, the sun rises and sets red and rayless, and the moonlight reveals the blue mist unchanged by the absence of the sun's rays.

Haze has been known to affect a great part of Europe during a period corresponding with the prevalence of drought.

The formation of haze seems to be more common and more sudden in mountainous regions than on the plain. I had once an opportunity of observing the rapid production of a very dense haze from the top of Cader Idris, in Wales. The morning was bright, fine, and clear, but the heat very oppressive. About midday, signs were seen of an approaching thunderstorm, which, however, spent its force at some distance down the valley. Before the storm, a haze quickly gathered, and completely obscured even the nearer ranges. This haze resembled that which prevails sometimes during many hours before the occurrence of a thunderstorm in the level country.

The conditions favourable to the production of haze may be conveniently summed up as follows:—

(1) A gentle wind from east-south-east to north-east inclusive, and east wind in general, especially with dry weather in spring and summer. If the east wind be established up to a great height, the lower air is usually clear, but if the upper current is from a westerly direction, haze prevails.

(2) Fine settled weather, with variable currents, a dry air, and little dew.

(3) Opposition of currents—such as occurs when several shallow barometric depressions exist over the country—and the atmospheric state preceding thunderstorms.

(4) Damp weather, with light winds and varying temperature, as thaw after frost, with snow on the ground.

Turning to those conditions which are most unfavourable to the production of haze, or in which the air is most transparent, we find them to be—

(1) A state of great humidity, such as that which occurs often before bad weather, the wind being between south and west.

(2) Strong winds and showery weather.

(3) Winds between south-west and north.

(4) Fine settled summer weather, with westerly or southerly winds.

(5) Settled easterly or northerly winds, with either clear sky, or high clouds moving from those directions.

(6) Easterly or northerly winds, with a high continuous cloud canopy moving in the same direction, small range of temperature, and steady conditions; or, with detached cumulus in the daytime, and clear nights.

(7) North-west following a wind between north-west and south is particularly clear, except in thundery weather.

It thus appears that the most striking characteristic which may accompany the formation of haze is an unusual dryness of the air, and that a total absence of haze is often observed when the air is unusually charged with vapour. It does not follow that haze, or a light fog much resembling it, is not also seen in a damp state of the air, or that a saturated air is always free from haze; indeed, something much resembling a dry haze does occur with sudden changes of temperature in all ordinary hygrometric states in our climate. But the very condition to which haze in England is commonly, and in a certain sense correctly, attributed—namely, atmospheric humidity—is, if sufficiently uniform and extended, least favourable to its manifestation. A constant moisture-laden westerly breeze would give a climate nearly as clear as that of the south-west corner of France.

Two principal factors go to the production of ordinary haze: the first, a rather large amount of vapour between the earth and a great altitude, say 60,000 feet; and the second, a mixture of two heterogeneous masses of air. Evidence of the correctness of this proposition is to be found in the geographical distribution of haze and the state of the winds when it occurs.

The causes of fog are either radiation of heat from the earth into space and cooling of the overlying humid strata of air to a temperature below the dew-point, or else the mixture of two winds, differing in temperature and other conditions, one of the currents being usually near its point of saturation previous to contact with the other.

If the above-mentioned statement of the causes of haze be correct, we shall be enabled to account for the appearance of haze in certain conditions, which have been given, and for its absence in others. Taking them in order—

(1) A gentle wind from east to north-east inclusive is favourable to haze, especially if it extends to no very great height. Often the approximate depth or height of the easterly current is difficult to ascertain; but, in general, if it be of short duration, it is shallow, and sometimes upper clouds from a westerly direction may be observed. In these cases especially haze prevails. Considering the shallowness of lower winds compared with their extent—an easterly wind, for instance, which has travelled 300 miles beneath a westerly wind only four miles above the earth's surface—it is quite certain that a very large admixture of the two currents must take place. And we may be sure that in the majority of cases the easterly surface wind has above it an upper current from a westerly direction. Mr. William Stevenson (*Edinburgh Philosophical Magazine*, July 1853) observed the cirrus cloud at Dunse, Berwickshire, for eight years, and from his summary of the direction of the motions of that cloud we derive the following figures:—

	Per cent.
Direction of motion of cirri from between south-west and north-west inclusive	75·2
Direction of motion of cirri from between north and east inclusive	10
Other directions	14·8
Direction of wind at surface of the earth from south-west to north-west inclusive	54·6
Direction of wind at surface of the earth from north to east inclusive	32·4
Other directions	13

Thus there remains a difference of over 20 per cent. excess of westerly upper current over westerly surface wind, and at the level of the cirrus a wind between north and east only prevails once to every three occasions of a surface wind from that quarter. The significance of these figures is not seriously affected by the idea, first suggested by Admiral Fitzroy, that visible cirrus is less likely to form in the polar than in the equatorial current, and any careful observer can easily satisfy himself that westerly winds are more common and easterly winds less common at the cirrus level than on the surface. Mr. Buchan ("Handy Book of Meteorology," p. 230) remarks that, as the north-west current advances into southern latitudes, the increasing heat of the sun will tend to dissolve the cirri which mark its course, and he therefore thinks that the north-west upper current is the most prevalent in Great Britain. The actual numbers obtained by Mr. Stevenson during the eight years were 243 for north-west, and 256 for south-west direction of cirrus.

Mr. Ley ("Laws of the Winds," Part I. p. 154) remarks:—"The fact, indeed, that the observed westerly upper currents prevail over the observed easterly upper currents, even more than the westerly surface winds do over the easterly surface winds, has been admitted by most of the observers who have investigated the subject in different parts of Western Europe; and the same phenomenon is noticed in similar latitudes of North America. . . . Be this as it may, the theory of prevalent polar upper currents derives no support from our own collection of examples. Again, the results of the observations classified in Table IV. appear altogether adverse to the supposition that an easterly upper current is common over the northern portions of those depression systems whose westerly winds are the strongest at the earth's surface. . . . Instead of easterly upper currents, we find a great preponderance of southerly currents."

Out of nine balloon ascents recorded in Glaisher's "Travels in the Air," in which the wind at starting from the surface was easterly, there was not one in which a different current was not encountered at a moderate elevation. The changes were as follows:—

Date.	Surface Wind.	Wind at
April 18, 1863.	N.E.	A moderate height, N.
July 11, 1863.	E.	A moderate height, N.
		5400 feet, N.N.W.
May 29, 1866.	N. by E.	Above 2000 feet, N. by W.
		5100 feet, nearly calm.
Mar. 31, 1863.	E., gentle.	Between 10,300 and 15,400 feet, W.
		About 15,400 feet, N.E.
		Higher still, S.W. and W.
Jan. 12, 1864.	S.E.	1300 feet, strong S.W.
		4000 feet, S.
		8000 feet, S.S.W.
April 6, 1864.	S.E.	About 9000 feet, N.W.
June 10, 1867.	Surface calm, low elevation N.E.	Higher, N.N.E.
		Higher still, N.
Aug. 12, 1868.	N.E.	5000 feet, S.W.
June 16, 1869.	N.E.	10,000 feet, S.W.

On one occasion—January 12, 1864—the temperature from 3000 to 6000 feet was higher than on the surface, but at 11,500 feet it was more than 30° colder—namely, 11°. A large number of balloon ascents show not only a variety of currents, but large and sudden variations of temperature within a few thousand feet.

Thus we may confidently assume, in the majority of cases of east wind, and especially when this wind is of brief duration, local, or gentle, that a westerly wind flows above it at no great distance from the surface of the earth. Considering the perpetual rapid interchanges (hardly to be called diffusion) going on in the atmosphere, the lower wind must be largely mixed with air of a different condition derived from the westerly current. If a cold dry east wind be permeated by patches and filaments, however minute, of moister and warmer air, they must be cooled by contact with the polar wind, and a slight deposition of vapour may take place. Or the countless invisible dust particles may, by increased radiation towards space through a drier air, either cause a slight deposition of moisture upon themselves or collect still smaller particles together, as dust is known to collect on cold surfaces in a warm air. If deposition of moisture take place, the dryness of the air prevents the water particles from growing to anything like the size of the

particles of a fog; a relatively small diffused quantity of vaporous air in minute parcels could not produce by condensation any but extremely small and transitory water particles, in the aggregate visible through long distances, but probably individually beyond the power of the microscope to discern. They may be compared to the blue mist escaping from the safety-valve of a boiler under high pressure: the invisible steam turns for a moment blue, and then to the ordinary white of visible steam. The haze may possibly be equally momentary in duration, dissolving long before reaching the white stage, but fresh filaments are perpetually keeping up the process and giving the appearance of a persistence like that of smoke or dust. According to Espy, every cloud is either forming or dissolving (Buchan's "Handy Book of Meteorology," p. 175).

The action of a north-east wind setting in over England would be represented by a trough of water, say 2 feet square and 2 inches deep, containing warm water flowing in one direction, while cold water enters from the whole length of the opposite side. The cold water would force its way under the warm, and the two opposite currents would continue to flow; but through friction and diffusion there would be a great deal of mixture of portions of the upper with the lower stream.

A haze similar to that accompanying the east wind is frequently seen where two currents of the same wind meet at different temperatures, as at the junction of two valleys, or at projecting headlands (Buchan's "Handy Book of Meteorology," p. 171). It is also common with a humid wind, otherwise clear, when it passes over ranges of hill and valley of moderate elevation, owing probably to the mixture of parcels of air of different temperatures by alternate upward and downward thrusts. The thin white mist which appears in gales from the south-west on sunshiny days is probably due to the forcible and rapid mixture of air warmed by the ground with colder portions from a higher level, the deposition of minute particles of dew being aided by the abnormal amount of salt carried up from the sea in spray, and borne to great distances inland.

A very good instance of the powerful influence of the mixture of two currents of air, not greatly differing in temperature and other conditions, to produce haze occurred on August 26, 1889, in southern Surrey. The wind over a wide area, including the south of England, was variable and gentle from west to north-west. At the place of observation it had been about west-north-west during the afternoon, and the views were fairly clear. Cirro-cumulus, both at a moderate and at a great elevation, moved from north-west. At about 5.30 p.m. the landscape was suddenly invested with haze, which, during the following hour, was thick enough to obscure altogether hills about six miles off. Simultaneously the wind dropped a good deal and shifted to north-west and north for a short time, but soon backed, and the air again became clear about 7.30. It would thus seem sufficient that a reduction of temperature a little more than the ordinary about the time of sunset should occur, in order to precipitate visible moisture upon the dust-particles of the air. Both the sensation and the appearance of the sky resembled that during a disagreeable misty east wind, and, just before the change, a very dark bank of cloud appeared in the north, which, on passing over, was seen to be more mist than a well-defined cloud stratum. It seems not unlikely, judging from the experience of aëronauts, that in this case a current from north or north-east was driven like a wedge into the general north-west wind a few thousand feet or less above the ground.

If the account of the formation of haze in an easterly wind given in the foregoing pages be correct, there should be a clearing of the atmosphere when either the east wind extends itself to the upper regions or the westerly wind succeeds in driving back its opponent out of the lower space. In point of fact, the air does clear itself in either of these events. Moreover, a clearing away of haze is a good indication of a strengthening of the polar current or its expulsion by the equatorial; other signs, such as the motion of cirrus and the aspect of the clouds, plainly informing us which of the two changes will occur.

(2) The second favourable state for the production of haze was given as "fine settled weather, with variable currents, a dry air, and little dew." This state prevails often with anticyclones, and the movement of the air is to a great extent vertical, an interchange taking place between upper and lower strata. Consequently, there is a great mixture of portions of air at different temperatures, with a result like that already described. The heterogeneous character of the lower atmosphere in a horizontal direction declares itself by the poor transmission of sound. But a great deal remains to be explained in the production of haze in these conditions. The cause is probably the same as that which sometimes covers the whole of the British Isles with a damp fog, extending high into the atmosphere. This occurs when two winds of a different character meet in such a manner as to interdiffuse gradually over a wide area. But in the case of haze, how can it endure when the general dryness of the air is far above the point of saturation? Haze sometimes continues in summer right through the day, when the dry and wet bulbs show a difference of 12° to 15°. It would seem as if our methods of estimating the dew-point do not altogether hold for air in a certain condition and for certain particles in it. Is it not possible that condensation to a slight degree may occur upon some minute crystalline particles, such as the salt-dust which pervades our atmosphere, at temperatures above the dew-point? Such action would only be consistent with the effect of crystals in hastening the boiling and congelation of water. It is probable that, if means were available for testing the temperature of successive minute portions or strands of air passing over a thermometer, we should find a great variation from one moment to another. A difference of 12° between the dry and wet bulbs may represent a mean between much higher and much lower values; and on the driest days, when haze prevails, there may be extremely minute portions with a temperature at the dew-point—that is, containing more vapour than, at the particular temperature to which it is a certain moment exposed, can remain uncondensed. That volumes of air at different temperatures take a long time to become thoroughly incorporated, may be regarded as certain. Threads of smoke in a still room often remain for many minutes unbroken, and behave as if they were held together by some cohesive force, and, generally, strains of air or gas at widely differing temperatures, when mixed, tend to hold together rather than to diffuse. Thus, small surfaces, of which the vapour-particles are at different temperatures, are frequently in contact. When we consider that different currents of air frequently prevail within a a few thousand feet of the earth's surface, and that within five miles a temperature of −2° may exist early in September,[1] it seems possible that, in so bad a conductor of heat as air, temperature at different points on the same level may vary greatly. On September 1 and 2, 1889, the condition of the air was instructive with regard to the formation of fog and haze. The night of August 31–September 1 was fine, and radiation rapid, so that in the morning there was a copious dew. From 6 to 8 a.m. there was thick fog, which, as the sun's power increased, lightened and lifted, but the sun did not finally break through till past 11. The wind was fresh from north-east. A thin blue haze remained after the fog had dissipated, and did not altogether disappear during the day. The air was not damp, even before the fog had lifted, though there was a very slight drizzle about 9 a.m. On September 2 the night had been very fine and clear, but in the morning

[1] See "Travels in the Air," Glaisher's ascent of September 5, 1862.

a thick wet fog, with fresh north-east wind, prevailed. This fog cleared, and the sun shone through, about 9 a.m. A mist, however, remained much later. Now, in these cases, the fog was due to the cooling of the earth by radiation (for it did not appear till after midnight) and to the cool north-east wind co-existing with higher currents from a different quarter.[1] The persistence of the haze much beyond the fog reveals the difference between a general saturation and what might be termed molecular saturation. The fog breaks, decreases rapidly, and has gone when the last few shreds of clouds lifted from the earth vanish in the blue, but the haze looks unchanging and uniform over the country. When we see volumes of vaporous air separated, without any apparent reason, into dense clouds and clear intervals, *e.g.* cumulus in a blue sky, it becomes easy to understand that very small microscopic clouds, in which condensation is only momentary, may permeate air otherwise far from saturation.

It would hardly be reasonable to exclude electricity as a possible agent in the otherwise not wholly accountable phenomena of mist and cloud. It may be that the dust-particles of two currents of air differing in electric quality or quantity may be attracted to each other, or that the mixture of currents of different temperature may in some way set up molecular aggregations.

Whatever the cause, we should bear in mind the small quantity of non-transparent matter required to produce the dimming effect of haze. If the eye can observe the colour produced in a drop of water by the fifty-millionth of a gramme of fuchsine, possibly a weight of water or dust not much greater would suffice for visibility in a column of air 1000 feet long. The atmosphere is at all times charged with dust-particles to a degree which it is difficult to realize. The purest air tested by Mr. Aitken previous to his measurements on the top of Ben Nevis, contained about 34,000 dust-particles to the cubic inch—this was on the Ayrshire coast. In every cubic foot there would be 35,232,000 particles, and, in a horizontal column of 1000 feet, 35,232,000,000 particles. It is manifest that a condensation upon a small proportion of these, or an agglomeration of a small proportion into larger groups, or a momentary adhesion by electric attraction, would suffice to produce optical effects.

The evidence concerning the appearance of haze by irregular transmission of light due to unequally heated currents of transparent air seems to be quite insufficient, and however great the heat near the surface of the ground, say in the desert, with consequent distortion of images, it does not, as a rule, bring about the haze so common in temperate climates.

Haze of an abnormal kind need barely be mentioned here—namely, that due to smoke, palpable dust, and the products of volcanoes. It may, however, be very widely spread and very dense. In 1783 Europe was for months covered by the dust ejected by an Icelandic volcano, and the Atlantic for 900 miles west of the north-west coast of Africa is every year subject to a haze composed of fine particles of sand from the Great Desert.

(3) Opposition of currents, such as takes place when several shallow barometric depressions pass over the country, results in mixture of differing air, partial condensation, sultriness, haziness, and frequently thunderstorms. Not at all improbably, the differing electric conditions of two winds, the rapid condensation of vapour, and the projection of highly vaporous air to a great height, accelerate the growth of water-particles, until they fall to the earth in large drops. The saying that thunderstorms advance against the wind is merely a way of asserting that two winds are adjacent, one above the other, and that the clouds move in the upper current. The haze preceding thunderstorms announces beforehand the contention which is going on, and the conglomeration of dust or water particles by electric attraction or rapid cooling.

(4) Damp weather with light winds and varying temperature, as thaw after frost, with snow on the ground. The cause of haze in this condition is obviously the contact of warm moist air with air cooled by contact with, and by radiation towards, the ground. In this case, again, it is mixture of portions of air of different temperatures which produces partial condensation and haze. It must be remembered that the air is always charged with an immense quantity of fine dust, such as particles of salt,[1] that these are capable of radiating, and that when they fall 1° or 2° below the temperature of the air, moisture may be deposited upon them sufficiently to become visible. In the case supposed, of an equatorial current supervening after frost and snow, the mist produced by mixture of parcels of air at different temperatures will be thin and blue if the filaments in which saturation and deposition occur are very small in proportion to the surrounding unsaturated air, and white if the proportion of saturated air is large. For the blue mist or haze indicates deposition in very minute clusters of water-molecules, and instant reversion to the invisible state by the contact of unsaturated air, while the white mist is the result of condensation in much larger quantities in air on the whole very near or at the point of saturation.

Consider next the conditions of weather in which the air is most transparent.

(1) A state of great humidity, such as that which occurs often before bad weather, the wind being between south and west. What does this clearness signify, according to the views of the causation of haze above detailed? Chiefly that the air up to a great height is fairly homogeneous—that is, of the same kind and quality as regards moisture, electricity, and temperature, with due allowance for the normal changes depending on altitude. The humidity is not owing to this homogeneity, but often accompanies it, simply because the south-west and westerly winds have passed over a large extent of ocean. In fact the air throughout has been subjected to the same influences, and nothing has occurred to disturb its uniformity, so that it can for some considerable time carry a large amount of aqueous vapour without precipitation. When precipitation does occur, it is usually by the thrusting upwards of the warmer strata into cold upper strata, and then condensation proceeds without check and rapidly from invisible particles to rain-drops. Thus, on reaching the first mountainous region, or in passing over land heated to a temperature much above that of the sea surface, the ascent of the most humid strata into the cold upper air is often followed by rain. The remarkable transparency before rain signifies a correspondence in direction as well as in qualities between the upper and lower strata. If the wind be between west and south, as it usually is in these cases, we are informed of a similar wind at a high level—that is, that the upper current, as well as the lower, is more than commonly humid, and its vapour tending to condense by passing towards higher latitudes. It only requires slight disturbances in a vertical direction to precipitate the abundant vapour, and hence the frequency of showers, especially where large columns of heated air rise from the land, at a distance from the south coast, and in hilly country. The south-westerly wind being a warm one, is more likely to ascend and to have its vapour condensed to rain than a colder current. The clear lower air indeed owes its clearness partly to its ascending movement.

(2) Strong winds and showery weather. Strong winds usually prevail when the air up to a great height partakes more or less of the same movement. There is

[1] "On Saturday evening, August 31, a balloon, as it ascended, crossed and recrossed Luton several times."—*Daily News*, September 2, 1889.

[1] Salt is shown to be present everywhere in the atmosphere by the spectrum of a flame.

also no opportunity for the filtering through of small portions of dissimilar air, and, if portions do descend into the lower levels, they are broken up, diffused, and dispersed. Still, in the colder half of the year, if the lower wind blows from between east and north, and does *not* extend to a great height, a strong mist may be produced by its being mixed with detached portions of the westerly upper current, which take a long time to be thoroughly incorporated and dissolved, and contain more vapour than they can hold invisible in contact with the cold surface-breeze. Thus the prevalence of much haze with a north-easterly gale indicates an equatorial upper current, and the polar wind is apt to be replaced by it before long. With regard to showery weather, it may almost be said to be the opposite of hazy weather, and for the following reasons:—First, as we have seen above, showers are produced by the upward projection of lower air, containing a good deal of vapour, into upper cold air of the same kind. Then, they are often the expression of a state of the atmosphere when the interchange between the upper and lower strata proceeds by large ascending columns and large down-rushes, instead of by small convection currents, and ascending and descending filaments over a very large area. The clearness of the air with a showery north-east wind is quite surprising, for it is sufficient to banish to a great extent even London smoke. Here, again, the north-east wind prevails to a great height, and the air is homogeneous and rather dry. When a shower or even a cumulus cloud passes over a large town, the smoke is seen to be drawn up in a moving column to the height of the cloud. Probably the chief cause of the clearness of a showery north-east wind is the prevalence, as in other cases, of the same wind in the upper regions, so that there is no admixture of strange threads in its composition, no strands of extra-humid particles to be rendered visible by incipient condensation.

(3) Winds between south-west and north. These are, on the whole, clear for a similar reason, for it has been shown that the upper currents in Great Britain usually move from between south-west and north-west. If, as occasionally happens, an east wind blows overhead, they are very far from transparent.

(4) Fine settled summer weather, with westerly or southerly winds, is clear not only for the reason above stated, but on account of the general moderate dryness of the atmosphere. In such weather, barometric pressure is frequently highest over Spain or France, and our upper currents are accordingly from north-west, becoming warmer as they advance southwards and increasing in capacity for moisture. There would be no condensation if portions of these currents were to descend into the lower air.

(5) Settled easterly or northerly winds, with either clear sky or high clouds moving from those directions. Haze does not form where the wind is steady, the air dry and homogeneous up to a great height, and equilibrium stable, for there is nothing to lead to condensation except at the particular level of saturation where clouds are manifested.

(6) Easterly or northerly winds with a high continuous cloud canopy moving in the same direction, small range of temperature, and steady conditions; or, with detached cumulus in the daytime, and clear nights. The same remarks apply here as to the last.

(7) North-west wind, reaching that point from west or south, is particularly clear. Great transparency in this case is not a sign of rain, but rather of fair weather. It is probably due to its agreement in general direction with upper currents, the increasing dryness as it reaches warmer latitudes, and to the uniformity and equilibrium attained by passing over the ocean.

F. A. R. RUSSELL.

THE PULSION MECHANICAL TELEPHONE.

(FROM A CORRESPONDENT.)

A NEW mechanical telephone of extraordinary power has recently been exciting considerable attention in London and some other cities and towns in this country. It is of American origin, like so many other modern improvements of exceptional character, being the invention of one Lemuel Mellett, I believe of Boston, U.S. There have been many previous mechanical telephones, as your readers are aware, some of which have obtained much publicity for a short time, and then have been heard of but little more; but having had opportunities of experimenting frequently with the new instrument, and observing its vocal power, so to speak, under very various circumstances, I cannot doubt that it has a great future before it.

It may be clearly stated at once that the pulsion instrument is absolutely independent of all electrical aids or appliances, and therefore needs neither battery power to bring it into play, nor insulation of any of its parts to keep them effective. It consists solely of two cheap and simple instruments connected by an ordinary non-insulated wire of copper, or, better still, of a double steel wire, the two parts being slightly intertwisted, say with about a single turn in a couple of feet. The wire (or wires) is simply looped to the instrument at either end, the connection being made in a few seconds. The instrument consists of a disk in combination with a series of small spiral springs inclosed in a case of some three or four inches in diameter. These springs, arranged in a manner that has been determined by experiment, and so as to produce harmonized vibrations, appear to possess the power of magnifying or accumulating upon the wire the vibrations which the voice sets up in the disk, and the wire seems to possess—undoubtedly does possess—the power of transmitting to great distances, and giving out upon a second pulsion instrument, the sounds of the voice.

The ability of this simple system of springs, disks, and wires to convey conversational and other sounds to considerable distances with great clearness and distinctness, reproducing the very tones of the voice and the qualities of musical sounds with but little reduction or modification, is most surprising, and to none more so than to the many men of science who have been recently experimenting with it.

The writer of this notice cannot, perhaps, do better than state his own experiences with this system. After examining and experimenting over several short lengths of wire, some of them exceeding a mile and a half, he last week went to the Finchley Road Station of the Midland Railway, from a point near to which a line had been conveyed to near the Welsh Harp Station, a distance of three miles by the line of railway, and of more by the track of the wire, which for the larger part was carried by the telegraph-posts, to which it was attached by very simple means. Conversation through this length of line, of over three miles, was exceedingly easy; indeed, so powerfully was the voice transmitted, that an ordinary hat sufficed for all the purposes of the second instrument, without going near to which conversation was carried on repeatedly by means of the hats of three gentlemen who were present, the tops of which were merely placed against the telephone wire.

I then went into the garden of the "Welsh Harp," where a short length of wire had been led between two points, the wire on its way from one point to the other being twice tightly twisted, at an interval of some yards, round small branches of trees, of about 1 inch in diameter, being wound round and round the branch three times in each case. Strange to say, this tight twisting of the wires round the branches in no way interfered with the transmission of the voice from end to end of the wire.

A third and last experiment was made with a wire laid obliquely across the Welsh Harp lake, and allowed to sink to, and rest upon, the lake bottom. The length of the line was roughly estimated at about one-third of a mile, and from end to end (excepting a few yards at each end where the wire was led from the water's edge to the telephone box) the wire was completely immersed, and without any other support than the bottom of the lake offered it. Yet, notwithstanding this immersion of the whole wire, conversation was carried on through it by means of the pulsion instruments without the least difficulty. In fact, the voice came through the immersed wire, and the longest wire (of over three miles) previously mentioned, with greater purity and mellowness than through shorter lengths.

I must leave to others to explain, and if necessary to discover, the scientific grounds of the success of this extraordinary little instrument. Looking, however, at its practical capabilities as exemplified above, it is not surprising that Post Office, police, railway, and other commercial people, are already overwhelming with applications those who are arranging to supply the new telephone, which from its extreme simplicity is manifestly a cheap one.

NOTES.

No fewer than 1810 patients bitten by dogs were treated at the Pasteur Institute in the year ending October 31. There were thirteen deaths.

THE *Daily Graphic*, the first number of which will appear on January 4, will be interesting from a scientific as well as from a popular point of view. Twenty years ago, when the *Graphic* was started, so bold an enterprise would have been impossible. At that time the pictures in illustrated journals were produced only by the old method of wood-engraving, which could not, of course, supply all the needs of a daily illustrated paper. By means of various scientific processes, drawings can now be so rapidly and effectively reproduced, that the issue even of a daily illustrated journal may be safely undertaken. The new paper is likely to afford a very striking instance of the influence of these processes on art and journalism.

THE Government of New South Wales has adopted an entirely new scheme of technical education. The present Board of Technical Education is to be abolished, and technical schools will be placed under the direct control of the Education Department. A sum of £50,000 is to be expended in the erection and equipment of a new Technical College and Museum in Sydney, while branch technical schools will be established throughout the country districts. It is estimated that £50,000 will be required annually to carry out the new arrangements.

MR. E. W. COLLIN has been deputed by the Government of Bengal to make inquiries as to the present condition of technical education in Bengal, and to find out what steps should be taken by the Government towards its advancement in that Presidency. The Civil Engineering College at Seebpore, an institution for the training of overseers and civil engineers, is supported by the Bengal Government, but it does not appear that there are any means at present in Bengal for the technical training of artisans. Mr. Collin has addressed a circular to various public bodies asking for information, and he will submit a report on the question about the end of the year.

MR. G. BERTIN is to deliver, at the British Museum, a series of four lectures on the religion of Babylonia. The first lecture will be given on November 26, and the others on the three following Tuesdays, at 2.30 p.m.

MR. G. B. SCOTT, of the Indian Survey Department, who has lately been employed on a survey of the Wards Estates in Bengal, has been placed in charge of the new Cadastral Survey of Upper Burmah.

THE next *conversazione* of the Royal Microscopical Society will be held on Wednesday, the 27th instant, at 8 o'clock.

MR. THOMAS CHILD, who has just returned from Pekin, has sent us very beautiful photographs of the two interesting old astronomical instruments at the Pekin Observatory. These instruments are the most ancient of the kind in the world, having been made by order of the Emperor Kublai Khan in the year 1279. They are exquisite pieces of bronze work, and are in splendid condition, although they have been exposed to the weather for more than 600 years. They were formerly up on the terrace, but were removed down to their present position to make way for the eight instruments that were made by the Jesuit Father Verbiest in 1670, during the reign of the Emperor K'ang Hsi, of the present dynasty.

THE metric system of weights and measures having been adopted in the Photographic Office of the Indian Survey, a series of tables for the conversion of these measures to British, and *vice versâ*, has been prepared by Colonels Thuillier and Waterhouse, Surveyor-General and Assistant-Surveyor-General of India. The scope of the tables, however, has been extended so as to meet, as far as possible, the ordinary requirements of general and scientific reference. The multiples and fractions of the British and metric units have each their equivalent expressed in the other, so that the number requiring to be converted may be multiplied directly by the decimal fraction representing the equivalent value of one unit of the required denomination. The relative equivalents are given for the conversion of measures of length, weight, and capacity, cubic and square measures, and also of British-Indian and metric weights. There are also a few miscellaneous tables that may be found generally useful.

IT is well known that whales can remain a long time under water, but exact data as to the time have been rather lacking. In his northern travels, Dr. Kückenthal, of Jena, recently observed that a harpooned white whale continued under water 45 minutes.

THE elephant skeleton set up in the front hall of the Madras Museum is 10 feet 6 inches high, and it has been stated that this is the skeleton of the largest elephant ever killed in India. Mr. Edgar Thurston, Superintendent of the Museum, in his latest Report, says that this is a mistake. Mr. Sanderson gave 10 feet 7½ inches as the largest elephant he had met, and there is a still larger one in the Indian Museum, Calcutta.

SOME fragments of a gigantic elephant's tusk (we learn from the *Rivista Sci. Ind.*) were lately obtained by Signor Terrenzi, the tusk having been found in the yellow Pliocene (marine) sands of Camartina, Narni. It must have been about 10 feet long. One piece (which seems to have been near the base) measured about 2 feet round at the thickest. The tusk had been broken up by the peasants, and distributed as an infallible remedy for tooth-ache and for belly pains in cattle! It probably belonged either to *E. meridionalis*, Nesti, or to *E. antiquus*, Falc. The finding of elephant remains in the Pliocene marine sands of Italy is not new, but it is rare.

A REMARKABLE paper on "The Ethnologic Affinity of the Ancient Etruscans," by Dr. Daniel G. Brinton, was read before the American Philosophical Society on October 18, and has now been issued separately. Dr. Brinton's attention was specially called to the subject during a sojourn of some months in Italy, early in the present year, when he had an opportunity of studying many museums of Etruscan antiquities. The object of the

paper is to prove that the Etruscans probably came from Northern Africa, and belonged to the same stock as the Kabyles, on the borders of whose country Dr. Brinton had spent some time before his visit to Italy. He thus sums up his conclusions:—(1) The uniform testimony of the ancient writers and of their own traditions asserts that the Etruscans came across the sea from the south, and established their first settlement on Italian soil near Tarquinii; this historic testimony is corroborated by the preponderance of archæologic evidence as yet brought forward. (2) Physically, the Etruscans were a people of lofty stature, of the blonde type, with dolichocephalic heads. In these traits they corresponded precisely with the blonde type of the ancient Libyans, represented by the modern Berbers and the Guanches, the only blonde people to the south. (3) In the position assigned to woman, and in the system of federal government, the Etruscans were totally different from the Greeks, Orientals, and Turanians; but were in entire accord with the Libyans. (4) The phonetics, grammatical plan, vocabulary, numerals, and proper names of the Etruscan tongue present many and close analogies with the Libyan dialects, ancient and modern. (5) Linguistic science, therefore, concurs with tradition, archæology, sociologic traits, and anthropologic evidence, in assigning a genetic relationship of the Etruscans to the Libyan family.

A LAKE-DWELLING has been discovered in the neighbourhood of Somma Lombardo, north-west of Milan, through the draining of the large turf moor of La Lagozza. The Berlin Correspondent of the *Standard*, who gives an account of the discovery, says that this "relic of civilization" was found under the peat-bog and the underlying layer of mud, the former being 1 metre in thickness, and the latter 35 centimetres. The building was rectangular, 80 metres long and 30 metres broad; and between the posts, which are still standing upright, lay beams and half-burnt planks, the latter having been made by splitting the trees, and without using a saw. Some trunks still retain the stumps of their lateral projecting branches, and they have probably served the purpose of ladders. The lower end of these posts, which have been driven into the clay soil, is more or less pointed, and it can be seen from the partly still well-preserved bark that the beams and planks are of white birch, pine, fir, and larch. Among other things were found polished stone hatchets, a few arrow-heads, flint knives, and unworked stones with traces of the action of fire.

MR. R. ETHERIDGE, JUN., contributes to the Report of the Australian Museum, just received, an interesting appendix on the limestone caves at Cave Flat, junction of the Murrumbidgee and Goodradigbee rivers, county of Harden. Having recorded the observations made by him in these remarkable caves, Mr. Etheridge offers some remarks on the Murrumbidgee limestone. This, he says, is of a dense blue-black colour. It is much jointed and fissured, highly brittle in places, with a hackly conchoidal fracture, and crammed with fossils, especially corals. As a display of these beautiful organisms in natural section, he has never seen its equal. Large faces of limestone may be seen, with the weathered corals, and particularly Stromatopora, standing out in relief and in section also. Many of these masses of coral, particularly those of Stromatopora and Favosites, are as much as 4 feet in diameter. The Murrumbidgee limestone has been classed as Devonian by the late Prof. de Koninck, but Mr. Etheridge has not yet sufficiently examined the fossils of this deposit either to gainsay or confirm this view. He thinks it not improbable, however, that Prof. de Koninck's view may be correct.

THE *Comptes rendus* of the Paris Academy of Sciences, of November 4, contains a note by M. A. Angot, on the mean hourly velocity of the wind at the summit of the Eiffel Tower, measured during 101 days, ending with October 1, by means of an anemometer placed at 994 feet above the ground, and compared with the results of a similar instrument at the Paris Meteorological Office, placed at 66 feet above the ground. The average velocity on the tower was 16 miles an hour, being over three times the amount registered at the Meteorological Office, where it was only 5 miles an hour. At the lower station the diurnal variation showed a single minimum about sunrise, and a single maximum about 1 h. p.m. On the tower the minimum occurred about 10h. a.m., and the maximum about 11h. p.m., while the characteristic maximum of lower regions about the middle of the day was hardly perceptible on the tower. It is remarkable that this inversion, which is usual upon high mountains, should occur at so small a height as that of the Eiffel Tower. The ratio of increased velocity was constant at about 5 : 1 between midnight and 5h. a.m.; it then decreased rapidly and became 2 : 1 at about 10h. a.m., and maintained this value until 2h. or 3h. p.m., when it again rose regularly until midnight. These results are of considerable importance to the study of aërial navigation.

THE new number of the *Mineralogical Magazine* opens with an important paper, by Mr. L. Fletcher, F.R.S., on the meteorites which have been found in the desert of Atacama and its neighbourhood. This paper is accompanied by a map of the district. Prof. McKenny Hughes, F.R.S., has a paper on the manner of occurrence of Beekite and its bearing upon the origin of siliceous beds of Palæolithic age. There are also three short papers by Dr. M. F. Heddle, and one by Mr. R. H. Solly.

SOME experiments on the photography of the red end of the spectrum, by Colonel J. Waterhouse, appear in the Proceedings of the Asiatic Society of Bengal for April 1889. In order to render the ordinary commercial gelatine dry plates sensitive to the red rays they are bathed for one or two minutes in a solution of 1 part of alizarin blue ($C_{17}.H_9.NO_4$) to 10,000 parts of distilled water with 1 per cent. of strong ammonia added. Plates treated with this dye show very intense action through the violet and blue regions as far as *b*; from E to C there appears to be a minimum of action; the sensitiveness, however, increases between C and A, and is strongest between C and B and a to A. Below A the sensitiveness quickly diminishes. Colonel Waterhouse finds that plates saturated with a special preparation of cyanin and sulphate of quinine have their maximum sensitiveness between D and B, but between B and A the action is much weaker than that obtained by using alizarin blue, hence the latter dye is valuable as a ready and simple means of photographing the spectrum between C and A with ordinary dry plates. For orthochromatic photography, rhodomine was found to be almost as efficient as erythrosin, and to be especially useful for photographing the region immediately about D. The photographs were taken by means of Rowland's plane and concave diffraction gratings.

A NEW mode of preparing manganese, by which the metal can be obtained in a few minutes in tolerably large quantities and almost perfectly pure, is described by Dr. Glatzel, of Breslau, in the current number of the *Berichte*. A quantity of manganous chloride is first dehydrated by ignition in a porcelain dish, and the pulverized anhydrous salt afterwards intimately mixed with twice its weight of well-dried potassium chloride. The mixture is then closely packed into a Hessian crucible and fused in a furnace at the lowest possible temperature, not sufficient to volatilize either of the chlorides. A quantity of metallic magnesium is then introduced in small portions at a time, the total quantity necessary being about a sixth of the weight of the manganous chloride employed. Provided the crucible has not been heated too much above the melting-point of the mixture of chlorides, the action is regular, the magnesium dissolving with

merely a slight hissing. If, however, the mixture has been heated till vapours have begun to make their appearance, the reaction is extremely violent. It is therefore best to allow the contents of the crucible, after fusion, to cool down to a low red heat, when the introduction of the magnesium is perfectly safe. When all action has ceased, the contents of the crucible are again heated strongly, and afterwards allowed to cool until the furnace has become quite cold. On breaking the crucible, all the potassium chloride and the excess of manganous chloride is found to have been volatilized, leaving a regulus of metallic manganese, fused together into a solid block, about three parts by weight being obtained for every two parts of magnesium added. The metal, as thus obtained, is readily broken up by hammering into fragments of a whitish-gray colour possessing a bright metallic lustre. The lustre may be preserved for months in stoppered glass vessels, but, when exposed to air, the fresh surface becomes rapidly brown. The metal is so hard that the best files are incapable of making any impression upon it. It is so feebly magnetic that a powerful horse-shoe magnet capable of readily lifting a kilogram of iron has no appreciable effect upon the smallest fragment. It was noticed that the introduction of a small quantity of silica rendered the manganese still more brittle, and caused it to present a conchoidal fracture, that of pure manganese being uneven. The specific gravity of the metal, former determinations of which have been very varied, was found to be 7·3921 at 22° C. This number, which was obtained with a very pure preparation, is about the mean of the previous determinations. Dilute mineral acids readily dissolve the pulverized metal, leaving a mere trace of insoluble impurity. It is also satisfactory that practically no magnesium is retained alloyed with the manganese, and the introduction of carbon is altogether avoided by the use of this convenient method.

THE additions to the Zoological Society's Gardens during the past week include a Common Marmoset (*Hapale jacchus*) from South-East Brazil, presented by Mr. O. Burrell; a Common Squirrel (*Sciurus vulgaris*), British, presented by Miss B. Tatham; a Common Stoat (*Mustela erminea*) from Northamptonshire, presented by Mr. Cuthbert Johnson; a Wattled Crane (*Grus carunculata*) from West Africa, presented by Mr. Robert Sinclair, Jun.; a Redshank (*Totanus calidris*) from Devonshire, presented by Mr. R. M. J. Teil; a White-backed Piping Crow (*Gymnorhina leuconota*) from Australia, presented by Mr. W. H. Felstead; a Grey-headed Porphyrio (*Porphyrio poliocephalus*) from India, presented by Dr. Gerard Smith; a Common Chameleon (*Chamæleon vulgaris*) from North Africa, presented by Mr. G. W. Alder; a Dwarf Chameleon (*Chamæleon pumilus*) from South Africa, presented by Mrs. Leith; a Green Lizard (*Lacerta viridis*), European, presented by Mr. C. H. Whitlow; a Common Jay (*Garrulus glandarius*), European, purchased; five Carpet Snakes (*Morelia variegata*) from Australia, received in exchange.

OUR ASTRONOMICAL COLUMN.

OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at Greenwich at 10 p.m., November 21 = 2h. 3m. 21s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G. C. 527	—	—	2 15 28	+41 35
(2) 15 Arietis	6	Yellowish-red.	2 4 31	+18 59
(3) α Arietis...	2	Yellow.	2 1 0	+22 57
(4) β Trianguli	3	Bluish-white.	2 3 0	+34 28
(5) DM + 56° 724 ...	9	Reddish-yellow.	2 42 32	+56 31
(6) R Tauri	Var.	Very red.	4 22 16	+ 9 55

Remarks.

(1) Sir John Herschel's description of this nebula is as follows:—! Bright, very large, very much extended. The spectrum has not yet been recorded.

(2) This is a star of Group II., in which Dunér records bands 2–8, but states that they are neither wide nor dark. The star falls in species 13 of the subdivision of this group, and is well advanced towards Group III. Metallic lines, and possibly hydrogen lines (dark) may therefore be expected. In the earlier stages of the group, no hydrogen lines appear, the radiation from the interspaces between the meteorites being balanced by the absorption of the gas surrounding the incandescent stones; but in the more advanced members, as in α Orionis, the absorption will probably be found to slightly predominate. The presence or absence of the F line, and of metallic lines, and their relative intensities, should therefore be noted.

(3) This is a star of either Group III. or Group V., and the usual criteria (see p. 20) should be observed in order to determine which. At the same time, the relative intensities of the hydrogen lines and the metallic lines (say *b* and D) should be recorded, so that the star may be placed in a line of temperature with others.

(4) According to Gothard this is a star of Group IV. The usual observations are required.

(5) Dunér classes this with Group VI. stars, but states that the type of spectrum is a little doubtful. Further observations are therefore required. As the most advanced stars of the group are very red, the colour of this star indicates that it probably belongs to an early stage of the group, in which the carbon bands would be narrow, and therefore somewhat difficult to observe with certainty; in that case traces of *b* and D might be expected. The colour should also be checked.

(6) Gore gives the period of this variable as 325·6 days, and the range as 7·4–9·0 at maximum to < 13 at minimum. The maximum will occur on November 30. The spectrum is of the Group II. type, and belongs to species 9. Dunér states that the dark bands, especially 7 and 8, are very wide. In several variables of this class (R Leonis, R Andromedæ, &c.), Espin has observed bright hydrogen lines near maximum, and the question is, Is this common to all the variable stars of this type? As stated with reference to 15 Arietis, under normal conditions the hydrogen lines in the earlier species of the group are absent, because the interspacial radiation balances the absorption; but if through some cause the temperature increases at maximum, more hydrogen would be driven into the interspaces and radiation would predominate. It may be mentioned that, according to the meteoritic theory, the increase of temperature and luminosity is brought about by the periastrion passage of a secondary swarm through the outliers of the central one. It is not unlikely that slight variations of colour will take place from maximum to minimum, and it is important therefore that the colour should be noted when the spectroscopic observations are made.

A. FOWLER.

THE MINIMUM SUN-SPOT PERIOD.—M. Bruguière, in *L'Astronomie*, November 1889, gives a series of observations made with a view to determine the exact date of the minimum sun-spot period. The following tables show the condition of the sun's surface with respect to spots from the beginning of January to the end of July of this year:—

Date, 1889.	No. of days without spots.	
Jan. 3–15	13	
,, 18–31	14	
Feb. 8–21	14	
Mar. 2–6	5	
,, 17–31	15	
April 11–30	20	25
May 1–5	5	
,, 10–26	17	
,, 28–31	3	18
June 1–15	15	
,, 29–30	2	13
July 1–11	11	
,, 25–27	3	

Date, 1889.	No. of days with spots.
Jan. 16–17	2
Feb. 1–7	7
,, 22–29	8
Mar. 1 and 8–16 ...	10
April 1–10	10
May 6–9	4
,, 27	1
June 16–28	13*
July 12–24	13*
,, 28–31	4

* The same spot.

If the small spots that were seen from May 6–9, and also on May 27, be neglected, it will be seen that there would be a period without spots extending from April 11 to June 15—that

is, sixty-six days; but if these small spots be considered we find an interval of twenty-five days without spots—namely, from April 11 to May 5. The minimum period, therefore, appears to have passed about the end of April, this being the time when the greatest number of days passed without spots being observed on the sun. The new period opened with the appearance of a large spot on June 16.

RETURN OF BRORSEN'S COMET.—The following elements and ephemeris for this comet are given by Dr. E. Lamp in *Astronomische Nachrichten*, No. 2933:—

T = 1890 February 24·1358 Berlin midnight.

$$\left.\begin{aligned} \omega &= 14^\circ\ 55'\ 35''\cdot89 \\ \Omega &= 101\ 27\ 33\cdot74 \\ \iota &= 29\ 23\ 48\cdot25 \end{aligned}\right\} \text{Mean Eq. } 1890\cdot0$$

$$\phi = 54\ 7\ 46\cdot19$$

$$\mu = 650''\cdot3693$$

Ephemeris for Berlin Midnight.

1889.	R.A. h. m. s.	Decl. ° ′	1889.	R.A. h. m. s.	Decl. ° ′
Nov. 21	22 9 17	−45 4·6	Dec. 11	22 26 40	−39 42·8
22	9 47	−44 50·3	12	27 55	−39 24 6
23	10 19	−44 35·8	13	29 11	−39 6·1
24	10 54	−44 21·2	14	30 30	−38 47·4
25	11 31	−44 6·4	15	31 50	−38 28·5
26	12 11	−43 51·4	16	33 12	−38 9·3
27	12 53	−43 36·2	17	34 36	−37 49 8
28	13 38	−43 20·8	18	36 2	−37 30·1
29	14 25	−43 5·2	19	37 29	−37 10·1
30	15 14	−42 49·4	20	38 58	−36 49·8
Dec. 1	16 6	−42 33·4	21	40 29	−36 29·2
2	17 0	−42 17·3	22	42 1	−36 8·4
3	17 56	−42 0·9	23	43 35	−35 47·2
4	18 54	−41 44·4	24	45 10	−35 25·8
5	19 54	−41 27·7	25	46 47	−35 4·1
6	20 57	−41 10·8	26	48 26	−34 42·0
7	22 1	−40 53·6	27	50 6	−34 19·6
8	23 8	−40 36·3	28	51 48	−33 56·8
9	24 16	−40 8·7	29	53 32	−33 33·6
10	25 27	−40 0·9	30	55 17	−33 10 0

THE COMPANION OF η PEGASI.—A companion to η Pegasi was discovered by Sir William Herschel in 1780, and subsequently observed by South in 1824. Its magnitude has been rated from twelve to fifteen. Mr. S. W. Burnham, however, notes (*Astronomische Nachrichten*, No. 2933) that, using the 36-inch refractor at the Lick Observatory, the Herschel companion appears as a close double. South's mean of two measures is given in his catalogue as:—

1824·84 338°·9 89″·82 2*n* S.

The following is the mean of four measures made at Mount Hamilton:—

η Pegasi.

B and C.					A and BC.		
1889·53	83°·3	0″·29	10·1	10·1	1889·53	339°·0	90″·38

The close pair is difficult, and can hardly fail to be a physical system, and Mr. Burnham thinks that, although it is not a test for the large telescope, it will not be seen with any small instrument.

GENERAL BIBLIOGRAPHY OF ASTRONOMY.—The second part of Vol. I. of this comprehensive bibliography has been published. It represents Houzeau's last work, and hence it is well that his biographical note, by A. Lancaster, should be included. The first part of Vol. I., published in 1887, contained the references to historical works and those relating to astrology; the part just published contains the references to biographies of astronomers and their epistolary communications, general astronomical works, astronomical societies and their proceedings, and everything relating to spherical astronomy. Works on theoretical astronomy are also well represented. The third and last part of Vol. I. is now in press, and contains references to all the published matter on the mechanism of the heavens, physical, practical, and descriptive astronomy, and the systems of cosmogony. The utility of this bibliography, when completed, needs no comment.

J. C. HOUZEAU'S "VADE MECUM."—With reference to our biographical note on J. C. Houzeau (p. 20), M. A. Lancaster writes to remind us that Houzeau's "Vade Mecum" was issued after the appearance of the second volume of the "Bibliographie Générale de l'Astronomie," the publication of which began in 1879. Moreover, the "Vade Mecum" was only a second edition of the "Répertoire des Constantes de l'Astronomie," inserted in 1877 in the first volume of the new series of the "Annales Astronomiques" of the Brussels Royal Observatory. The numerous materials brought together for the "Bibliographie Générale" suggested to Houzeau the idea of issuing a new edition of the "Répertoire" considerably corrected and enlarged.

A NEW COMET.—A new comet was discovered on November 17 by Mr. Lewis Swift, of the Warner Observatory, Rochester, New York. Place at November 17, 6h. 35m. 2s. G.M.T.; R.A. = 22h. 42m. 24s.; N.P.D. = 78° 9′. Daily motion in R.A., + 2m.; in N.P.D., − 15′. The comet was only faint.

MIRAGE IN THE SOUTH AMERICAN PAMPAS.

WAS staying in the Pampas of the Argentine Republic, near Melincue, a small town of the Province of Santa Fe, from September 1888 to March 1889. During my stay I had the opportunity of observing certain mirage phenomena. It is possible that my notes may contain something of interest. They were, designedly, taken without reference to any previous knowledge of the theory of mirage that I might possess.

To illustrate my observations I had drawn *eight* diagrams; but, for the purpose of insertion in NATURE, I have been obliged to reduce these to *two*. Hence I fear that my descriptions may not be as clear as I should wish.

The most general conclusion at which I arrived was that there were *two* classes of mirage of very different character. The one I shall call "the summer mirage," the other "the winter mirage." I would observe that, without a telescope of some sort, one would be unable to make observations of much value; and that, as I had but a binocular telescope, in many details I failed to make out as much as I could had I possessed a larger telescope steadily mounted.

I. *The Summer Mirage.*

(1) This mirage is seen in full day. I was told that in normal years it is most remarkable in the extreme heat of summer. The summer of December, January, and February 1888 and 1889 was abnormally wet, however. And I myself saw the mirage most frequently in spring (September, October, and the earlier part of November), the grass being then short and very dry. Later on the grass became very long, and unusually green and damp, owing to the heavy rains. And then I saw the mirage but rarely in the grass plains, though on the several occasions on which I passed, in the blaze of a summer day, the dry sandy bed of an old laguna, the mirage was there to be seen very clearly.

On one or two occasions in spring I saw the mirage when there was a fairly cold wind and no perceptible sunshine, but still in full day.

(2) This kind of mirage usually appeared as a strip of "water" running more or less parallel to the horizon, at one end narrowing to a point, and at the other end opening out into the sky. It appeared much as an arm of the sea, or an estuary, seen near the horizon, and running parallel to it. The "water" was of the same colour as the sky above it near the horizon.

(3) Viewed through glasses, the whole of the land seen above and beyond the "water," the "water" itself, and to a less extent the land seen just this side of it, appeared wavy and ill-defined, flocculent, and (when there was any breeze) possessed of a drifting movement down the wind. At the thin end of the "water," and just beyond it in the line of the layer, one could see broken fragments of "water" drifting over the land; and, in like manner, the peninsula of land appeared to end in a line of drifting fragments.

(4) It appeared to me that the land seen beyond the watery layer was either within the limits of the natural horizon, or not much beyond them. One did not, as one did in the "winter mirage," see houses, &c., that were normally out of sight.

(5) Cattle, &c., seen in the watery layer were ill-defined. But on the whole it seemed that their legs were hidden, and bodies were reflected inverted, much as if they had been standing in shallow water.

(6) When I mounted higher, a mirage, if seen at all, was further off than when I stood lower.

If, when looking at the watery layer of a mirage, I mounted higher, the "water" narrowed, and the strip of land beyond it widened, until at a certain height of my head the "water" had narrowed into a wavy line of fragments. Further mounting caused the "water" to disappear. If, on the contrary, I stooped, the "water" appeared to widen, the strip of land above it to narrow, until at last the mirage joined the sky.

On one occasion, when the mirage was about a mile and a half distant, and on another occasion when about 250 yards distant, I caused the "water" to appear and disappear by a vertical movement of my head not exceeding 1 foot.

(7) Objects situated in the watery layer but rising out of it, or on the strip of land beyond it, were reflected in the "water" much as in true water; but all was ill-defined, and the inverted reflections often broken and lengthened.

(8) It appeared to me that objects on the strip of land beyond the watery layer were also to be seen faintly reflected in the land that lay between them and the "water." And when, as in (6), I had raised my head until the "water" had just dwindled away, objects near the horizon were reflected inverted in the region from which "water" had vanished.

(9) By the aid of my glasses I came to the conclusion that objects were not really, as they appeared to the naked eye, "drawn up" by the mirage. But it seemed rather that, an object being seen above its (often elongated) reflected image, and both being ill-defined, to the naked eye the whole appeared like the object "drawn up." In this way clumps of grass appeared as trees.

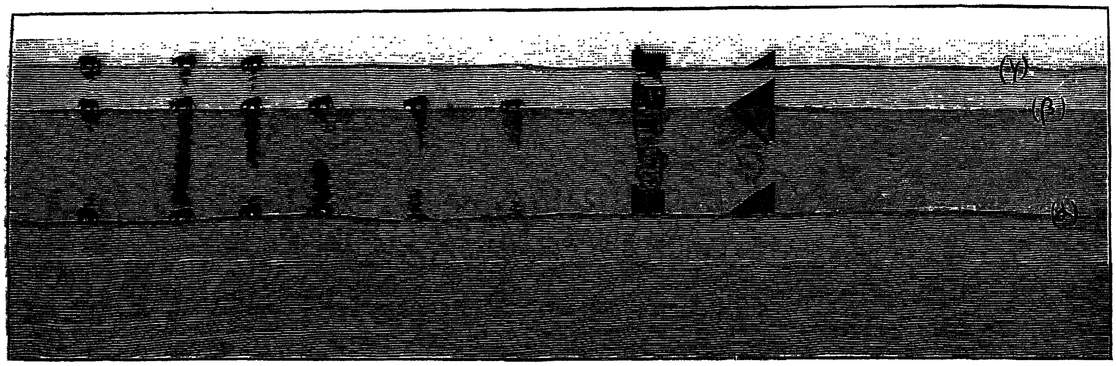


FIG. 1.

(10) In (1) I have mentioned the usual form of the mirage. But with various slopes, &c., of the ground, the form of the mirage varied. Sometimes the "water" opened out into the sky both ways; and several times I saw an isolated patch of "water" over an isolated patch of bare hot earth.

Conclusions as to Summer Mirage.—It seemed, then—

(1) That this mirage was due to a layer of relatively warm air close to the earth.

(2) That this mirage-giving layer was not more than about 5 feet in depth, and that it may have been less.

(3) That there were not, to any noticeable extent, vertical elongations of objects nor extensions of normal horizon.

(4) That in this mirage there were no images, erect or inverted, seen above the real object.

In fact, it seemed that the sky and terrestrial objects were simply reflected in a sheet of warmer air lying close to the ground. (Of course the paths of the rays would be curved.)

II. *The Winter Mirage.*

[I was told that this mirage is seen in winter, and best on fine mornings after hard frost. What I saw were, it seemed, but poor specimens.]

(1) I saw this mirage several times, always about sunrise and after a frost. Before sunrise, as soon as there was any light, I looked out into the plains with my binoculars. It appeared as if the horizon were higher than usual, and that one could see tracts of land, with houses and other objects, that were usually concealed below the horizon.

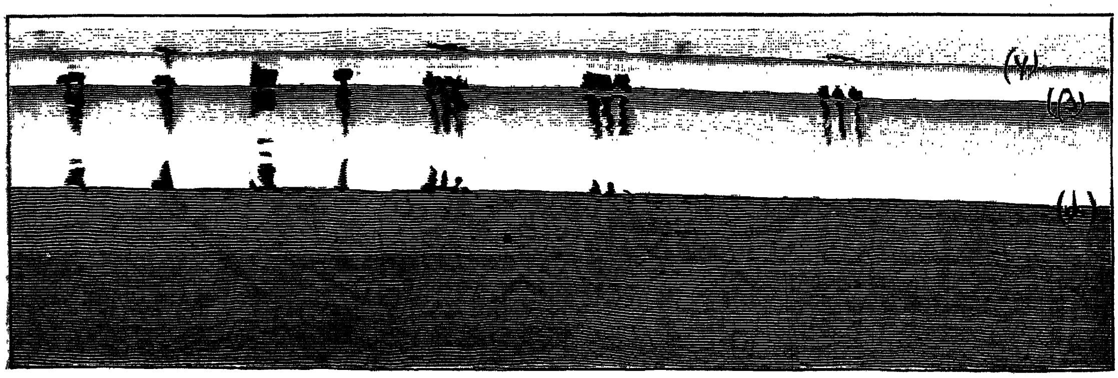


FIG. 2.

Further, it seemed that this extension of horizon was not really continuous, as it at first appeared, but that it was divided into layers. As far as I could judge, the line (α) was beyond the normal limits of the horizon, the tract from (α) to the limit (β) was more or less a repetition of the tract below (α), and from (β) to (γ) was again more or less a repetition of the same tract. As to what one could see above the line (γ), I could make no trustworthy observations.

Before sunrise, this extension of the horizon was seen all round; and, though the layers referred to could be distinguished fairly well, there were as yet no "watery layers" to be seen.

The land seen just above the lines (α) and (β) was paler than that seen just below these lines.

(2) Thanks to a most convenient distribution of cattle of various colours, and of other objects, I was able, with the aid of my glasses, to make out a good deal.

But the images changed as the cows moved, the appearances varied as time went on, and were so different in different parts of the horizon, that I could only arrive at some general conclusions.

There would be, for example, just below, or on the edge of, the line (α), a cow. This I will call the "*first cow*," or the

"original cow." Just below or on the line (β), vertically above the *first cow*, and, like it, erect, would be a *second cow*, a repetition of the first. And often, above this again, below or on the line (γ), would be a third cow, also erect.

Sometimes there were confused images hanging from the *second cow* and joining other confused images piled on the *first cow*; sometimes the first cow was clear of images, while they hung down from the second cow; sometimes the second cow was clear, and there were images piled on the first. Often the *third cow* was missing (see Fig. 1). As the original cow moved, these images changed their disposition or vanished, and the third cow appeared or vanished. But in all these changes it seemed to me that the *first cow*, *second cow*, and (when visible) the *third cow*, were the permanent images. These, it appeared, were always erect.

(3) After the sun had risen, all continued *in statu quo* for a short time. But soon, at various parts of the horizon, the land just above the edges (α), (β), and (γ) paled away, and finally melted into the appearance of "sky" or "water." There were left, in the later stages of the mirage, first, the plain itself, with an extension, the limits of which were not sharp, beyond the normal horizon; secondly, above this a strip of land, apparently suspended in the air; thirdly, in some parts of the horizon another strip of land suspended in the air above this again. The interval between (α) and (β) was in all stages greater than that between (β) and (γ). One of the appearances in the later stages is indicated in Fig. 2.

Other changes crept in, too. Very often the original objects were wholly or partly sunk out of sight; the images were less defined; and the confused images hanging from the *second cow*, *e.g.*, or piled on the *first cow*, were now seen in the watery layers, sometimes bridging it over.

(4) As time went on, the watery layers widened. The images, too, became still vaguer, and the original objects were usually out of sight or only just indicated above the line (α). Moreover, the aërial images, with their confused trails of images hanging from them, began to assume more the appearance of "inverted images suspended over objects hidden below the horizon."

(5) In these later stages, no doubt, anyone would have guessed that the aërial images were indeed very vaguely defined *inverted* images. But to me, as I followed the phenomenon from the beginning, it seemed that they were not so. It seemed to me that each aërial image was really topped by an *erect* image, which, with the trails hanging from it, seemed like an inverted image. At least I can say that, so long as the images were well defined at all, I never made out a clear case of the *main*, or permanent, aërial images being inverted. Thus, as the *first cow* moved, it was the erect *second* (and sometimes *third*) *cows* that remained clear.

(6) In these later stages it was only trees and houses that could be seen in the mirage, and these were ill-defined.

(7) The mirage lasted until about an hour and a quarter after sunrise. The last traces of aërial images of land appeared just under the sun, and in that part of the horizon that lay just opposite to it. Whether the abnormal extension of the horizon entirely ceased at the same time, I cannot say; but there did not remain any noticeable extension.

(8) As with the summer mirage, I found I could alter appearances by altering my level above the earth. But the change in level had to be more considerable. I have no good notes on this matter; but I believe that usually I could recover a past stage of the mirage by a sufficient descent down a ladder from my post of observation.

General Conclusions as to Winter Mirage:—

(1) It is due to the earth, and the air near it, being considerably chilled below the temperature of the rest of the atmosphere.

(2) The phenomena of extended horizon and multiple images are to be observed.

(3) The "drawn up" appearance of objects is really due to a number of images piled upon one another, only to be separated by the use of a telescope.

(4) No case of a terrestrial object having above it a single inverted image, or images of which the uppermost was inverted, came under my notice. W. LARDEN.

SCIENTIFIC SERIALS.

American Journal of Mathematics, vol. xii. No. 1, and index to vols. i.-x. (Baltimore, 1889).—This volume opens with an instalment of sixty pages of a memoir by A. R. Forsyth, F.R.S., on "Systems of Ternariants that are Algebraically Complete." In this the writer has found it convenient to use "'ternariant' as a generic term for concomitants of ternary quantics, instead of giving it the signification which Prof. Sylvester proposed (*Amer. J. of Math.*, vol. v. p. 81) to give to it, viz. the leading coefficients of those concomitants." The memoir is divided into three parts, and deals with the theory of the algebraically independent concomitants of ternary quantics, taking as the starting-point the six linear partial differential equations of the first order satisfied by them. References are supplied to numerous memoirs on the subject.—Captain (now Major) P. A. Macmahon continues (pp. 61-102) his investigations (vol. xi. No. 1) in a "Second Memoir on a New Theory of Symmetric Functions." Herein he is engaged with functions which are not necessarily integral, but require partitions, with positive, zero, and negative parts for their symbolical expression. The author thus summarizes his results: (1) a simple proof of a generalized Vandermonde-Waring power law which presents itself in the guise of an invariantive property of a transcendental transformation; (2) the law of "groups of separations"; (3) the fundamental law of algebraic reciprocity; (4) the fundamental law of algebraic expressibility which asserts that certain indicated symmetric functions can be exhibited as linear functions of the separations of any given partition; (5) the existence is established of a pair of symmetrical tables in association with every partition into positive, zero, and negative parts, of every number, positive, zero, or negative.—The closing portion of the number (pp. 103-114) is taken up with an article entitled "De l'Homographie en Mécanique," by P. Appell.—A likeness of M. Poincaré faces p. 1.—The index is of a twofold description—of authors and of subjects. From the forewords we learn that papers have been published from eighty-nine contributors; these comprise "most of the leading mathematicians of the world."

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, November 11.—M. Hermite in the chair.—Presentation of Report of Proceedings of the permanent International Committee for preparing a photographic chart of the heavens, by M. E. Mouchez. Fifteen Observatories will be ready by the middle of next year; and five others before the end. The zones are indicated.—Note of M. Daubrée with descriptive catalogue of the meteorites of Mexico prepared by M. Antonio del Castillo. Meteorites are abundant in Mexico. A remarkably wide area of dispersion is indicated by three portions of one mass, found at the angles of a triangle, whose two longer sides were 90 km. and 60 km. In one of these places two plates were found 250 m. apart; and they seem to have formed one huge plate over 24,000 kgm. weight, which broke near the ground.—On the incineration of vegetable matters, by M. G. Lechartier. Trying various methods, he finds, that in the carbonization and incineration of a plant, there is considerable loss of sulphur, volatilized in various combinations; and special precautions are necessary in determining this constituent. Under the same conditions, and care being taken to prevent loss of solid matter carried away mechanically with the issuing gas, there is no sensible loss of phosphorus.—M. Picard was elected member in Geometry, in place of the late M. Halphen.—On a rotating magnetic field formed with two Ruhmkorff coils, by M. W. De Fonvielle. A current from accumulators is sent through the primary of one coil, the secondary of which is connected with that of the other coil, which is in a line with the first, and the primary of which may be open or closed.—On certain ellipsoidal areas, by M. G. Humbert.—On a new calculating machine, by M. L. Bollée. While in previous machines, multiplications, *e.g.*, are done by successive additions, this one has a multiplying apparatus which determines immediately, in one function, the product of a number by each figure of the multiplier.—On the solubility of the chlorides of potassium and of sodium in the same solution, by M. Étard. The results of experiment are shown in graphic form; the curves of solubility of each salt separately being compared with those of the mixed salts, &c. The sum of the dissolved salts is represented by a continuous straight line. The curves for the mixed salts cross at temperature 97°; that for NaCl falling while the other rises.—On an application of thermo-chemistry, by M. A.

Colson. The formation of nicotine monohydrochloride liberates about twice as much heat as that of the dihydrochloride under like conditions; hence a probable difference in constitution of the two nitrogen groups of nicotine. The action of nicotine on coloured reagents shows at once a difference in the two basicities.—On the myelocytes of fishes, by M. J. Chatin. In fishes, as in other zoological groups, the nervous elements termed myelocytes, are not to be referred to a special histic type, but to the nerve cell; which is simply modified, chiefly by enlargement of the nucleus, and corresponding reduction of the somatic part.—On the continuity of the pigmented epithelium of the retina with the external segments of the cones and rods, and the morphological value of this arrangement in vertebrates, by MM. R. Dubois and J. Renaut. This new fact makes it probable (according to the authors) that in the retina of vertebrates a similar process occurs to that in the light-sensitive apparatus of Mollusks like *Pholas*; by mechanism of impression and transformation of luminous movement into contractile, then sensorial.—On strabismus, by M. H. Parinaud. The immediate cause of the deviation (in squinting) is a disorder of innervation, excess in convergence, defect in divergence, caused generally by the accommodative effort in one case (hypermetropia), and the little use made of accommodation in the other (myopia). The deviation, when sufficiently fixed and prolonged, induces anatomical changes both in the brain-connections and the tissues of the eye (in the latter case, not only shortening of muscles, but retraction of all relaxed fibrous parts, especially Tenon's capsule). This has important bearings on treatment.—On the morphology and the biology of the fungus *Oidium albicans* (Robin), by MM. G. Linossier and G. Roux. Besides the *yeast* form, and the *globulofilamentous*, he finds a third, similar to *chlamydospores*, and probably needing some new natural habitat for full development. This fact, with the absence of *ascospores*, &c., suggests removal of the organism from the genus *Saccharomyces*. Again, it is found, that in culture of the fungus, the complication of form increases with the molecular weight of the aliment; there is a growing tendency to form long thin filaments. This tendency is also favoured by high temperature, excess of oxygen, a trace of nitrates, and antiseptics.—Comparative activity of various digitalines, by M. Bardet. He compares crystallized and amorphous digitaline, prepared according to the French codex, German *digitoxine*, French *digitaleine*, and German *digitaline* (the power of the two last is much less than those of the others).

DIARY OF SOCIETIES.

LONDON.

THURSDAY, NOVEMBER 21.

ROYAL SOCIETY, at 4.30.—(1) Further Discussion of the Sun-spot Observations at South Kensington; (2) on the Cause of Variability in Condensing Swarms of Meteorites: J. Norman Lockyer, F.R.S.—On the Local Paralysis of Peripheral Ganglia, and on the Connection of Different Classes of Nerve Fibres with them: J. N. Langley, F.R.S., and W. Lee Dickinson.—On the Tubercles on the Roots of Leguminous Plants, with Special Reference to the Pea and the Bean (Preliminary Paper): Prof. H. M. Ward, F.R.S.

LINNEAN SOCIETY, at 8.—External Anatomical Characters indicating Sex in Chrysalids, and Development of the Azygos Oviduct and its Accessory Organs in Vanessa Io: Prof. W. Hatchett Jackson.—Anatomy of Lepidoptera: E. B. Poulton.—Lepidoptera of Ichang, North China: John H. Leech.

CHEMICAL SOCIETY, at 8.—The Law of the Freezing-points of Solutions: S. U. Pickering.

MONDAY, NOVEMBER 25.

ROYAL GEOGRAPHICAL SOCIETY, at 8.30.—The Bahrein Islands, Persian Gulf: J. Theodore Bent.

SOCIETY OF ARTS, at 8.—Modern Developments of Bread-making: William Jago.

TUESDAY, NOVEMBER 26.

ANTHROPOLOGICAL INSTITUTE, at 8.30.—The Ethnology of the Western Tribe of Torres Straits: Prof. A. C. Haddon.

INSTITUTION OF CIVIL ENGINEERS, at 8.—Water-Tube Steam-Boilers for Marine Engines: John I. Thornycroft. (Discussion.)

UNIVERSITY COLLEGE BIOLOGICAL SOCIETY, at 5.15.—A New Genus of Polychæt Worm: Florence Buchanan.

WEDNESDAY, NOVEMBER 27.

SOCIETY OF ARTS, at 8.—Scientific and Technical Instruction in Elementary Schools: Dr. J. Hall Gladstone, F.R.S.

THURSDAY, NOVEMBER 27.

INSTITUTION OF ELECTRICAL ENGINEERS, at 8.—Electrical Engineering in America: G. L. Addenbrooke.

FRIDAY, NOVEMBER 29.

INSTITUTION OF CIVIL ENGINEERS, at 7.30.—Principles of Iron Foundry Practice: G. H. Sheffield.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Pubblicazioni del Real Osservatorio di Palermo, vol. iv. (Palermo).—Obeah; Witchcraft in the West Indies: H. J. Bell (Low).—Through Atolls and Islands in the Great South Sea: F. J. Moss (Low).—The Lesser Antilles: O. T. Bulkeley (Low).—Humanitism: W. A. Macdonald (Trübner).—Memoirs and Proceedings of the Manchester Literary and Philosophical Society, vol. ii., 4th series (Manchester).—Report on the Mining Industry of New Zealand, 1889 (Wellington).—Reports on Mining Machinery and Treatment of Ores in Australian Colonies and America (Wellington).—Die Labyrinthodonten der schwäbischen Trias: E. Fraas (Stuttgart, E. Schweizerbart'sche).—The Butterfly; its History, &c.: J. Stuttard (Unwin).—A Glossary of Biological, Anatomical, and Physiological Terms: T. Dunman and V. H. W. Wingrave (Griffith, Farran).—An Introduction to the Study of Shakespeare: Dr. H. Corson (Boston, Heath).—On the Animal Alkaloids: Sir W. Aitken, 2nd edition (Lewis).—Matebele Land and the Victoria Falls, 2nd edition; F. Oates, edited by C. G. Oates (K. Paul).—Euclid's Elements of Geometry, books i. and ii.: H. M. Taylor (Cambridge University Press).—Travels in India by Jean Baptiste Tavernier, 2 vols.: V. Ball (Macmillan).—Results of Meteorological Observations made in New South Wales during 1887; H. C. Russell (Sydney, Potter).—Ethnographische Beiträge zur Kenntniss des Karolinen Archipels: J. S. Kubary (Leiden, Trap).—Les Animaux et les Végétaux Lumineux: H. Gadeau de Kerville (Paris, Baillière).—Bibliographie Générale de l'Astronomie, tome premier, 2nde partie: J. C. Houzeau and A. Lancaster (Bruxelles, Hayez).—The Evolution of Sex, Prof. P. Geddes and J. A. Thomson (Scott).—Synthèse Scientifique et Philosophique: A. H. Simonin (Paris, E. Leroux).—The State: W. Wilson (Boston, Heath).—Notes on Sport and Ornithology: late Crown Prince Rudolf of Austria; translated by C. G. Danford (Gurney and Jackson).—Blackie's Geographical Manuals; No. 2, the British Empire; Part 1, The Home Countries: W. G Baker (Blackie).—Gold-Fields of Victoria; Reports of the Mining Registrars for the Quarter ended June 30, 1889 (Melbourne).—Victoria; Annual Report on the Working of the Registration and Inspection of Mines and Mining Machinery Act during the Year 1888 (Melbourne).—Magnetism and Electricity, Advanced and Honours Questions: A. Jamieson (Griffin).—Electrical Engineering, Ordinary and Honours Questions: A. Jamieson (Griffin).—Results of Rain, River, and Evaporation Observations made in New South Wales during 1888: H. C. Russell (Sydney, Potter).—Astronomical and Meteorological Workers in New South Wales, 1778–1860: H. C. Russell (Sydney, Potter).—The Thunderstorm of October 26, 1888: H. C. Russell.—On a Self-recording Thermometer: H. C. Russell.—President's Address by H. C. Russell at the First Meeting of the Australian Association.—The Source of the Underground Water in the Western Districts: H. C. Russell.

CONTENTS.

THURSDAY, NOVEMBER 28, 1889.

MR. STANLEY.

MR. STANLEY'S latest letters, which have been exciting universal attention, present as fascinating a record of travel, adventure, and geographical discovery as any that has ever awakened the interest of civilized mankind. It is impossible to read them without the warmest admiration for the writer's resolute energy, inexhaustible resource, and dauntless courage. No previous traveller can have been confronted by a greater number of formidable—often apparently insurmountable—difficulties. Mr. Stanley never allowed himself to be disheartened by the obstacles in his way, but pressed steadily on, varying his methods to meet changing needs, until the immediate object of his great enterprise was attained. Not the least serious of his perplexities sprang from the reluctance of Emin Pasha to be "rescued." It was not unnatural that Emin should hesitate to quit a region for which he had made so many sacrifices, and with regard to which he had entertained so many hopes; but it is certain that if he had remained he would soon have fallen a victim to treachery. Happily, Mr. Stanley, after many an argument, succeeded at last in overcoming his scruples and hesitations, and on April 10 the two men, accompanied by a party of about 1500 persons, including native carriers, started from the southern shore of Albert Nyanza on their homeward journey. No part of Mr. Stanley's narrative is more interesting than that in which he tells the story of his efforts to persuade Emin that he might with honour resign a task which had already been practically taken out of his hands. The tale brings out vividly a most striking contrast between two types of character, each of which in its own way commands our sympathy and respect.

The scientific results of Mr. Stanley's journey are full of interest, and form a most important addition to our knowledge of Central Africa. On April 11 (NATURE, vol. xxxix. p. 560) we gave an account of his geographical discoveries so far as they were then known; and anyone who will consult the map which we printed on that occasion will be able to trace without difficulty the main lines of the explorer's later course. In 1877 Mr. Stanley discovered Muta Nzige, which he now calls Lake Albert Edward. This lake is less extensive than was originally supposed. At the time of its discovery it could not be determined whether its waters were discharged into the Nile or the Congo, but now Mr. Stanley has found that it is one of the feeders of the former river. It receives all the streams of the south-western part of the Nile basin, just as Victoria Nyanza receives all the streams of the south-eastern part of the Nile basin. The two lakes discharge their waters into Albert Nyanza, whence flows the White Nile. Lake Albert Edward and Albert Nyanza are connected by a river called the Semliki, whose valley Mr. Stanley vividly describes.

Lake Albert Edward occupies the south-western end of a great area of depression, at the north-eastern end of which lies Albert Nyanza. This area of depression lies between 3° N. lat. and 1° S. lat., and is from 20 to 50 miles broad. East and west of it rise extensive uplands, those on the western side forming the water-parting between the Nile and the Congo. Towards the east, beyond the valley of the Semliki—that is, the central part of the line of subsidence—is a great mountain range called Ruwenzori, "the Mountains of the Moon," culminating in peaks which Mr. Stanley estimates to be between 18,000 and 19,000 feet. Past this splendid range the party advanced on their way southwards. Says Mr. Stanley:—"Much as we had flattered ourselves that we should see marvellous scenery, the Snow Mountain was very coy, and hard to see. On most days it loomed impending over us like a tropical storm-cloud ready to dissolve in rain and ruin on us. Near sunset a peak or two here, a crest there, a ridge beyond, white with snow, shot into view, jagged clouds whirling and eddying round them, and then the darkness of night. Often at sunrise, too, Ruwenzori would appear fresh, clean, brightly pure; profound blue voids above and around it; every line and dent, knoll, and turret-like crag deeply marked and clearly visible; but presently all would be buried under mass upon mass of mist until the immense mountain was no more visible than if we were thousands of miles away. And then, also, the Snow Mountain, being set deeply in the range, the nearer we approached the base of the range, the less we saw of it, for nigher ridges obtruded themselves and barred the view. Still we have obtained three remarkable views—one from the Nyanza Plain, another from Kavallis, and a third from the South Point."

Lieutenant Stairs tried hard to reach the loftiest summit, but succeeded only in attaining a height of 10,600 feet, which was separated from the snow-covered peaks by deep ravines. He is of opinion that the central mass of the Ruwenzori range is an extinct volcano, and that certain jutting pinnacles on the sides of the mountains are survivals of the time when volcanic forces were in full activity. So much of the *débris* is borne along by the Semliki that the southern part of Albert Nyanza is being rapidly filled up.

Mr. Stanley has much that is new to tell us, not only about Albert Nyanza and Lake Albert Edward, but about Victoria Nyanza, a great south-western extension of which he has discovered. About the many tribes through whose territories he passed he has also a vast amount of curious and suggestive information, offered with all the freshness due to his immediate contact with the facts he describes. Nothing could be better in its way than his account of the Wakonju, a tribe from whom he and his people received much kindness. They occupy the slopes of the Ruwenzori Mountains, on which some of their villages are built at a height of 8000 feet. Here they have taken refuge from their enemies the Warasura. It is noteworthy that in many parts of the Central African uplands which he visited Mr. Stanley found a physical type which he identified with that of the Abyssinians. On these and many other points of interest the world may expect soon to receive from him further enlightenment. Meanwhile, we desire to join most cordially in the expressions of high appreciation that have been everywhere evoked by his success, and by the great qualities of intellect and character by which it has been achieved. Such geogra-

phical labours as his are unsurpassed in hardship, and the results obtained make his work one of the most important and fruitful researches of the time.

THE HABITS OF THE SALMON.

The Habits of the Salmon. By John P. Traherne. (London: Chapman and Hall, 1889.)

THE Stormontfield breeding-ponds have taught us much of the history of the salmon from the eggs to the smolt stage. After that he passes to the sea, beyond the reach of observation, and, with the exception of what we have learned from the return to the rivers of fish that have been marked before their passage to the sea, all that purports to be knowledge of the habits of the fish is really only guesses at truth.

Theories by a practical salmon-fisher, of wide experience, are entitled to respectful examination. This Major Traherne can claim; more than that he does not claim. The arrangement of the chapters in the book is objectionable as tending to confusion. It would be preferable to take first the chapter on smolts, and then to follow the life of the fish through its grilse, salmon, and kelt stages.

Notwithstanding that "smolts bred in the Stormontfield, Howietown, and other fish-ponds have never as yet been known to evince the least desire to go to sea before the spring months," yet Major Traherne is of opinion, and supports his opinion with good evidence, that there is a double emigration of smolts—autumn as well as spring. Smolts that are bred artificially are always the produce of ova spawned in November, and these form the spring migration. It is assumed that the later spawned ova form the autumn migration. If this be so, it may explain the mystery of the spring and summer run of fish. It is proved that smolts leaving Stormontfield ponds in the spring have returned to the river as grilses in July of the same year, having increased in weight from 3 to 9 pounds each, the grilse caught on July 1 weighing 3 pounds, and that caught on July 31 weighing 9½ pounds. The smolt would probably weigh about 2 ounces, and the rapidity of growth, without any expense for feeding, should make those who have charge of salmon legislation ponder over the problem of close time.

What, then, becomes of the autumn emigration of smolts? Do they come back as spring salmon? The first run of spring salmon, like the first run of grilse, is small in size. From 8 to 10 pounds would be the average weight of the first run of spring fish. The spring smolt takes three months to return a grilse; the autumn smolt would have five months to return a spring salmon.

We quite agree with Major Traherne that spring fish stay in the rivers to spawn. We also think, from the appearance of the fish, that the early, small spring fish are maiden fish that have never spawned. Are they not the autumn smolts?

But all rivers do not have a run of spring fish. Major Traherne says: "I notice that early ascending salmon are far more numerous in rivers that have an annual close time commencing on or before September 1, than in rivers where the close time commences after that date." This is simply a confusion of cause and effect. It is the early river that causes the early close time, not the early close time that causes the early river. What causes a river to be early? or, in other words, what causes spring fish to run up one river, and not to run up another? Major Traherne replies, the temperature of the river. He contrasts the early arrival of salmon in Loch Naver with their late arrival, by way of the Thurso, in Loch More, and he says that the River Naver, being fed by a large, deep loch, is warmer than the Thurso, which runs from a small shallow loch; therefore the earlier run of fish into Loch Naver! But the fish run as early up the Thurso River as they do up the Naver River; so this illustration fails. He afterwards refers to the Shin, the Cassley, and the Oykel, all of which rivers empty themselves into the Kyle of Sutherland. He says that the temperature of the water in the Shin—a river flowing from a very large lake—is higher than the temperature of the Cassley, or the Oykel, which are not fed by big lakes; and that this is the reason why the Shin is the only river, running into the Kyle of Sutherland, which produces early salmon. We reply by denying the premise. The Shin may be a rather better early salmon river than the Oykel, but it is not an earlier river. The opening day always finds clean fish in the Oykel, and, this year, from one bank, the Oykel yielded thirteen fish in March. Last year the yield of one bank of the Oykel in April was twenty-three fish; both banks of the Shin yielding thirty fish. Twenty fish in March would be a good yield for the Shin.

But to come back to the question, What causes a river to be early? Certainly it is not the absolute temperature of the river. On the north and east of Scotland the rivers are early, on the west coast they are late. The temperature of the rivers on the west is higher than that of the rivers on the north and east. Contrast the rivers Oykel and Inver. The former rises in the eastern slopes of Ben More in Assynt, and is fed in March and April by the melted snows. It has not any big lock as a reservoir, and in March is often frozen over. The Inver runs out of Loch Assynt at the western foot of Ben More. Little snow lies on the western side of the hill, and Loch Assynt is large and deep. The water of the Inver is higher in temperature than the water of the Oykel. The rivers lie opposite to one another in Sutherlandshire; the Oykel, icy cold in the spring, running east; the Inver, much warmer, running west. The cold river is an early river; the warm river is late. Major Traherne is therefore wrong when he says that the high temperature of a river makes it early. We say that the relative temperature of the river to the sea into which it empties itself determines the run of the salmon. If the temperature of the river closely approximates to the temperature of the sea the fish will run, no matter how cold both river and sea may be. On the west coast the sea is so warmed by the Gulf Stream that the rivers on that coast, although positively warmer than on the east coast, are, relatively to the sea, colder, and they are accordingly late rivers.

The relative temperature of the air and the water has a great effect, too, upon the feeding of the salmon. Major Traherne says: "I never expect to meet with a

blank day in the coldest weather, if I know there are fish in the river." A cold mist coming on will always prevent fish from rising. On a fine April day, when the sun is bringing down snow water, the time to take fish is after the sun has warmed the river, but before the snow melted by the sun about the sources of the river has had time to run down and chill the water. In both cases it is a question of the relative temperature of the air to the water.

"Do salmon feed in fresh water?" is one of the questions the author asks. He answers it in the affirmative, as he cannot believe that fish rush at spinning baits, eat prawns, and chew up a bunch of lob-worms simply to gratify the angler's love of sport. It is difficult, indeed, to understand how the theory of salmon living for months in fresh water "on his own fat, which has been accumulated while feeding in salt water"—as Dr. Francis Day puts it—could have been accepted by him, or by the late Frank Buckland. Why are good salmon rivers bad brown trout rivers? Simply because the salmon feed on the trout.

The question of close time Major Traherne says "is the key to the situation; in other words, to the adjustment of the various claims of netting proprietors and anglers, as the prosperity of our salmon fisheries, and the increase or decrease of a most valuable article of food depends in great measure upon the periods fixed to suit each river." This means that the proper adjustment of close time to each river will divide the clean fish fairly between the upper and lower proprietors, and will also provide abundant spawning fish to fill the beds upon the upper waters. At present the weekly close time in England and Scotland, extending from 6 p.m. on Saturday to 6 a.m. on Monday, is too short to enable fish to run past all the nets on many of our rivers; the upper nets sweeping in on Monday morning most of the fish that left the salt water on Saturday night. Again, the rod fishing is kept open too late. We have constantly seen gravid fish taken in October, out of which the eggs or milt ran when the fish were landed—fish that were neither able to fight, nor fit for food. Late in the season the gravid fish will take any bait as voraciously as the kelts in early spring, and the angler is able to state that he killed his six or eight heavy fish a day. After being kippered they are just eatable, and that is the best that can be said for them. On the other hand, with each of the female fish—and most of the fish killed at the end of the season are hen fish—perish some 20,000 eggs fully developed. All that Major Traherne says about the weekly close time, as well as about the closing of the fishing in the autumn, deserves careful consideration.

AN ELEMENTARY TEXT-BOOK OF GEOLOGY.

An Elementary Text-book of Geology. By W. Jerome Harrison, F.G.S. (London: Blackie and Son, 1889.)

It is well known that there are certain things, which, like reading and writing, come by nature, such as the driving of a gig, and the management of a small farm. These every man can do. And till lately it seems to have been very generally held, that, when a man or woman had shown by repeated failure that he or she was hopelessly incompetent to earn bread in any other way, there was nothing to forbid him or her from opening a school for small children: the laying of the foundations of an education was such a simple matter that it was within the reach of everyone. It looks also as if the writing of an elementary text-book on a scientific subject is very generally held to be an equally easy task, at least the bounteous profusion with which such books are showered upon us would appear to point to such a conclusion. But anyone who has tried to teach or to write a book that shall be used for teaching purposes, knows only too well that it is with the beginner and in the elements of his subject that the real difficulty lies. And besides the inevitable obstacles to success which from the nature of things he must meet with here, there are to be taken into account others of a more artificial kind. An elementary text-book must be cheap; neither author nor publisher can be expected to be wholly indifferent to profits, and only cheap books pay in science; but, setting this consideration aside, it is of the first importance that the work should be within the reach of the largest number possible of buyers. Cheap, and therefore small and sparingly illustrated. So here arises the first difficulty. What to leave out in the text and how far illustrations may be dispensed with.

Before these questions can be answered, the author must make up his mind what end he proposes the book shall be made to compass. For there are two most distinct purposes which a text-book may be intended to serve. It may be designed to educate the reader; or it may be put together in order to help him to get through an examination. And for books of the first kind there are two classes of readers to be provided for: some will never go beyond the elements of the subject; for others the text-book is only the first step on a journey which will lead them on through all the details and ramifications of its subject. But the needs of both classes are at the outset very much the same. Both want a basis, broad and flat in its simplicity, on which they can plant their feet firmly; not a surface so rough and jagged with complicated details that they are bewildered to know where, or whether anywhere, a secure foothold is to be found on it. For both the aim of the book must be to give fibre and sinew to the mind, not to pack into it a miscellaneous assortment of useful and interesting facts; the mastery of the book must involve not the mere exercise of memory, but the continuous use of observation and the logical faculty.

In every branch of science there are certain parts which are eminently fitted to serve these ends, and other parts which will most effectually defeat them if introduced into an elementary work. Now, in the Presidential address to the British Association at the recent meeting at Newcastle the objects which ought to be exhibited in a Museum intended for popular instruction were most lucidly marked off from those that ought not: an almost identical classification will divide those parts of a scientific subject which ought to find a place in an elementary text-book from those that ought not. In the same address an emphatic warning was given against overcrowding the cases. Equally must the writer of a text-book be on his guard against congested sentences or chapters.

Here, as in all education, the course of instruction, if it

is to be of any value for mental discipline, must lead up from the simple to the complex, from the particular and concrete to the general and abstract. To start with the nebular hypothesis in geology may claim to be taking things in their historical order, but is like giving meat to a baby of three months old. To lay before the beginner a familiar object such as a lump of sandstone or limestone; to show him how to pull it to pieces and find what it is made of; to give him reasons for the belief that it has not existed from the beginning of all things, but is a naturally manufactured product; to drive him to rummage brook, river, pond, and sea, the whole field of outdoor nature, in hopes of finding some similar product now in process of manufacture,—some such treatment as this at the outset would seem to be the way to lead a beginner on to use his hands, his eyes, and his reasoning faculties—in a word, to educate him. And at this stage only well ascertained facts, and conclusions on the soundness of which no doubt can be thrown, ought to be introduced; incomplete observations and experiments, inferences which are no more than likely, all provisional and speculative hypotheses, and all controversial matters, ought to be kept carefully in the background. We do not trust a youngster among quicksands and shaking bogs till much walking over sound ground has given him sturdy legs, sure feet, a quick eye, and sound judgment. There is a bit of advice given in the preface to the book now before us, which is not likely to do much harm because it certainly will not be followed by those for whom the book is written; but one shudders to think of the mental chaos that would result from reading every book or article on geology which can be bought or borrowed, the controversy on the Taconic System included. To encourage so omnivorous an appetite is not according to knowledge.

The limits of an article will not allow of more than the fringe of the subject being just touched upon; but enough has been said to show what seem to be the things to be striven after and the things to be avoided in a book on elementary science which aims to educate its readers.

The other kind of text-book is necessarily constructed on a totally different principle. The author's aim is to satisfy the requirements of a syllabus or code; lucky it is if he is a slave to only one, and does not vainly struggle to meet the demands of many. The reader must be fortified against every possible form of question which the ingenuity of the examiner can devise without going outside the prescribed limits; and as that ingenuity is boundless, the number of such questions must be legion. Hence arises the necessity of packing into a small compass an endless variety of subjects, with the result that only a few words can be spared for each. Each also, instead of standing out crisp and sharp with an appropriate heading to call attention to it and emphasize its importance, shares with two or three others, with which it may have only a remote connection, the cramped quarters of a single sentence. What a risk there must be in such a case that matters of great moment may be passed by unheeded! Even in a crowd we may stumble on interesting folk, but it is not in a crowd that intimate acquaintance or lasting friendships usually begin.

There is another evil in books of this kind; they foster the dangerous belief that there are short cuts to learning—a notion welcome enough in this age of hurry and unrest, when everything is to be done quickly, well also if you can, but quickly at any cost.

An amusing illustration of the educational value of the ordinary text-book may perhaps be allowed a place here. A girl, sharp enough to be worth taking pains with, came to me for assistance in the preparation for her examination. She was happy in the possession of a text-book which professed to give all the information which her syllabus required on I know not how many branches of science. She was just beginning the section on chemistry and was much exercised as to the meaning of chemical symbols. I was able to remove her difficulties, and to send her away hopeful that further progress would be easy and rapid. The latter it certainly was, for at the end of a week she came again with a beaming face; she had finished chemistry, and made some way in meteorology. I naturally demurred to her getting her geology in this fashion, and substituted for the geological section of her book a well-known primer. She repaid me and showed her appreciation of what scientific writing ought to be, by declaring that this was as good as a story-book.

But it would not be fair to take the precious compendium from which, but for a lucky accident, this girl would have derived all her knowledge of science, as a fair sample of the average text-book. On many even of the second class it is possible to look with qualified satisfaction, and, though the work before us must be placed in this class, it is good of its kind. There is life and spirit in it, and here and there its points are happily put. No one who reads it attentively can fail to get from it information which not only will be serviceable in examinations, but may be used as a stepping-stone to further progress in its subject. But I should like to call the attention of the author to a few points in which there seems to be room for improvement.

The exigencies of space demand that there should be no repetition in a book of this kind. But there is more than one case in which our author says over again what has been already said on a previous page. For instance, on pp. 71 and 72 we have much that has been previously given in chapter ii. The amount of dissolved matter in the Thames is stated twice over, on p. 11 and again on p. 73. Other cases might be quoted. The general arrangement of chapter viii. does not seem to be commendable: it is hard to see why such simple matters as ripple-marks, rain-pittings, and sun-cracks should come after the more complicated structures of foliation and faulting; what would seem the natural arrangement, of beginning with the simple, is absolutely reversed. The term current-bedding is used and partially explained on p. 22, but we do not find a full definition till p. 45.

A few cases of incomplete information and even of looseness of statement may be noted. In speaking of the consolidation of sediment by pressure, only the weight of the overlying rock is mentioned on p. 18. Whether glaciers move solely by the force of gravity, as is implied on p. 76, is to say the least a moot point. The description of fire-clay as "a fairly pure variety of clay, *containing but little water*," can hardly be said either to be accurate or complete. Marl is not clay mixed with *lime*. It is surprising to find among so many really good illustrations the time-honoured section across the Jura on p. 42, which only deserves to be preserved as about the most successful

effort that was ever made to represent things as they are not. The two paragraphs on contorted strata and inverted strata which follow are instances of the congestion which is unavoidable in text-books of the second class. It is impossible in so small a space to give the prominence which it deserves to the conception of horizontal thrust and compression, and very few readers would realize, from the few words devoted to them, the surprising character of the thrust-planes of the Scotch Highlands. It is scarcely fair to magnetite to say that it *sometimes* exhibits magnetic properties, and ferrous carbonate does not give a green, blue, grey, or purple colour to rocks (p. 70). One and only one more objection will I urge. There is a lamentable absence of geological sections. No verbal descriptions will suffice to convey to anyone, let alone a beginner, clear notions of the geological structure of a country without illustrative sections. The reader of the present work will gather from it the parts of the country in which the various formations are seen at the surface, but he will come away with very few notions as to the lie of the rocks. I cannot help feeling that the "imaginary scenes" during the several geological epochs might be usefully replaced by a set of geological sections.

A. H. GREEN.

THE FLORA OF DERBYSHIRE.

A Contribution to the Flora of Derbyshire; being an Account of the Flowering Plants, Ferns, and Characeæ found in the County. By the Rev. W. H. Painter. 8vo, pp. 156, with a Map. (London: George Bell and Sons, 1889.)

DERBYSHIRE is much the most interesting of our midland counties from a botanical and physico-geographical point of view. Geographical botanists, following Watson, divide the surface of Britain into two regions of climate—a lower or agrarian region, in which the cultivation of cereals and the potato is practicable, so far as climate is concerned; and an upper or Arctic region, in which no cultivation is possible. The agrarian region is divided into three zones, and whilst in Surrey, Hampshire, Wiltshire, and Kent, only one of these three zones is represented, in Derbyshire, Shropshire, and Cheshire, we get all three of them, and a greater area of super-agrarian zone in Derbyshire than in any other midland county. The plants of Britain, botanical geographers divide into two principal groups—the southern types, which have their head-quarters in Central Europe, and the boreal types, which have their head-quarters in Northern Europe, and grow only upon high mountains further south. The southern types are to the northern as six to one—about 1200 species against 200; but less than 50 species reach the midland counties. In Derbyshire we get a declination of surface from mountains nearly 2000 feet high down to a low level, so that it shows better than any other county how, in the centre of England, the boreal and austral elements of the flora meet and mingle together.

The whole area of the county is a little over a thousand square miles—about one-sixth that of Yorkshire. The Pennine chain, the backbone mountain-ridge of the north of England, extends for some distance into Derbyshire forming the watershed between the streams that flow into the German Ocean and the Irish Channel. We may divide the county into two unequal halves by a line that runs across it from west to east, from Ashbourne to Duffield. South of this line, with Derby in its centre, is a leve tract underlaid by new red sandstone, with a flora like that of Leicestershire, Nottinghamshire, and Warwickshire. North of this line, all the rocks are Palæozoic, and the level gradually rises. The Carboniferous limestone occupies the lower levels about Castleton, Matlock, and Buxton. This is much the most interesting part of the county, and the best known to strangers, the region of lead-mines, caverns, and romantic narrow dales, girdled by high cliffs of limestone: Miller's Dale, Monsal Dale, Ashwood Dale, Chee Tor, Chatsworth, Haddon Hall, are all familiar names alike to botanists and lovers of fine scenery; and Dovedale, Bakewell, and Rowsley are classic ground to anglers. The market-place at Buxton is over 1000 feet above sea-level, so that Buxton is on a par, so far as plants go, with Dundee or Aberdeen. The heights of Abraham, over Matlock, are about the same height above sea-level as the town of Buxton. About Castleton and Buxton the limestone reaches a height of 400 or 450 yards, and with it many plants of the lowlands; for instance, *Epilobium hirsutum*, *Galium cruciatum*, *G. verum*, *Lamium purpureum*, and *L incisum*, reach a higher level than anywhere else in the country. On the whole, the botany of the Derbyshire limestone tract is most like that of Ribblesdale, Airedale, and Wensleydale. Above the limestone in the Peak country, and around Buxton and Castleton, there is a considerable thickness of shale and millstone grit. The flora of these higher levels is poor and monotonous, but we get the cloudberry (*Rubus Chamæmorus*) on Axe-edge, the bearberry (*Arctostaphylos Uva-ursi*) on the moors round the head of the Derwent, and the whortleberry (*Vaccinium Vitis-idæa*) in several places about Buxton and Glossop. East of all these is an area of coal-measure country, the flora of which seems to be very poor, and to resemble that of the country round Huddersfield, Sheffield, and Halifax.

Mr. Bagnall has already shown, in the *Journal of Botany*, that Mr. Painter's numerical analysis, on p. 4 of the "Derbyshire Plants," classed under their types of distribution, needs material revision. Out of 532 plants universal in Britain, Mr. Bagnall's estimate, founded on Mr. Painter's detailed list of species, is 486 species for Derbyshire. In all probability, most of the other 46 species will be found if they are carefully sought; but, of the 599 species which represent the characteristically southern element in the British flora, there are 238 species in Derbyshire, or less than half. I cannot understand why the figure of the Germanic, or characteristically south-eastern plants, which is 127 for Britain as a whole, 38 for North Yorkshire, 26 for Northumberland and Durham, should be as low as 14 for Derbyshire. Out of 201 boreal British species, there are 39 in Derbyshire against 104 for the Lakes, 93 for Northumberland and Durham, and 76 for North Yorkshire. What Watson called the intermediate type, is a very interesting group; they are concentrated in the north of England, and I suspect that the principal reason of this is, that they are Montane plants with a preference for limestone. The

comparative figures are: 37 species for Britain as a whole, 33 for North Yorkshire, 21 for the Lakes, 21 for Northumberland and Durham, and 16 for Derbyshire. The total number of Derbyshire plants is 782 species out of 1425 recorded for the whole of Britain.

Mr. Painter's note (pp. 5-10) on the bibliography of the botany of Derbyshire is full and satisfactory. Unfortunately, many of the early records contained in Pilkington's "Derbyshire," and copied into the old "Botanist's Guide," are evidently inaccurate. But a great many trustworthy records, which stand on the personal authority of Mr. H. C. Watson and Mr. J. E. Bowman, are contained in the "New Botanist's Guide," of which Mr. Painter seldom takes notice. The curious *Achillea serrata*, a plant not known anywhere in a wild state, which Sir J. E. Smith describes and figures, in "English Botany," from the neighbourhood of Matlock, he does not mention at all.

As Mr. Painter explains in his preface and indicates in his title, his work is not put forward as a complete record of the flora of the county. It is not likely that much that is new will be found in the limestone tract and on the gritstone moors, but the exploration of the coal tract and level new red sandstone country is still very incomplete. A full and adequate flora of a county so interesting would be a very acceptable contribution to the literature of botanical geography. J. G. B.

OUR BOOK SHELF.

Science of Every-day Life. By J. A. Bower, F.C.S. (London: Cassell and Co., 1889.)

We have here another attempt to simplify the acquirement of a knowledge of some of the elementary facts of science, but though there is much to be commended, some points certainly require revision. With reference to the well-known experiment in which bits of straw, wood, or cork come together when thrown into a basin of water (p. 22), the author has fallen into the common error of ascribing the effect to gravitation instead of to surface-tension. If a few wax-lights or other things not wetted by water be added, it will be found that a substance which is not wetted is *repelled* by a substance which is, and that only "birds of a feather flock together." Again, with young students, loose or incomplete statements cannot be too carefully guarded against; the statement on p. 59 that 15 pounds or 30 inches of mercury is "equal to a square inch column of air to *whatever height* it may extend" is of this class.

The book is apparently intended more especially for the young people's section of the National Home-Reading Union, but it is hardly likely that many of the branches will be furnished with the necessary apparatus for the experiments. The ground covered includes the properties of matter, and the physics and chemistry of air and water.

Elementary Physics. By M. R. Wright. (London: Longmans, Green, and Co., 1889.)

In this book Mr. Wright has added to the more elementary part of his work on sound, light, and heat, the leading facts of other branches of physics, so as to form a general introduction to physical science. The subject is an essentially experimental one, and the author having learned by experience that a study of facts is the first duty of beginners, very little space is given to theoretical considerations. There is very little that is new, and indeed it is hardly to be expected. Most of the experiments are clearly described and are capable of easy performance, but one or two improvements may be suggested. On p. 4 the student is told to "cut a hole in an iron plate so that a flask filled with cold water just passes," an operation beyond most students, and we see no reason why a piece of card should not do equally well. Again, on p. 6, the making of a thermometer is hardly sufficiently detailed; having made a bulb at one end of the tube, the student is simply told to make one at the other end, but he will certainly not see his way to do this without further assistance. There are no less than 242 diagrams, but, needless to say, most of them have done good service before.

The book is excellently adapted for such a course of instruction as that laid down in the syllabus of alternative physics by the Science and Art Department.

Teacher's Manual of Geography. By J. W. Redway. (Boston, U.S.: D. C. Heath and Co., 1889.)

We have of late heard a good deal on the subject of how geography should be taught, but now we find an author who believes "that less energy devoted to improvement of methods, and a little more to the quality of the material taught, would not be amiss." The author's view of the scope of geography is much broader than that generally accepted, and, in this country at least, the title "physical geography" would be regarded as more appropriate.

The first part of the book consists of "hints to teachers," and very valuable hints they are. Oral instruction and out-of-door lessons are strongly recommended, and the author attempts to make the subject a practical one by suggestions as to the use of the moulding board for representing the various features of a country. The free use of pictures and instructive stories from authentic books of travel, especially with primary pupils, is also recommended.

In the second part, common errors, such as the assertion that "lakes which have no outlet are salt," are corrected. There is also an interesting chapter on the history of geographical names. The book is quite unique, and teachers will find much to interest as well as instruct them.

Notes on the Pinks of Western Europe. By F. N. Williams, F.L.S. Pp. 47. (London: West, Newman, and Co, 1889.)

Last week we noticed Mr. Williams's classified enumeration of all the known species of Dianthus. In the present pamphlet he gives Latin descriptions of, and English notes upon, the species of Western Europe. Out of a total of upwards of 200 species, there are altogether 55 in Western Europe, which are distributed through the different countries as follows, viz. 43 in Spain, 33 in France, 13 in Portugal, 7 in Germany, 5 each in Belgium and Holland, and 4 in England. His descriptions seem to be clear and explicit, and he has worked out carefully the geographical range of each species, but he does not give references either to published figures, or, with few exceptions, to the books and papers in which the plants have been originally described. As a rule, he admits species freely, but he unites the common European *Dianthus Seguieri* with the Chinese and Japanese *D. sinensis*, which is the parent of many cultivated forms. This gives the species a range from Portugal to Japan. Many of the West European forms are so puzzling, and the descriptions are so widely scattered, that it will be a boon both to botanists and gardeners to have them all brought together and worked out on one uniform plan.

American Resorts, with Notes upon their Climate. By Bushrod W. James, A.M., M.D. (Philadelphia and London: F. A. Davis, 1889.)

WHOEVER imagines, from the imposing exterior of this volume, that he will find much information within its covers on American health-resorts, is doomed to disappointment. In most cases he will be as well or better off if he consults a good gazetteer or geographical dictionary. It is true it contains a translation of some chapters of Dr. Woeikof's "Die Klimate der Erde"; indeed, this forms more than one-third of the volume—a singular method of producing an "original" work.

This translation no doubt contains a great deal of technical detail, but there is extremely little in it to help the ordinary inquirer to select a suitable winter or summer resort. If a possessor of this volume desired to obtain, for instance, some accurate and detailed information as to the climate of Southern California and its principal resorts, he would find the whole of this important region disposed of in less than four pages; while one of its most rising resorts, Santa Barbara, is disposed of with fourteen lines at p. 52, and exactly the same number of lines at p. 152; and another, Los Angeles, gets less than ten lines. No references to meteorological observations, and no climatological details of any kind, are contained in these extremely meagre accounts. In other parts of the book, seven or eight health-resorts are disposed of in a single page (pp. 33, 37, 44). Less than three pages are devoted to Florida and all its resorts. Again no meteorological details of any kind. Denver is disposed of in eight lines, Colorado Springs in a like number, and Salt Lake City in two lines.

It is scarcely necessary to deal seriously with a book put together in this fashion.

Idylls of the Field. By Francis A. Knight. (London: Elliot Stock, 1889.)

WITH the papers in this dainty volume readers of the *Daily News* are already familiar. In spirit and style they closely resemble the papers included in the same author's "By Leafy Ways." Mr. Knight has a genuine love for the poetic aspects of Nature, and in these "Idylls," as in his previous book, he gives many a vivid sketch of scenes and incidents by which he himself has been impressed. The text is illustrated by a number of photogravures from drawings by Mr. E. T. Compton.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

A New Logical Machine.

A STRANGE little instrument has been sent to me from Auckland, intended to illustrate the connection between the mathematical laws of thought and the laws of growth.

The machine itself is simple, and consists of two wheels so arranged that, by turning a horizontal one, a perpendicular one is made to revolve. The axle of this latter projects; and on it can be fastened a piece of cardboard. All the magic is in the precise forms of the cards sold with the machine; and of these I must now speak.

Mr. Betts, of the Government Survey, Auckland, devised a mode of stating arithmetically the main laws of thought. (He had not read George Boole's book; but his principle is, in the main, the same as that on which my husband worked.)

Mr. Betts wished to make diagrams which might represent his formulæ to the eye. Having arranged his scales, he proceeded to draw the diagrams; and found, to his surprise, that he was drawing the outlines of various leaves. These leaf-forms have been seen by many artists, who declare that they are not *conventionalizations* but true *simplifications* of leaves occurring in Nature. Mr. Betts next cut these leaf-forms out in white cardboard; cutting slits to mark the growth-lines. When one of these cards is fastened on the axle of his machine, and whirled, bands of colour appear, which differ according to the form of the leaf; but the preponderating colour is green.

When Mr. Betts told me of this by letter, I confess I hardly believed his account; but he has now sent me a machine and some cardboard leaves, and several friends have seen the colours.

Although I understand Mr. Betts's main principle, and am sure that it is identical with my husband's, I will not attempt to explain it, my object being to induce mathematicians here to put themselves in communication with this extraordinary mathematical logician, who, not knowing the calculus of Newton, has supplemented his deficiency by inventing a calculus of *form*, which is so far like in principle to that used by the Creator, as to have received from Nature the consecration of *colour*.

I have, of course, seen the colours; but, having bad sight, I distrusted my own impressions, till I had heard many persons, more fortunate than myself in this respect, describe what they saw.

The address is, Benjamin Betts, Esq., Milton Street, Mount Eden, Auckland, N.Z.

MARY BOOLE.

103 Seymour Place, Bryanston Square.

Lamarck *versus* Weismann.

MR. WALLACE's note with the above title in NATURE (vol. xl. p. 619) contains an illustration of a kind of reasoning that is so common with the post-Darwinians (I know of no other concise expression to designate this class of thinkers) that I desire to call attention to it. His remarks are *àpropos* of the twist in the skull of the flat-fishes, and of Dr. Lankester's comments on the explanation of its origin offered in his book "Darwinism." Mr. Wallace has, as it appears to me justly, ascribed the rotation of the eye of these fishes to the "transmission of a series of slight shiftings of the eye acquired in successive generations by the muscular effort of the ancestors of our present flat-fish" (Lankester, in NATURE, vol. xl. p. 568). This, observes Lankester, pointedly, is "flat Lamarckism." Now Mr. Wallace explains that he has added the following language, which he thinks negatives the explanation cited by Dr. Lankester; "those usually surviving whose eyes retained more and more of the position into which the young fish tried to twist them." Mr. Wallace then says that the "survival of favourable variations is even here the real cause at work."

In the three sentences cited from Mr. Wallace, we have the whole question at issue between the post-Darwinians and the neo-Lamarckians in a nutshell. We have stated the "origin of the fittest" and its probable cause; the "survival of the fittest"; and the *non sequitur* of the post-Darwinians closely following. I point expressly to the words of Mr. Wallace, that the "survival of favourable variations is even here the real cause at work," as containing the paralogism (as Kant would say) which constitutes the error of post-Darwinian reasoning. That survival constitutes *a* cause is clear enough, since from survivors only, the succeeding generations are derived. But it is strange that it does not seem equally clear, that if whatever is acquired by one generation were not transmitted to the next, no progress in the evolution of a character could possibly occur. *Each generation would start exactly where the preceding one did, and the question of survival would never arise*, for there would be nothing to call out the operations of the law of natural selection. *Selection* cannot be the cause of those conditions which are prior to selection; in other words, a selection cannot explain the *origin* of anything, although it can and does explain survival of something already originated; and evolution consists in the origin of characters, as well as of their survival.

The attempt to produce variations by mutilations, or by abrupt modifications of the normal conditions of plants and animals, is not likely to prove successful, as it has evidently not been Nature's way of evolving characters, although some well-authenticated instances of such inheritance are on record. And the fact that we have not as yet an explanation of inheritance, may be applied with equal force against any and all theories of evolution that have been entertained.

E. D. COPE.

Philadelphia, November 3.

Galls.

In his suggestive paper on Prof. Weismann's theory, Mr. Mivart says, while alluding to the formation of galls, "It would be interesting to learn how natural selection could have caused this plant to perform actions which, if not self-sacrificing (and there must be some expenditure of energy), are at least so disinterested."

Mr. Mivart here strikes what has always appeared to me one of the most important facts in organic nature with reference to the theory of natural selection. I have always so considered it, because it seems to me the one and only case in the whole range of organic nature where it can be truly said that we have unequivocal evidence of a structure occurring in one species for the exclusive benefit of another.

Moreover, the structure is here a highly elaborate one, entailing not only a drain on the physiological resources of the plant (as Mr. Mivart observes), but also an astonishing amount of morphological specialization. Indeed, the latter point is so astonishing, that when we study the number and variety of gall-formations in different species of plants—all severally adapted to the needs of as many different species of insects, and all presenting more or less elaborate provisions for ministering to such needs—it becomes idle to doubt that, if such cases had occurred elsewhere and with any frequency in organic nature, the theory of natural selection would have been untenable, at all events as a general theory of adaptations and a consequent theory of species. But seeing that the case of galls is unique in the relation which is now before us, it becomes reasonable to attribute the formation of galls to the agency of natural selection, if there be any conceivable manner in which such agency can here be brought to bear.

Now, although it is obvious that natural selection cannot operate upon the plants *directly*, so as to cause them to grow galls for the benefit of insects, I think it is quite possible to suppose that natural selection may operate to this end on the plants *indirectly through the insects*, viz. by always selecting those individual larvæ the character of whose excitatory emanations is such as will best cause the plant to grow the kind of morphological abnormality that is required.

This explanation encounters difficulties in some special cases of gall-formation, which I will not here occupy space by detailing; but as it is the explanation given in a course of lectures which I am at present delivering to the students here, I should like to take the opportunity, which Mr. Mivart's paper affords, of asking whether anybody else has a better explanation to offer.

George J. Romanes.

Edinburgh, November 18.

"Modern Views of Electricity."

Your reviewer (p. 5) takes rather high ground wherefrom to criticize a confessedly popular and expository book; and some of the charges of vagueness—as, for instance, that I do not definitely specify the velocity with which electricity travels in a given current—strike me as rather out of place, seeing that the same charge might be made against the treatise of Clerk-Maxwell. A want of definiteness about the constitution of the ether I must perforce admit; and I can hardly be surprised at your reviewer's want of sympathy with my struggles to convey to non-mathematicians some idea of the tendencies of modern inquiry, when I find that he thinks it "open to question whether attention has not of late years been too much diverted from the condition of the charged bodies in the electric field to that of the medium separating them."

But it is not so clear how, holding this view, he can say that the tentative theory attempted to be explained by me "is in its most important features almost identical with the old two-fluid [action at a distance] theory published by Symmer in 1759"; nevertheless, by taking a few statements from the earlier and introductory portion of my book, and caricaturing them a little, he does manage to make it appear as if the so-called "modern views" were merely a case of reversion to an ancestral type.

However, it is not on these general topics that I break a wholesome rule and reply to a review: it is because I am charged with four or five definitely misleading statements, and it is these I wish to either withdraw or justify.

First, concerning the relation between the Peltier effect and the E.M.F. at a junction. I have argued this matter out fully in the *Philosophical Magazine* for March 1886, p. 269, and have shown that the only "further assumption" needed is this:—*The measure of the E.M.F. at any section of a circuit is the work done per unit electricity conveyed past that section, or,* $dW = QdE$. Until this is disproved I regard it as axiomatic: and, so regarding it, I hold that what I have said about contact E.M.F. is true. My position in the matter is, at all events, perfectly clear and definite, and is fully explained in the *Philosophical Magazine* article referred to, as well as in several others of older date.

Second, as regards tourmaline. I certainly did not *intend* to explain pyro-electricity as due to unilateral conductivity solely, but perhaps my brief statements concerning it on p. 122 might be more cautiously worded so as to avoid any possible misconception.

Third, the "dead-water" argument against electric momentum (p. 103) is not *left* as a valid proof of its non-existence, though it is introduced as at first sight so tending; and all that my critic says against it resolves itself into a question of degree.

The same is true of what he says on the fourth point, concerning Fitzgerald and the Kerr effect; and his assertion that Fitzgerald's deductions do not coincide with the observations of Kerr and Kundt seems to me to convey a much falser impression than my nine-year-old statement (p. 323) to which he objects: "Mr. Fitzgerald, of Dublin, has examined the question mathematically, and has shown that Maxwell's theory would have enabled Dr. Kerr's result to be predicted."

Lastly, my suggested possible account of the Thomson effect (pp. 117, 120, 295), though it does not indeed altogether hold water (as both Prof. Everett and Prof. J. J. Thomson have kindly pointed out to me), breaks down for a reason entirely different from that supposed by your reviewer, who is estimating it only from his own caricature of an ether theory. The real weak point lies in forgetting that the condition required is unequal *impulse*, not simply unequal *force*.

In thus replying to objections raised, I by no means suppose that my critic has made them in any unfriendly spirit. I only feel that he has read the book rather unsympathetically, and (possibly on account of faults in the preface) has regarded it as more scientifically pretentious than its style and object at all warrant. Misleading statements as to matters of fact I have indeed strenuously endeavoured to eschew, and I trust that to very few of them shall I have, in a second edition, to plead guilty.

Oliver J. Lodge.

November 16.

Geometrical Teaching.

Mr. Woodall has called attention to an evil which, even at the present day, is more extensive and persistent than is generally supposed to be the case by those who imagine that "improved methods of geometrical teaching" are making themselves felt.

It is surprising that such a subject as Euclid, which of all subjects perhaps is best calculated to produce in the minds of young persons an exact method of reasoning, should be so badly taught. There can be, I should imagine, only one opinion as to the method of teaching described by Mr. Woodall, viz. that it is decidedly bad; and even worse, that it is perfectly useless.

It is often objected by this class of teachers that young people cannot be brought to appreciate the intricacies and subtleties of Euclid's propositions, and that, in consequence, if they be learnt at all they must be learnt by heart. But is not this a great mistake? My own experience has shown me that young persons *can* be induced to appreciate and take an intelligent interest in Euclid if it be taught intelligently. This demands some little trouble on the part of a teacher, and I suspect that a large proportion of our bad geometrical teaching is due to the disinclination of the teacher to take overmuch trouble in his work, coupled with the fact that it is often very difficult for him to get over the superstition of his own school-days, that a proposition, if it be learnt at all, must be learnt by heart, without any display of intelligent interest.

It does not seem to me to be necessary, at the outset at any rate, in order to improve the teaching, that the ordinary well-known edition of Euclid should be taken to pieces and a new and elaborate arrangement of the propositions made out of the fragments. The effective teaching of Euclid may be conducted upon the old lines, so well known to us in Potts and

Todhunter; but to make it effective our teachers must be possessed of ordinary common-sense. So long as this is absent, all the elaborate and scientifically improved editions of Euclid's "Elements" in the world will not produce the much-to-be-desired change. Let the teacher go through any edition of the first book of Euclid's "Elements" in a common-sense manner with his pupils, and he will find that, instead of the apathy and general disgust exhibited by them when undergoing the ordinary process of Euclidian cram, there will be a general air of brightness, interest, and intelligent appreciation. H.

The Yorkshire College, Leeds, November 25.

A Brilliant Meteor.

While at my observatory to-night, at 9.37 p.m., I saw the largest and brightest meteor I have seen since November 1880. It became visible near ν Eridani, and disappeared near α Leporis. The colour was a bright greenish blue, and the brightness was twice or three times Venus at greatest brilliancy. It cast a distinct shadow. J. Cockburn.

St. Boswells, N.B., November 23.

STAR DISTANCES.[1]

The festal offering contributed by Prof. Oudemans to the Pulkowa celebration is an especially appropriate one. The incidents of the long parallax-campaign can scarcely be recapitulated without recalling, in connection with the name of Friedrich Struve, the *quorum pars magna fui* of Æneas. He it was who, in Sir John Herschel's opinion (Memoirs R. Astronomical Society, vol. xii. p. 442), made the first real impression upon the problem by showing that not one of twenty-seven circumpolar stars discussed in 1819–21 could possibly have an annual parallax amounting to half a second of arc. Thenceforward, astronomers knew what they had to expect. Sanguine hopes of meeting comfortably large, and properly periodical residuals among ordinary observations, were checked, if not extinguished. The changes of stellar position reproducing, according to the laws of perspective, the movement of the earth in its orbit, were perceived to be on a scale so minute that their satisfactory disclosure lay, for the moment, beyond the range of what was feasible. Success in the enterprise, it was evident, was conditional upon the employment of more perfect instruments than had heretofore been available with a precision and vigilance of which the very idea was absent from all but a few prescient minds. Sir William Herschel seemed to have anticipated the conjuncture when he declared in 1782 the case to be "by no means desperate," although stellar parallax should fall short of a single second (*Phil. Trans.*, vol. lxxii. p. 83). The memorable "triple event," by which, almost simultaneously, at the Cape, at Königsberg, and at Pulkowa, his confidence was justified, is familiar to all readers of astronomical history. Its significance may be estimated from Bessel's admission that, until the yearly oscillations of 61 Cygni emerged from his measures in 1838, he was completely in the dark as to whether stellar parallax was to be reckoned by tenths or by thousandths of a second (*Astr. Nach.*, No. 385).

The value to students of Prof. Oudemans' synoptical view of what has since then been achieved in this direction can hardly be overstated. Not only does he record every individual result worth considering, but the tabulated particulars enable a fair judgment to be formed as to the value of each. There are, indeed, one or two cases in which a note of warning might with advantage have been added. Thus, Dr. Brünnow's small parallax for 85 Pegasi, to say the least, requires confirmation. A perfect *equability* in the mode of observing is essential in such delicate operations; but the Dunsink astronomer was himself conscious of, and noted with his usual care, a slight change, as the series flowed on, in his habit of "bisecting" the large star (*Dunsink Observations*, vol. ii. p. 38). The distance of this interesting binary system can hence scarcely be regarded as even approximately known.

Still less reliable, though for different reasons, are Johnson's measures of Castor, and Captain Jacob's of α Herculis. The parallax assigned to the latter star of $0''.062$ relative to its fifth magnitude companion cannot be other than illusory, since the pair, as evidenced by a small, but well-ascertained common proper motion, are physically connected, and must therefore be at virtually the same distance from the earth.

Forty-nine stars, all save one measured within the last sixty years, are included in Prof Oudemans' list. The exception deserves particular mention. Samuel Molyneux erected at his house in Kew Green in 1725, a zenith sector by Graham, with which he began, in combination with Bradley, a set of observations for parallax on γ Draconis. The same star had, in the previous century, been similarly experimented upon by Robert Hooke with something of a dubious success. The well-known eventual issue of Molyneux's observations was Bradley's discovery of the aberration of light; but they included besides an element of true parallactic change, brought out by Dr. Auwers's discussion in 1869,[1] after it had lain concealed among them for 142 years. The eye and hand must indeed have been faithful thus to record an ebb and flow of change profoundly submerged, at that comparatively remote epoch, in the reigning confusion betwen the real and the apparent places of the heavenly bodies.

A light-journey of sixty-five years (parallax = $0''.05$) may be considered the present limit of really measurable stellar distance. Forty of the forty-nine objects so far investigated lie—most of them certainly, a few only probably—within it. Forty stars can thus be located with some definiteness in space—forty among, say, forty millions! The disproportion between our knowledge on the point and our ignorance is so exorbitant that general conclusions seem discredited beforehand, and negative ones at any rate can have no weight whatever. Nevertheless, one remark at least is fully warranted by the evidence.

It is this, that the largest stars are not always those nearest to the earth. For to the narrow category of stars at ascertained distances belong no less than seven invisible to the naked eye, one of them in closer vicinity to us than Sirius, all than Capella, Vega, Arcturus, or Canopus. A cursory view might almost suggest—irrespective of geometrical possibilities—that stellar brightness had nothing whatever to do with remoteness. The legitimate and certain conclusion to be derived from the facts, however, is that the disparities of stellar light-power are enormous. A farthing rushlight is not more insignificant compared with the electric arc than a faint compared with a potent sun. Sirius emits 6400 times as much light as a ninth magnitude star north of Charles's Wain (Argelander-Oeltzen 11,677); our own sun falls nearly as far short of the radiative strength of Arcturus. Inequalities of the same order between the members of revolving systems emphasize this result. Sirius shines like four thousand of its own companions; and the movements of other stars are perhaps swayed by almost totally obscure bodies.

The inference that the apparent lustre of individual stars tells us nothing as regards their distance was already

[1] "Uebersicht der in den letzten 60 Jahren ausgeführten Bestimmungen von Fixstern parallaxen." Von J. A. C. Oudemans. Eine Festgabe zum 50 jährigen Jubiläum der Sternwarte zu Pulkowa. *Astronomische Nachrichten*, Nos. 2915–16.

[1] *Monatsberichte*, Berlin, 1869, p. 630. The result places γ Draconis at a distance of 35½ light-years, but with a very large "probable error" (parallax = $0''.092 \pm 0''.070$).

drawn by Dr. Huggins in 1866 (*Phil. Trans.*, vol. clvi. p. 393); it has been amply confirmed since, and cannot be too forcibly insisted upon. We are unable to place either an upper or a lower limit to stellar dimensions or intrinsic emissive intensity. Until Arcturus was proved to be immeasurably remote, few would have been disposed to credit the existence of a sun in space at least six thousand times as effulgent as ours is; but we know no reason why Arcturus itself should not be as vastly exceeded by some giant orb at the outskirts of the Milky Way; while we are equally debarred from asserting that among sixth, seventh, twelfth magnitude stars, there may not be found some minute bodies at half the distance from us of α Centauri.

But when we pass from particular to general reasoning, the aspect of the matter changes. No cause has yet been shown why the stars should be exempt from obedience to the "law of large numbers" which provides (as Prof. Edgeworth has ably shown) a clue to other labyrinths of facts. Statistics, it is true, are often misleading, but only when they are wrongly employed. The frequent misuse of a method does not justify its total rejection. And the statistical method is peculiarly liable to misuse. Attempts to get from it more than it will properly give inevitably fail; and what it will properly give are general statements which should only be generally applied. An average result may not be the less instructive because it is by its nature incapable of furnishing specific data.

The stars then *must*, on the whole, decrease in brightness as their distances increase, and they must do so according to an underlying fixed law which will be more and more closely conformed to the larger the number of instances included in the generalization. Each descent of one stellar magnitude represents a falling off in light in the proportion of $2\frac{1}{2}$ to 1; it represents, accordingly, an augmentation of distance in the proportion of the square root of $2\frac{1}{2}$, or 1·59 to 1. Theoretically, that is to say, stars of any given magnitude are 1·59 times more remote than those one magnitude superior, $2\frac{1}{2}$ times (1·59 × 1·59), where the gap is of two magnitudes, and so on. This would be strictly and specifically true if all the stars were equal; but since they are enormously unequal, the rule may be grossly misleading in particular instances, and can only, by taking wide averages, be brought to approximate closely to actual fact.

The determination of individual parallaxes has always, with astronomical thinkers, been subordinate to the higher aim of obtaining a unit of measurement for sidereal space. Hence continual attempts to fix the "average parallaxes" of classes of stars, which, however, remained futile so long as precarious assumptions supplied the place of direct information. Nor could this be obtained until the exigencies of the research had evoked improved means of practically meeting them. The earlier observers chose the subjects of their experiments entirely with a view to their successful issue. Stars likely, owing to their brilliancy, their swift motion, or both combined, to be nearer the earth than most others, were picked out for measurement, with results, each by itself of high interest, but worthless for generalizing purposes. It is only a few years since increased skill in the handling of methods authorized an extension of the range of their application. The first systematic plan for investigating "mean parallax" was proposed by Dr. Gill in 1883, and is now in course of combined execution at Yale College and the Cape. The completion last year of a section of the work enabled Dr. Elkin to deduce an average distance of thirty-eight light-years for the ten first magnitude stars of the northern hemisphere; but it would of course be folly to regard this avowedly "provisional and partial" result as a satisfactory basis for definitive conclusions about the distances of more remote classes of stars. At the most, it makes a useful temporary starting-point for some trial-trips of thought through space. Before long, however, through the exertions of Dr. Gill and Prof. Pritchard, direct measures, not only of all the first, but of most of the second magnitude stars all over the sky, will have been executed; and the proportion between distance and brightness thus established may with some confidence be used as a fathom-line for sounding otherwise inaccessible sidereal abysses.

A. M. CLERKE.

DR. H. BURMEISTER ON THE FOSSIL HORSES AND OTHER MAMMALS OF ARGENTINA.[1]

THIS handsome volume is a continuation of the author's monograph on the fossil horses of the Pampean beds of Argentina, of which the first part was published at Buenos Ayres in 1875, and is stated to have been specially brought out for the Paris Exhibition. The author has, however, not done himself justice as regards the title of this portion of the work, since, in addition to the description of remains of the horses of the Pampean, he also describes and illustrates the osteology of *Megatherium*, *Mastodon*, and *Macrauchenia*, so that a better title for this volume would have been "The Fossil Horses and other Mammals of the Pampean Deposits."

Like the former part, the text of this volume is printed in parallel columns of Spanish and German; and the execution of the plates leaves nothing to be desired, so far as a clear delineation of the essential features of the specimens portrayed is concerned. All the specimens forming the subject of this monograph, are, as we learn from the introduction, preserved in the National Museum at Buenos Ayres, of which the learned author is the Director; and, so far as we may judge from the description and figures, that collection of fossil mammals must be unrivalled in the excellence and completeness of its specimens.

The first section of the work, or that to which the title alone properly applies, is devoted to the horses; and the author commences his description by observing that the *Equidæ* differ from all other Ungulates in that the premolars are larger than the true molars. For the more generalized species of the Pampean deposits, like *Equus principalis* of Lund, Dr. Burmeister adopts the Owenian genus *Hippidium* (*Hippidion*), remarking that these forms are distinguished from the modern horses by the shorter and more curved crowns of their cheek-teeth, which are of a more simple general structure, and also by a difference in the form of the narial aperture, as well as by their shorter limbs and stouter limb-bones. In the

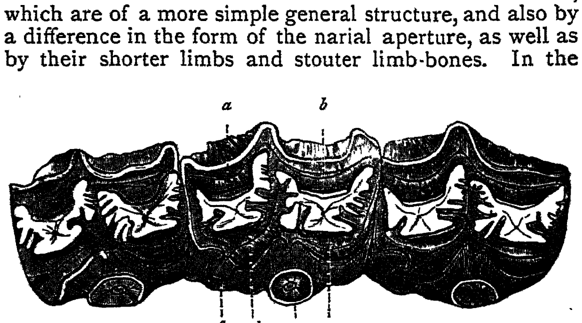


FIG. 1.—Three right upper cheek-teeth of *Hipparion*, *a*, posterior, and *b*, anterior outer crescent; *c*. anterior, and *d*, posterior inner crescent; *e*, anterior, and *f*, posterior pillar.

structure of their upper cheek-teeth the horses of this peculiar South American group make, indeed, a decided approach to the more generalized representatives of the family, such as *Hipparion*. In the latter the anterior pillar of these teeth (Fig. 1, *e*) forms, as is well known, a

[1] "Los Caballos Fósiles de la Pampa Argentina," Suplemento. ("Die fossilen Pferde der Pampasformation," Nachtrags Bericht.) By Dr. Hermann Burmeister. Folio, pp. 65, pls. 4. (Buenos Ayres, 1889.)

subcylindrical column totally unconnected with the anterior crescent (*c*); in *Hippidium* this pillar retains almost the same form as in *Hipparion*, but becomes connected with the crescent; while in the existing horses the same pillar has become greatly elongated in an antero-posterior direction. Further, in *Hippidium* the first premolar, which in modern horses is generally absent, and if present is minute and deciduous, is of very large size, and always persists.

The Pliocene *Equus stenonis* of Europe forms, however, a connecting link in respect of dental characters between the American *Hippidium* and the modern horses; and it is therefore to a great extent a matter of individual opinion whether or no the retention of *Hippidium* as a distinct genus is convenient. A new species referred to *Hippidium* is described from Tarija, in Bolivia. Of more typical horses the author describes additional remains of *Equus curvidens*, *E. argentinus*, and *E. andium*; and he adds to his description a useful word of warning in regard to the many forms of fossil horses from other parts of South America which have been described as distinct species, suggesting that all or several of these may be based merely on individual variations.

In the second section of the volume we have a description of remains of other mammals from the Pampean deposits recently acquired by the Museum at Buenos Ayres. The first of these additions is an entire skull of *Megatherium americanum*, which shows that our previous knowledge was incomplete. This skull formed part of a nearly entire skeleton of a very large individual found in August 1888 on the Rio Salado, but which is as yet but partially disinterred. It shows that instead of the aperture of the nares being bounded superiorly merely by short nasal bones which did not reach within a long distance of the premaxillæ, there was a large prenasal bone extending nearly as far as this point; while there was also a lateral process projecting forward from the upper part of the maxilla into the nasal aperture. This prenasal bone is 4½ inches in length, and it is considered probable that it became united with the nasals in the adult. Still more remarkable, however, is the presence of another ossification extending upwards and backwards from the superior surface of the extremity of the premaxillæ towards the prenasal bone, from which it is only separated by a short interval. These two ossifications, we may observe, are evidently a rudiment of the complete bony arch connecting the premaxillæ with the nasals in *Mylodon darwini*, which was on that account generically separated by Reinhardt as *Grypotherium*; and they serve to support

Fig. 2.—The third left upper true molar of *Mastodon humboldti*; from the Pampean of Buenos Ayres. Two-thirds natural size.

Prof. Flower's view that the last-named species is not separable from the genus in which it was originally placed.

The author next proceeds to the consideration of the skull of that species of *Mastodon* which he terms *M. antium*. No mention is made of the earlier name *M. cordillerum*, which appears to be the proper one for this species; and in amending the usual spelling *M. andium* to *M. antium*, one cannot help wondering why the same course was not adopted in the case of *Equus andium*. The object of this part of the work is to show that the reference by the late Dr. Falconer to *M. cordillerum* (as we will call it) of mandibles from Texas, furnished with long tusks is incorrect, and that this species really had, like its near ally *M. humboldti*, a mandibular symphysis of the same general type as that of the elephants, without any tusks at all in the adult. Figures are given of an immature and of an adult skull with the mandible *in situ* to support this redetermination. Dr. Burmeister then proceeds to institute a comparison between *M. cordillerum* and *M. humboldti*, in which he states that, although very similar, a careful examination shows very clearly the distinctness of the two forms. Here we may observe that it is to be regretted that no comment or reference is made to the notices and figures published by Falconer and other English writers in reference to these forms; but perhaps the real explanation of this omission is that the libraries at Buenos Ayres are not so well stocked as those of London. According to our author, *M. cordillerum* is the smaller of the two species; the length of the mandible from the condyle to the symphysis being 75 centimetres against 85 centimetres in *M. humboldti*; the last dimension agreeing with the British Museum skull of that species originally described by Falconer in *M. andium*. Falconer's observations as to the more complicated structure of the molars of *M. humbolti* are in the main confirmed. A small specimen of a last upper molar referred to this species in the British Museum is (with the permission of Dr. Woodward) figured in the accompanying woodcut, to show the complexity of the crown, in which the valleys are much blocked by accessory tubercles. In the early stage of wear of this specimen imperfect trefoils of dentine are shown only on the inner columns; but when more worn trefoils would evidently also appear on the outer columns. In the well-worn upper molar of *M. cordillerum*, represented in Plate x., Fig. 5, of the work before us, the absence of a distinct trefoil on the outer columns, which Falconer mentioned as one of the distinctive features of this species, is well shown. Dr. Burmeister further observes that the molars of *M. cordillerum* are characterized by their blackish enamel, and the brown or

reddish colour of the dentine; while in *M. humboldti* the whole of the crown is of a yellowish or white hue, with darker roots. These distinctive colours are very noticeable in many of the specimens in the British Museum, which have been respectively referred to the two species in question.

The work concludes with descriptions of the remains of two species of the remarkable Perissodactylate genus *Macrauchenia*, viz. the typical *M. patachonica* of Owen, and *M. paranensis*, originally described by Bravard as *Palæotherium*. Of the former species an entire skeleton is figured, and the author concludes that the genus is, on the whole, most nearly allied to *Palæotherium*, although the skull presents some remarkable resemblances to that of the tapirs. It appears, moreover, from the presence of muscular impressions on the cranial bones, that the nose formed a short proboscis, as in the latter group. The author also gives us an elaborate description of the teeth, which are undoubtedly of a Palæotherioid type. It is further observed that in the author's opinion there appear to be no grounds for generically separating *M. paranensis* and the smaller *M. minuta* from the typical genus; and the author concludes his volume with some remarks on the proposal of Dr. F. Ameghino to regard the former as the type of the genus *Scalibrinitherium*, and to adopt the name of *Oxydon*[*to*]*therium* for the latter.

The above appears to be the gist of Dr. Burmeister's new contributions to our knowledge of the wonderful Tertiary fauna of South America, which he has done so much to enrich. And we congratulate him on the results of this his latest work, and especially on the excellent illustrations by which it is accompanied, since the want of such aids to a right comprehension of the text forms such a great drawback to the work hitherto published by other contemporary South American writers on the same subject. R. L.

NOTES.

In his speech at Nottingham on Tuesday evening Lord Salisbury made a most important reference to the subject of what is called free education. He said:—"There is another question which we have heard a good deal discussed, and that is with regard to what has been, in my opinion, improperly termed free education. I should rather call it assisted education, because I do not know that anybody, however extreme his views, would desire that all the inhabitants of this country, whether rich or poor, whether capable of paying for the education of their children or not, should enjoy free education for those children at the cost of the Chancellor of the Exchequer. On the other hand, I have before expressed the opinion—I expressed it four years ago, before the two last general elections, at Newport—that by making education compulsory, by forcing the people to send their children to school whether they ask it or not, you were incurring a certain obligation to relieve the burden of that compulsion, where the circumstances of the parent were such that it was too heavy for him to bear. We believe that considerable progress in that direction may be made. We have already introduced measures to that effect in Scotland. I believe that with perfect consistency with sound principle, and merely recognizing the fact that where you enforce a duty upon a man you are bound to make it as easy for him as you can—I believe that it will be possible considerably to extend that principle in England, and very greatly to relieve the difficulties of the working man in that respect. But allow me to say that I consider the question as to its rapidity, and as to its progress, to be a question for the Chancellor of the Exchequer. If he has got the money I have no doubt he will do it, but if he has not got the money he will not. But it is an object to which I believe a great deal of the money of a Chancellor of the Exchequer may very fairly be applied." The Government is to be congratulated on the pledge thus given to consider the matter.

The Royal Society will hold its anniversary meeting on Saturday. After the meeting the Fellows will dine together.

On Tuesday the degree of D.C.L., *honoris causâ*, was conferred in Convocation, at Oxford, upon Mr. Alfred Russel Wallace. Prof. Holland presented him for the degree, and dwelt upon his labours as a naturalist in Brazil, the Malay Archipelago, and elsewhere; upon the now famous doctrines elucidated by him, and upon the relations between him and Mr. Darwin, reflecting equal honour upon both.

A Conference, called by the National Association for the Promotion of Technical Education, was held in the Manchester Town Hall on Tuesday. About 300 delegates were present from the different technical schools and associations throughout the Kingdom. The chair was occupied at first by the Mayor of Manchester, and subsequently by Mr. Rathbone, M.P. General Donnelly was present to represent the Science and Art Department, South Kensington. Sir Henry Roscoe, M.P., Sir Edmund Currie, Mr. A. H. D. Acland, M.P., and Mr. Mather, M.P., were among those present. The discussions related to the question of the working of the Technical Instruction Act, 1889. A report was read by Sir Henry Roscoe, showing that the Act was being adopted partly or wholly in a large number of towns throughout the Kingdom. The meeting will do great good, and we shall refer to it next week.

According to a circular which has recently been sent to the leading physicists, electricians, and others interested in the history of English science, it is proposed to establish a Gilbert Club, the inaugural meeting of which has been convened this day in the rooms of the Society of Arts at 4.30 p.m. The object of the Club is to do justice to the memory of the illustrious President of the College of Physicians who was in the possession of, and was actually carrying on, the true experimental method of scientific inquiry at a time when Bacon was only talking and writing about it. There can be no doubt that the claims of William Gilbert, of Colchester, have been to a great extent overshadowed by the fame of the renowned Lord Chancellor, and it is much to be regretted that we have not had handed down to us more of the results of Gilbert's labours than are to be found in his celebrated work "De Magnete," published in the year 1600. Such as it is, this work may, however, be justly regarded as the earliest English scientific classic, and its author must be recognized as the first truly philosophical investigator in the now all-important subjects of electricity and magnetism. The Club has been organized for the object of bringing out an English edition of "De Magnete" as nearly as possible in the style of the original folio edition, and to arrange for a befitting celebration of the tercentenary of this work in the year 1900. To quote the circular:—"The publication of 'De Magnete' not only marked an epoch in the science of magnetism, but constituted the absolute starting-point of the science of electricity. It has been hitherto a reproach to British electricians that they too little recognized the merits of the founder of the science." The preliminary list of members already includes the names of Sir William Thomson, Lord Rayleigh, Prof. Tyndall, Sir John Lubbock, Prof. Rücker, Prof. Lodge, Mr. Preece, Prof. Reinold, Prof. Perry, Prof. G. Forbes, Prof. D. E. Hughes, Sir F. A. Abel, Sir F. Bramwell, Sir Douglas Galton, Sir H. Mance, Colonel Festing, Captain Abney, Prof. Carey Foster, Prof. W. G. Adams, Prof. J. C. Adams, Prof. Roberts-Austen, Prof. Thorpe, Prof. G. H. Darwin, Prof. Liveing, Prof. Dewar, Prof. W. N. Shaw, Prof. Poynting, Prof. Ray Lankester, Mr. Crookes, Mr. J. Hopkinson, Mr. Glazebrook, Mr. G. J. Symons, Dr. J. H. Gladstone, Dr. B. W. Richardson, Prof. Victor Horsley, Mr. Latimer Clark, &c.

Dr. Quesneville, the French chemist, died on November 14, at the age of eighty. He took his degree of doctor o..

medicine in 1834, having studied chemistry under Chevreul. In 1840 he started the *Revue Scientifique*, a monthly periodical, which he afterwards called the *Moniteur Scientifique*. This periodical came to an end last month, Dr. Quesneville explaining that the task was rendered too severe by the infirmities of old age.

THE chemical laboratory, presented to the Stalybridge Mechanics' Institute by the late Mrs. Margaret Platt, was formally opened last week. The laboratory, which has been provided at a cost of about £600, was projected by Mrs. Platt—who always took a great interest in Stalybridge and its social and educational welfare—shortly before her death. Unfortunately she did not live to see the completion of this valuable addition to the work carried on by the institution, but her representatives have observed Mrs. Platt's wishes in every respect. The laboratory is fitted with all necessary appliances for the practical study of chemistry. At present there are twenty-two students undergoing a course of instruction.

THE ceremony of cutting the first sod on the site of the International Exhibition which is to be held in Edinburgh next year took place on Saturday last. The Lord Provost, who presided, said they were all aware that the Forth Bridge was to be opened soon, and a large number of scientific people would be present on that occasion. Therefore, it seemed a most opportune occasion to show a collection of matters connected with electricity such as had never been gathered together before. They had promises from all parts of the world, and the little difficulties that were in the way with the London Chamber of Commerce had, he believed, all been got over, and now there would be a unanimous feeling throughout the whole of the electrical world that this Exhibition should be made a great success.

THE Christmas lectures at the Royal Institution (adapted to a juvenile auditory) will this year be given by Prof. A. W. Rücker, F.R.S., on electricity. They will begin on Saturday, December 28.

THE following are the Science Lectures to be given at the Royal Victoria Hall during the month of December:—December 3, "Snakes and Snake-poison," by Dr. W. D. Halliburton; December 10, "A Visit to the Banks of the Rhine," by Mr. A. Hilliard Atteridge; December 17, "My Experiences in Cape Colony," by Prof. H. G. Seeley, F.R.S.

COUNT SALVADORI has just published the first part of a supplement to his famous work on the Birds of New Guinea and the Molucca Islands, entitled "Agguinte alla Ornitologia della Papuasia e delle Molucche." The present part consists of sixty-four pages, and relates to the *Accipitres*, *Psittaci*, and *Picariæ*, which were the orders treated of in his first volume of the "Ornitologia." During the seven years that have elapsed since the completion of Count Salvadori's work much has been done. Hunstein, who was an excellent collector, and whose untimely death by a tidal-wave in New Britain is deplored by all naturalists, made some valuable explorations in the Horse-shoe Range of the Astrolabe Mountains, and discovered the wonderful new Birds of Paradise, *Paradisornis rudolphi*, *Astrarchia stephaniæ*, and others. Mr. H. O. Forbes explored the same district, and also procured some novelties, and the adventurous expedition of the last-named naturalist and his wife to the Tenimber Islands is quite one of the exploits of the last decade. Mr. C. M. Woodford has likewise added many new species to the known avi-fauna of the Solomon Islands, so that altogether Count Salvadori has had ample material for his supplementary notes. Besides giving abundant information respecting the additional synonymy and geographical distribution of the members of the three orders treated of in the present supplement, the author adds twelve species of *Accipitres*, fourteen *Psittaci*, and nine *Picariæ*. Count Salvadori thinks that *Astur shebæ* of Sharpe from Guadalcanar is the same as *A. pulchellus* of Ramsay from Fauro, but as both species are represented in the British Museum such a mistake in identification is scarcely likely. He separates the Timor Laut *Astur*, supposed to be identical with *A. albiventris* of Bouru, as a new species, *Astur*, or as he calls it *Urospizias polionotus*. Several doubtful points among the Parrots, Count Salvadori will probably be able to settle when he comes to England and examines the series of skins in the British Museum. Of Cuckoos, he describes two new species (*Cacomantis arfakianus* and *Lamprococcyx poliurus*), and *Tanysptera meyeri* is a new Kingfisher.

IT is proposed that a meteorological station shall be established at the Bermuda Islands after the completion of the telegraph service between them and Nova Scotia. Many vessels leaving Halifax, the masters being unaware of the approach of storms from the West Indies, are dismantled before they have been out three days. The establishment of the proposed meteorological station would, therefore, be of great value, and the Canadian Government has willingly consented to bear half of the cost.

WE have received vol. xi. of "Aus dem Archiv der Deutschen Seewarte," containing the report of that institution for the year 1888. Great activity is displayed in the collection of observations at sea, not less than 740 logs and abstract journals having been received during the year, and synoptic charts of the North Atlantic have been published for four quarters, ending with August 1885. Several meetings have been held at the Seewarte for the purpose of preparing an atlas of clouds, and the work is now about to be published. In addition to several treatises on terrestrial magnetism, the volume contains (1) an article by Dr. Vettin on the volume of air flowing into or out of barometrical minima and maxima in different seasons, as determined from the direction, height, and velocity of clouds, observed at Berlin during the years 1882–83, in connection with the data afforded by the daily weather charts published by the Seewarte. (2) The rainfall conditions of Germany from 1876–85, by Dr. H. Meyer. The author has not been content with using the usual monthly values, but has investigated the daily observations from the original documents. He finds that periods of two to four rainy days are more frequent than the same periods of dry days. Periods of five or more wet days are more frequent on the coast than in the interior, but longer dry periods are more probable here than on the coast. On the coast the probability of a change from dry to wet is greater than a change from wet to dry, while the reverse holds in the interior. Periods of twenty or more wet days have occurred only in Western Germany, while the same periods of dry days are of the rarest occurrence in any part of the country.

THE Pilot Chart of the North Atlantic Ocean for November shows that, during the early part of the month of October, an extensive area of high barometer occupied the central regions of the North Atlantic; its position varied from day to day, but on the 12th its centre moved south of the 40th parallel, and low pressure prevailed over nearly the whole of the Transatlantic routes until the 19th. At this date an area of high barometer passed eastward from the American coast, and slowly traversed the ocean, reaching the British Isles towards the end of the month. Several storms occurred north of the 50th parallel, and also along the Transatlantic routes east of the 50th meridian. Two cyclones of great violence occurred off the Atlantic coast of the United States. One developed quite suddenly on the 14th, 150 miles east of Hatteras, and after lingering there for four days, started off rapidly to the eastward; the other storm, which was central off the Carolina coast on the 23rd, was remarkable for its violence and its increase of energy after reaching the Gulf Stream. Several other storms of minor importance occurred on that coast during the month. Comparatively little fog was

experienced, but ocean ice prevailed in considerable quantity to the eastward of the Straits of Belle Isle, and to some extent on the Grand Banks, in marked contrast with what is usually experienced at this time of year.

A CURIOUS dwarf Japanese tree, *Thuja obtusa*, brought by Mr. Samuel from the Paris Exhibition, was exhibited at the meeting of the Royal Botanic Society on Saturday last. The specimen was only some two feet high, and was stated to be about 130 years old. The secretary said that these dwarf Japanese trees were good illustrations of the power of endurance of plants and trees under severe ill-treatment. In the Society's garden may be seen several specimens of the common oak, between forty and fifty years old, yet only some ten or twelve inches in height. They were planted as an edging to a flower border, and kept clipped like the old-fashioned box.

THE greatest depth found by Captain Spratt in the Western Mediterranean basin was between Sicily, Sardinia, and Africa (about 10,600 feet). Recent measurements in the eastern basin by Commander Magnaghi, of the Italian Navy (*Riv. Sci. Ind.*) have yielded, as maximum depth, 13,556 feet, between the Islands of Malta and Candia.

AT the annual meeting of the Severn Valley Field Club, at Wellington, in January last, Dr. Callaway, the President, was asked to prepare a report of the year's proceedings with a shorter account of the work of the preceding year. These reports have now been issued, and show that a resolute effort is being made to promote a taste for geology and natural history in the district, and to make the Field Club something better than a picnic society.

COLONEL WOODTHORPE recently delivered, at Simla, a lecture on the Aka Expedition of 1883. It may be remembered that this tribe, which inhabits the hills north of Assam, owing to some forest disputes and a supposed interference with their trade in rubber, seized two of our forest officers and carried them off. To recover these men, a small expedition was despatched, under the command of Colonel Woodthorpe. The Aka houses are built on piles raised above the ground, with a large space at one end, where the children play. The dress consists of a tunic of Tibetan cloth, and trousers, reaching to the feet, made of thin white material. Long trousers are worn to keep off the *damdum*, a troublesome little fly or mosquito. Bows and arrows and knives, with blades easily detachable from a bamboo handle, are the chief weapons. The barbs of the arrows are dipped in aconite, and are so treated that, when any attempt is made to pluck out the arrow, the barb breaks off and remains in the wound. The poison is so deadly, that even a buffalo usually falls, after running a few yards, when he has been struck by one. Some of the superstitions of the Akas are curious. If a river runs between an Aka's house and his burying-place, his soul can never go home after death. This inability of the spirit to cross water is, however, overcome, and, every year, Akas may be seen stretching a string across the stream that divides the grave from the house of the departed. The ghost can easily cross when the slightest foothold is given him.

IT is sometimes said about old trees (*e.g.* an old lime in the new Gardens at Potsdam) that the present branches are properly roots; and it has been reported that trees may be planted, and will grow, in the inverted position. A scientific inquiry into this matter has been made by Herr Kny, in Germany, taking a number of plants of wild vine (Ampelopsis) and ivy, about 3·5 metres high. In 1884 he planted these with both ends in the ground; and in the spring of 1885, after the tops had rooted, he cut the arch at its highest point. In the first year two of the plants died, but the others (twelve vine and fourteen ivy) grew vigorously, and were still alive this last spring. To test the extent of the inversion, he cut slips from the inverted plants, and planted them in a greenhouse, some with their natural, and some with their artificial upper end uppermost. It appeared that the callus, from which the roots spring, was formed at both ends, but more readily at the naturally lower end, whether this was above or below, in the experiment. Herr Kny considers that, notwithstanding several years' successful culture, the inversion was not thoroughly completed. He proposes to continue his investigation, and invites people who have gardens to make like experiments with other plants, recommending willows, poplars, and roses.

THE latest Colonial Report from Basutoland contains a statement by Sir Marshall Clarke on education in that State, written at the request of Lord Knutsford. The total amount granted by the Government during 1888 for educational work was £4581 amongst four missions, of which £2900 went to the Paris Evangelical Missions. The number of schools receiving Government aid was 100, with a nominal roll of 4053, and an average attendance of 3480. The education offered is, for the most part, of an elementary character, suitable to a people of agricultural pursuits, whose children are withdrawn early for labour in the field. It consists of reading and writing in Sesuto, and a little elementary arithmetic and English. A higher education is offered at the missionary centres. The number of schools under direct European supervision is 21, with about 1400 pupils on the attendance roll. At Morija, the head-quarters of the Paris Evangelical Missionary Society, the training school affords a sound English education, the staff being composed of well qualified Europeans. There is an interesting girls' school at Roma, the chief Roman Catholic mission station, where the pupils are instructed in carding, spinning, weaving, and the elements of dressmaking, as well as in English and Sesuto. Schools receiving Government aid are, from time to time, inspected by Government officers, who check the attendance rolls, examine the pupils, and, at the end of the year, submit reports from each district.

MR. H. Y. L. BROWN, the Government Geologist of South Australia, returned to the Angle Pole head camp from his exploration trip to the Musgrave Ranges on October 7. According to the *Colonies and India*, the route was *viâ* Cootanoorina and Arkaringa Creek to Glen Ferdinand, a trigonometrical depot. The exploration extended among the ranges to longitude 131° E., latitude 26° S. Mr. Carruthers, the Government Trigonometrical Surveyor, starting from the depot, will continue the survey towards the western boundary, and expects to return in January. The Government Geologist returned *viâ* the River Alberga, striking the telegraph line at the Angle Pole.

FROM the Report of the Ceylon Survey Department for the past year, which has just been issued, it appears that when the calculations of the northward running chain of the 13-inch triangulation were completed, it was found that the computed distance between the two stations at Delft Island differed from that of the Indian system to such an extent as to show a considerable error, probably in the Ceylon work. The resulting error is too small to be appreciable on maps even of the largest scale, but, from a geodetical point of view, the outcome of so much work extending over a large number of years is disappointing. In order to verify the previous work, Colonel Clarke purposes carrying at an early opportunity a new system of triangles along the west coast, utilizing as many as possible of the old stations. A tentative scheme for the triangulation of the west coast has been drawn up, and when an officer is available, he will be sent to inspect the country, and report on the feasibility of the scheme. In consequence of the incompleteness of the diagrams and other records, the construction of a new series of diagrams, in which will be inserted the information gained

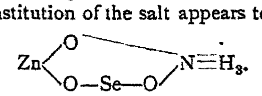
from an exhaustive examination of the record books, will be commenced. In the past year sixty-one sheets were scored under the superintendence of the Trigonometrical Assistant, each representing an area of 13·6 miles by 8·8 miles, and containing in all 1687 fixed stations. He has also prepared an elaborate map of the island, showing sheet line distances.

THE Report for the past year on the mining and mineral statistics of Canada, by Mr. H. P. Brumell, of the Dominion Geological Survey, has been received in this country. The total value of the production of minerals of all kinds for the year was $16,500,000—an increase of 1,500,000 as compared with 1887, and 6,000,000 against 1886. Coal is the largest mineral product of the Dominion, the value of last year's yield amounting to $1,098,610, as against $1,178,637, in 1887, and $1,330,442 in 1886. The decrease in the yield of gold has been anticipated for some years. Copper was mined to the value of $667,543, and these figures will in all probability be doubled this year, in view of the rapid development of the Sudbury and Lake Superior Mines. The asbestos yield amounted to $255,007, and the phosphate production shows an appreciable increase.

THE Smithsonian Institution has issued a "Preliminary Catalogue of the Shell-bearing Marine Mollusks and Brachiopods of the South-Eastern coast of the United States," by W. Healey Dall. The volume includes admirable illustrations of many species.

WE have received the sixty-second part of the first division of the "Encyclopædie der Wissenschaften," and the fifty-fourth and fifty-fifth parts of the second division of the same work (Breslau, Trewendt). The first of these three parts is a contribution to the hand-book of botany included in this Encyclopædia; the second and third conclude the seventh volume of the Encyclopædia's Dictionary of Chemistry.

A NEW series of well crystallized salts, ammoniacal selenites, are described by M. Boutzoureano in the current number of the *Annales de Chimie et de Physique*. Most normal selenites are found to be readily soluble in strong ammonia, and the solutions on evaporation either in the air or *in vacuo* deposit crystals of ammoniacal selenites. Four of these interesting salts have been studied in detail, those of zinc, cadmium, copper, and silver. Ammoniacal zinc selenite, $ZnO.SeO_2.NH_3$, is obtained by dissolving neutral zinc selenite, $ZnO.SeO_2$, a salt which crystallizes in rhombic prisms, in strong ammonia at the ordinary temperature. On allowing the solution to spontaneously evaporate, crystals of the ammoniacal salt are deposited in the form of fine long prisms capped by domo-prisms belonging to the rhombic system. The crystals are insoluble in water, which appears to exert no action whatever upon them. They are also unchanged by heating to 100° C., but when heated in a sealed tube the selenious oxide is reduced by the hydrogen of the ammonia with evolution of water vapour and sublimation of selenium. On ignition they are completely converted to zinc oxide. Acids readily dissolve the crystals even when largely diluted with water. The constitution of the salt appears to be



Normal cadmium selenite, $CdO.SeO_2$, is also soluble in ammonia, and the solution leaves on evaporation white rhombic crystals of an ammoniacal cadmium salt, $CdO.SeO_2.NH_3$, analogous to the zinc salt. These crystals are likewise unattacked by water, and are stable at 100°. They also give off water and vapour of selenium when heated in a sealed tube. The most beautiful salt of the series, however, is the ammoniacal copper selenite. Copper forms a normal selenite of the composition $3(CuO.SeO_2).H_2O$, which crystallizes in small green monoclinic crystals. These crystals readily dissolve in ammonia, forming a deep bluish-violet solution, which on slow evaporation in the air yields magnificent blue crystals of the ammoniacal salt belonging to the triclinic system. The salt is found to contain one molecule of water, and is represented by the formula $CuO.SeO_2.NH_3.H_2O$, the constitution being probably more nearly expressed in the following manner,

$$H_3{\equiv}N{<}\begin{matrix}CuO{-}OH\\ SeO{-}OH\end{matrix}.$$

Unfortunately these fine crystals soon alter in contact with air, losing their water and ammonia and becoming covered with a green coating of basic copper selenite. Water has apparently no action upon them, but in reality there is a surface action, the coating of basic selenite thereby formed preventing any further decomposition. In a similar manner silver is found to form an ammoniacal selenite, the crystals belonging, like those of the copper salt, to the triclinic system. They are anhydrous, $Ag_2O.SeO_2.NH_3$, and are blackened by exposure to sunlight. Thus the series is seen to be a very well defined one, the members consisting of normal selenites combined with one molecule of NH_3, generally anhydrous, but occasionally, as in case of the copper salt, containing water of crystallization.

THE additions to the Zoological Society's Gardens during the past week include a Barbary Ape (*Macacus inuus* ♂), a Saker Falcon (*Falco sacer*) from North Africa, presented by Captain Augustus Kent; a Malbrouck Monkey (*Cercopithecus cynosurus* ♂) from West Africa, presented by Dr. Messiter Lang; two Fieldfares (*Turdus pilaris*), British, presented by Mr. J. Young, F.Z.S.; a Golden-naped Amazon (*Chrysotis auripalliata*) from Central America, purchased; a Molucca Deer (*Cervus moluccensis*), born in the Menagerie.

OUR ASTRONOMICAL COLUMN.

OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at Greenwich at 10 p.m., November 28 = 2h. 31m. 57s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G. C. 575	—	—	2 33 30	+38 19
(2) ρ Arietis	6	Yellowish-red.	2 49 37	+17 53
(3) ξ′ Ceti	5	Yellowish-white.	2 7 12	+ 8 20
(4) γ Ceti	3	White.	2 37 36	+ 2 46
(5) D.M. + 57° 647 ...	9	Reddish-yellow.	2 42 51	+57 24
(6) R Ursæ Minoris ...	Var.	Reddish-yellow.	16 31 18	+37 34
(7) V Geminorum ...	Var.	—	7 16 59	+13 18

Remarks.

(1) Sir John Herschel describes this nebula as: Very bright, very large, very much extended, very much brighter in the middle. Dr. Huggins noted, in 1866, that the spectrum was continuous, but pointed out in his remarks that this was not to be understood to mean more than that, when the slit was made as narrow as the feeble light permitted, the spectrum was not resolved into bright lines. Further observations are therefore required, for it may be that slight brightenings in the apparently continuous spectrum were overlooked in the early observations. The case of the nebula in Andromeda indicates that, in some of the nebulæ of this class, bright carbon flutings may be superposed upon the continuous spectrum, in which case they will not be very obvious. The carbon flutings seen in the spectrum of the flame of a spirit-lamp are convenient for comparison in an observation of this nature.

(2) This is a typical star of Group II. Dunér describes it as superb and brilliantly developed, the bands 1–9 being perfectly visible. The star therefore affords an opportunity of observing the bright carbon flutings and checking their positions. If they

are very bright, the compound structure, as seen in the spectrum of a spirit-lamp or the base of a candle-flame, may be looked for. The star falls in species 9 of the subdivision of the group, and is accordingly of about mean condensation. Dark metallic lines will probably be found to make their appearance about this temperature, and the presence or absence of *b*, D, or other lines should therefore be noted.

(3) Vogel classes this with stars of the solar type, but is doubtful whether it does not belong to Group II. It is most likely that it is at an intermediate stage—either a late stage of Group II. or an early stage of Group III. There are evidently traces of some of the dark flutings, and it is suggested that the distinguishing numbers of these and the relative intensities of the lines should be noted. The observations made by Prof. Lockyer and myself seem to indicate that the bands in the red are the most persistent as the temperature increases.

(4) According to Gothard this is a star of Group IV., and the usual observations are required.

(5) This is classed with stars of Group VI. in Dunér's catalogue, but it is stated that the type of spectrum is rather doubtful. Like the star given last week, it may possibly be intermediate between Groups V. and VI., and similar observations are suggested.

(6) This is a variable star which will be at its maximum on November 30. Gore gives the period as 281·2 days, and the range as 8 at maximum to < 11·5 at minimum. The spectrum is of the Group II. type, and the suggestions made for the observation of R Tauri (see p. 68) apply equally in this case. It may be further suggested that the spectrum be observed for some time after the maximum, special attention being given to the fading out of the carbon fluting in the green (517, a little more refrangible than *b*) relatively to the other bright spaces.

(7) Gore gives the period of this variable (maximum on December 4) as 276 days, and the range as 8·6 to < 13·5. The spectrum and colour have not yet, so far as I know, been recorded, and midnight observers may therefore take advantage of the approaching maximum. A. FOWLER.

THE TOTAL SOLAR ECLIPSE OF 1886.—The report of the observations of the total solar eclipse of August 29, 1886, made at the Island of Carriacou by the Rev. S. J. Perry, has been published. The two main questions that required spectroscopic observations to answer them were:—(1) Does the absorption, which produces the Fraunhofer lines, take place mainly in a single layer of the solar atmosphere, or in concentric layers? (2) Does carbon exist in the corona? With respect to the first point, Father Perry thinks that the differences in the length of the lines which he observed before totality on the less refrangible side of *b* seems somewhat to strengthen the view that the selective absorption takes place in concentric layers. During totality a search was made for the two principal bands of the carbon spectrum. The part of the spectrum observed was from about *b* to λ560, but no trace was seen of the carbon bands. Father Perry, however, suggests that perhaps the intensity of the carbon spectrum may vary in each eclipse, and may have some direct connection with the amount of solar activity. Some sketches of the coronal streamers are appended to this report.

Mr. H. H. Turner's report of the observations of the same eclipse, made in the Island of Grenada, has also been received. The following is a list of the lines seen and the order in which they appeared:—

h. m. s.		
7 7 45	...	F line appeared.
7 8 55	...	4923 appeared; very short.
7 11 30	...	4923 and 4933. Immediately after, many lines appeared.
7 12 0	...	Totality.
7 20 50	...	Only F; 4923 and 4933 visible at times.
7 21 45	...	4923 still suspected, and 4956.
7 22 28	...	4956; certainly visible.
7 24 42	...	No line visible.

It will be seen that to some extent these observations lead to the same conclusion as that arrived at by Father Perry.

The corona was examined with a view to the detection of currents, but with a negative result.

PALERMO OBSERVATORY.—The fourth volume of observations made at Palermo has been issued by Prof. Riccò, and covers the period 1884-88. The observations of sun-spots during 1885 show that the limiting latitude in which the phenomena occurred were + 25° and − 30°. Two maxima are indicated by the curve of distribution that has been plotted, both extending from about 10° to 15° north and south of the equator, but the number of spots that have been observed in the latter hemisphere considerably exceeds that observed in the former. The minimum which occurs between these two maxima is in a latitude slightly north of the equator. Generally speaking, faculæ appear to have been equally distributed over the sun's surface. The spectroscopic observations that have been made of solar prominences in different latitudes demonstrate that the reversal of the coronal line 1474K and *b* was considerably more frequent a little to the south of the equator than in any other latitude, and was contained within the limits + 30° to − 30°, following somewhat the same line of distribution as that of spots.

Prof. Riccò has included some fine sunset observations made after the eruption at Krakatão, which support the view that, to a great extent, they were due to the suspension of volcanic dust in the atmosphere. A lengthy series of meteorological measurements, some observation of Nova Orionis, Nova Andromedæ, and various comets, are also contained in this publication.

THE VARIABLE STAR Y CYGNI.—The irregularities before observed in the period of this star have been verified by Mr. Chandler's more recent observations (*Astronomical Journal*, No. 204, October 1889). He finds that the period of the star, which increased by nearly two minutes during 1887 and 1888, is now decreasing at a similarly surprising rate. The reversal appears to have occurred about the middle of 1888, and the average value for the last twelve months has been about 1d. 11h. 56·7m. Assuming this average value for the period of the star, an ephemeris is subjoined. Only alternate minima are given.

Minima of Y Cygni. G.M.T.

	1889.	d. h. m.			1890.	d. h. m.
727	Dec.	2 12 38	...	747	Jan.	1 11 32·0
729	,,	5 12 31·4	...	749	,,	4 11 25·4
731	,,	8 12 24·8	...	751	,,	7 11 18·8
733	,,	11 12 18·2	...	753	,,	10 11 12·2
735	,,	14 12 11·6	...	755	,,	13 11 5·6
737	,,	17 12 5·0	...	757	,,	16 10 59·0
739	,,	20 11 58·4	...	759	,,	19 10 52·4
741	,,	23 11 51·8	...	761	,,	22 10 45·8
743	,,	26 11 45·2				
745	,,	29 11 38·6				

PARAMATTA OBSERVATORY.—The Government Astronomer at this Observatory, Mr. H. C. Russell, F.R.S., has collected and arranged in a concise form the history of what has been done in New South Wales for astronomy and meteorology since 1778. The paper may be found in the Proceedings of the Australasian Association for the Advancement of Science, Sydney, 1888, p. 45.

MINOR PLANET 282.—This planet, discovered by M. Charlois, January 28, 1889, has received the name of Clorinde.

COMET DAVIDSON (*e* 1889).—Ephemeris for Greenwich time:—

1889.		R.A. h. m. s.		Decl. ° ′
Nov. 29·5	...	19 17 21	...	+ 38 56
Dec. 1·5	...	,, 21 41	...	39 10
,, 3·5	...	,, 26 3	...	,, 25
,, 5·5	...	,, 30 25	...	,, 49

A NEW VARIABLE STAR IN HYDRA.—Mr. Edwin F. Sawyer, in the *Astronomical Journal*, No. 204, gives observations demonstrating the variability of the star 358 (U.A.) Hydræ, R.A. 13h. 41m. 59s., Decl. −27° 44′·5 (1875 0). An inspection of the observations that had previously been made of the magnitude of this star indicates fluctuations of about one unit, viz. 7m. to 8m., and the period would appear to be about one year. The star is quite red.

SUN-SPOTS IN HIGH SOUTHERN LATITUDES.—The Rev. S. J. Perry read a paper under this title at the meeting of the Royal Astronomical Society on November 8, in which he drew attention to some remarkable instances which have recently occurred of the appearance of sun-spots at a great distance from the equator. These took place on June 5, June 30, October 8, and October 10 respectively; that of June 30 being especially interesting, as the

spot seen on that occasion attained a latitude of 40°, a circumstance for which there are only a very few recorded precedents. Besides these spots mentioned by Father Perry some much larger groups have also been seen at a less but still considerable distance from the equator. Thus on July 26 and 27 a group was noticed in lat. 24° S., while another and more important group in nearly the same latitude was observed during three successive rotations in August, September, and October. Bearing in mind that the mean distance from the equator of all spots in 1888 was scarcely more than 7°, and in the first five months of 1889, but little more than 5°, these outbreaks in high latitudes become very significant; and taken with the marked increase in number and size of spots during the months of June, July, August, and September, as compared with the earlier part of the year, point to the minimum being definitely passed. If this be so, the period of quiescence has been decidedly shorter, the run down from maximum swifter, and the turn towards recovery sharper than in the preceding cycle. Judging from the form of the spot curve on previous occasions when a short period of minimum has followed a maximum of low intensity, as was that of 1883, we may expect that the revival will be rapid, and the next maximum a strongly marked one.

PROPOSED MEMORIAL OF DR. JOULE.

A PUBLIC meeting was held on Monday in the Mayor's parlour at the Town Hall, Manchester, for the purpose of considering the proposal to erect a memorial of the late Dr. James Prescott Joule. The meeting was convened in response to a memorial influentially signed by residents in Manchester, Salford, and the neighbouring country who desire that the "deep sense of the benefits conferred on mankind for all time, as well as of the great honour which accrues to this district, by the scientific work of the late James Prescott Joule should be marked by the erection of some durable memorial of him in the city." The meeting was very numerously and influentially attended. The Mayor of Manchester presided, and amongst those present were Sir H. E. Roscoe, M.P., Mr. J. W. Maclure, M.P., Dr. Ward (Vice-Chancellor of the Victoria University), Dr. Greenwood (Principal of the Owens College), Prof. Osborne Reynolds, Prof. Munro, Dr. Tatham, Mr. F. J. Faraday, and many others.

A number of letters of apology for absence were read. Lord Derby wrote from London:—

"I cannot attend the meeting on Monday in aid of the Joule memorial, having business here, but I heartily sympathize with the object, and will with pleasure contribute."

Mr. William Mather wrote:—

"When the beautiful simplicity of Dr. Joule's life and character are regarded in conjunction with the world-wide fame his labours have acquired among the greatest intellects of our time, we in Manchester must feel that our late fellow-citizen's memory deserves to be kept ever fresh in our midst by a memorial alike worthy of this city and of the imperishable renown which Dr. Joule has won. Those of us who apply science to industry are deeply indebted for the means through which we work to the original thinkers who put the laws of Nature into our hands with clear definitions as to their purposes. I trust this sense of indebtedness may be felt throughout this district, and that funds may be generously supplied to enable the committee to raise a memorial amply testifying to our gratitude and to our admiration for the late Dr. Joule."

The Bishop of Manchester wrote:—

"I greatly regret that I am prevented by an engagement from attending the meeting in connection with the proposed memorial to Dr. Joule. I think that it would be an honour to any town to be the birthplace and home of the man who first proved the truth of the great principle of the conservation of energy. I most heartily sympathize with the movement which the meeting is called together to initiate, and I shall very gladly give a contribution to any fund which may be to-day established or recommended."

The Mayor, having spoken of the relations between Manchester and science in past times, said the scientific work of Dr. Joule had made the name of Manchester famous throughout the world, not merely as that of a great industrial and trading city, but as a centre of intellectual culture and home of genius. This great man was born in Salford, but he learnt his science as a boy from Dr. Dalton, in George Street in this city. There, he, for a period of nearly half a century, found the congenial society which stimulated his genius. He read many of his papers there; his experiments were performed in this city; and to the end he continued to reside in the suburbs, in a quiet and unostentatious way, his riches truly consisting, not in the extent of his possessions, but in the fewness of his wants. The last generation honoured the memory of Dalton by a statue in marble by Chantrey, which was considered to be one of the most beautiful works of art in the city, and it was suggested that they should show their appreciation of Dalton's great successor in a similar way.

Mr. Oliver Heywood moved:—

"That this meeting desires to mark its deep sense of the benefits conferred on mankind for all time, as well as of the great honour which has accrued to this district, by the scientific work of the late James Prescott Joule, by the erection of a durable memorial of him in Manchester, in the form of a white marble statue."

Sir H. E. Roscoe, M.P., said he felt it a pleasure and an honour in more ways than one to be asked to second the resolution, because, in the first place, he was one of the oldest scientific friends of the man whose memory they had met to honour, and because it had been his privilege not only to become acquainted with his important scientific labours, but to enjoy the friendship of one who might truly be said to have been a typical man of science, the simple straightforward searcher after truth for its own sake and that alone. Another reason was a more personal one. On the occasion of his first public utterance in Manchester, now more than thirty-two years ago, when he read his inaugural address on taking up the duties of the Chair of Chemistry in the Owens College, he drew attention to the great work accomplished by Joule. This was, so far as he could learn, the first occasion on which Joule's work and its importance was brought publicly before a Manchester audience, and he remembered as if it were yesterday being asked by several Manchester friends who this Dr. Joule was of whom he had spoken in such high terms, and what was the great discovery he had made. And then he remembered that, after explaining as well as he could to unscientific people the meaning of the mechanical equivalent of heat and the conservation of energy, he added in joke, in order to impress the matter on minds unaccustomed to deal with subjects scientific, that in the good time coming Manchester would be immortalized, not, as they thought, by being the seat of the cotton trade, but rather as being the place where John Dalton worked out the atomic theory of chemistry, and James Prescott Joule placed upon a sure experimental basis the grand principle of the conservation of energy. Since that time many things had happened, many changes had occurred, and the knowledge of Science and her doings was more widespread. We had acknowledged our indebtedness to Dr. Dalton, and we were now met to consider how we could best do the same for Joule. The memorial which had been presented to the Mayor was of itself proof that Manchester was anxious to recognize merit such as that of Dr. Joule, and to acknowledge that services thus quietly and unostentatiously rendered were sometimes of far greater value to the State than those about which much more was heard. This was not the occasion nor was that the place to enter into an elaborate discussion of Joule's scientific labours. It was sufficient now to remember that, just as Lavoisier, more than a century ago, proved the indestructibility of matter, so Joule nearly half a century ago proved the indestructibility of energy—that we could no more destroy or create energy than we could create or destroy matter. And "thereby hangs a tale"—a tale so interesting that it would take long to tell it; a tale so far-reaching that it concerned every great industry; a tale so important that without it all the modern applications of scientific discovery to the daily wants of mankind could not have been made. The events which formed the incidents in this tale had happened in our midst, and had taken place so quietly that but few had known of their existence. Like many great discoverers, Joule was far in advance of his time; and even the results of his most important research, that on the determination of the mechanical equivalent of heat, met with opposition, and were received with incredulity by men who ought to have known better. Indeed, it was an open secret that when Joule's first paper on this subject, an abstract of which had been read at the Cork meeting of the British Association on August 21, 1843, was presented to the Council of the Royal Society for publication in their Transactions, some of the members of that learned body openly expressed their opinion that the paper was nonsense from beginning to end, that the author, who was a mere amateur, living in some remote and rather uncivilized part of the country, out of the charmed circle of metropolitan and professional science, had been entirely mistaken, because he had, forsooth! neglected the whole question

of friction, and had got hold of an absurd idea that the values of the various so-called imponderables could be expressed in quantitative terms, the one of the other. Fortunately for the credit of the Royal Society, someone more far-seeing than these critics, expressed the opinion that the Council had better take care what it was about, because if they acted on these ideas they might find that they, the highest scientific tribunal in the country, had refused to publish the most important scientific discovery of the century, and one which had already been received with acclamation by all Continental scientific authorities. And so the celebrated paper on the mechanical equivalent of heat was printed, seven years after its first announcement, in the Philosophical Transactions for 1850. But while this, with its immediate relations, was Joule's *magnum opus*, other portions of his work were of scarcely less importance, and to one only of these did he (Sir Henry) wish for a moment to revert, as it touched on a fundamental principle in the science of chemistry, and was therefore specially interesting to himself, whilst it served to show the wide area which Joule's researches covered. On January 24, 1843, Joule read a paper before the Literary and Philosophical Society in their rooms in George Street, hallowed by the memory of Dalton, entitled, "On the Heat evolved during the Electrolysis of Water." The results of this apparently trivial research were of the highest importance, as establishing the heat equivalence of chemical action. Dulong, in France, had already determined the amount of heat evolved during combustion, but he did not compare this with the heat evolved by the same combustion in the battery or elsewhere, and Joule's discovery, described in the above papers, was, that the heat which disappears during separation of the chemical elements was equal to that which made its appearance during their combination, on the principle that action and reaction were equal and opposite. And this was the discovery which established the law proving that chemical action was due to the clashing of the atoms, and that the same laws applied to those atoms singly as they did to them when taken in the aggregate, thus showing that chemistry was a branch of molecular physics. He trusted he had given good grounds for the acceptance by that meeting of the resolution he moved. He would humbly suggest that nothing short of a similar memorial to that erected to Dalton ought to be raised in Manchester in recognition of the labours of Joule. They had statues of Cobden, of Dalton, and of good Bishop Fraser; they would soon have one of Bright. Let them not place Joule in any less conspicuous position, for his work was as glorious as any of theirs. Let us have a marble statue as a companion to that beautiful one of Dalton, by Chantrey, in our Town Hall, and let us have a replica of it in bronze to place on our Infirmary flags, so that all who passed for generations might say, "That is the statue of our great Manchester man of science, James Prescott Joule, who did work in our midst not less important than that of his master, John Dalton, whose statue is hard by; both men were honoured by their contemporaries, and are even more honoured by us who follow them."

Prof. Osborne Reynolds, in supporting the motion, expressed regret that they had not present with them Sir William Thomson, who fought the battle with Dr. Joule. Sir William had written a letter, in the course of which he said: "Manchester is certainly, of all cities in the world, to be envied the honour of being able to erect a monument to Joule as one of its own citizens." Professor Reynolds also made a statement as to the action which had been taken by the Manchester Literary and Philosophical Society, with whom the proposal for a memorial of Dr. Joule originated.

On being put to the meeting, the motion was unanimously adopted.

Mr. Alderman W. H. Bailey moved the appointment of the following Committee to raise, by public subscription, a sufficient sum to carry the above resolution into effect, viz.:—Chairman—the Mayor of Manchester; Treasurer—Oliver Heywood; Thomas Ashton; the Ven. Archdeacon Anson; Sir William Cunliffe Brooks, Bart., M.P.; Alderman W. H. Bailey; Rev. St. Vincent Beechey; C. H. Bayley; Dr. James Bottomley; William Brockbank; J. H. Buxton; Rev. L. C. Casartelli; Councillor George Clay; R. S. Dale; Prof. W. Boyd Dawkins; Mr. Thomas Diggles; Samuel Dixon, President of the Manchester Society of Engineers; F. J. Faraday, Hon. Secretary of the Manchester Literary and Philosophical Society; Lavington E. Fletcher; R. F. Gwyther, Hon. Secretary of the Manchester Literary and Philosophical Society; Samuel Gratrix; Principal J. G. Greenwood; William Grimshaw; Charles J. Galloway; Sir W. H. Houldsworth, Bart., M.P.; T. C. Horsfall; Dr. Charles John Hall; Thomas Harker; Henry H. Howorth, M.P.; William W. Hulse; Henry P. Holt; Isaac Hoyle, M.P.; Dr. Edward Hopkinson; Canon Hicks; James Jardine, High Sheriff of Cheshire; W. H. Johnson; Thomas Kay; George King; Thomas Kay; Horace Lamb; Sir Joseph C. Lee; Ivan Levinstein; J. W. Maclure, M.P.; Councillor J. D. Milne; James Cosmo Melvill; Councillor Alexander M'Dougall, Jun.; Robert Montgomery; Dr. Morgan; William Mather, M.P.; Ludwig Mond (V.P. Chem. Soc.); Prof. J. E. C. Munro; Francis Nicholson; Councillor Charles O'Neill; Henry D. Pochin; W. O. Pooley; Sir H. E. Roscoe, M.P.,; Dr. Ransome; Prof. Osborne Reynolds; Henry Slatter; Dr. Schunck; Prof. Schuster; Councillor Dr. Henry Simpson; Colonel Thomas Sowler; William Thomson; Alderman Joseph Thompson; Councillor S. Chesters-Thompson; E. Leader Williams; Professor A. W. Ward; Thomas Worthington; Rev. Canon Charles W. Woodhouse. Convener of first meeting, Prof. Osborne Reynolds. In his remarks in support of the motion, Mr. Bailey said that speaking as an ex-President of the Manchester Society of Engineers he could testify that, however slow many people might have been to acknowledge Dr. Joule's work, the Society of Engineers had never forgotten Dr. Joule's labours and the benefit which those labours had conferred on the engineers of this country and on the industries of the world generally.

The motion was seconded by Colonel T. Sowler and unanimously adopted.

A vote of thanks to the Mayor for presiding and for the use of his parlour, accorded on the motion of Prof. Ward, seconded by Mr. C. Bailey, brought the proceedings to a close.

HOW PLANTS MAINTAIN THEMSELVES IN THE STRUGGLE FOR EXISTENCE.[1]

ORDINARY English scenery, so full of quiet and so suggestive of repose that one may not readily discover signs of a struggle for existence. In tropical scenery these signs are so clear that they have been recognized again and again by every thinking naturalist who has ever visited tropical regions.

Any comprehensive view of the phenomenon of life upon the globe clearly points to the one conclusion that all Nature is in a perpetual state of desperate warfare, and the keynote of this address must be: the utter remorselessness of Nature, the care for self; the absolute disregard for others. In all cases the weakest goes to the wall.

Evidences of Struggle for Existence in the Plant World.

Ficus parasitica. Seed dropped by bird germinates on fork of some tree, *e.g.* the jack fruit (*Artocarpus integrifolia*); sends long root into soil; gradually spreads itself over, and suffocates the unfortunate foster-mother.

Heracleum giganteum. Allowed to seed itself freely. On June 1, 1889, 573 seedlings had germinated; on August 19, 105 remained, the missing ones having been killed by the more vigorous survivors.

Bertholletia excelsa. Fifteen to twenty-four Brazil nuts are contained in each fruit, the fruit being indehiscent. All seeds germinate at once. The most vigorous gets first through a small hole at the top to the open air, and strangles and feeds upon all the rest.

What Plants struggle for.

Plants struggle for two main objects—viz. their own nutrition, and the reproduction of their species by means of offspring, which they leave behind them, and for which they make adequate provision. The two master functions, nutrition and reproduction, often stand out clearly marked the one from the other—*e.g.* in the Talipot palm (*Corypha umbraculifera*), where the period of leaf-bearing is succeeded by the period of fruiting, the latter being accompanied by the final death of the whole plant.

I.—NUTRITION.

Protective Adaptations associated with the mainly Nutritive Organs.

(1) *Mechanical contrivances.* Large forest trees (often 200 feet high) have buttressed trunks, *e.g.* *Canarium commune.*

[1] Abstract furnished by the Author, Prof. Walter Gardiner, of a lecture delivered at the Newcastle meeting of the British Association.

(2) Large leaves in palms (often 14 feet long), tied in at the leaf-base, *e.g. Didymospermum distichum.*

(3) Young buds of many tropical trees hang vertically downwards, so as to expose the least surface to sun, *e.g. Amherstia nobilis.*

(4) *Prickles and spines developed, e.g.* immense leaf of *Victoria regia* is protected from fish, &c., which, in rising from below, might rupture the leaf-tissue.

(5) *Patrols of ants attracted.* Ants provided with home, honey, and food, *e.g. Acacia sphærocephala.* Similarly, *Ipomœa paniculata* attracts ants by racemose glands supplied with definite ducts, two of which are present in each leaf, at junction of blade and stalk.

(6) During the unfolding and growth of the bud, special mechanisms exist. Thus, water-glands occur at the apex of each leaf-tooth (*Saxifraga crustata*), which provide for the escape of the superabundant water sucked up by the root: otherwise the delicate leaf-tissue might be ruptured. In fully developed leaves, on a cold night, drops may be seen escaping from the teeth, *e.g.* balsam (*Impatiens Balsamina*).

Other glands are also found which secrete mucilage or resin, and so protect the young structures from the effects of excessive drought, *e.g.* ferns (*Blechnum Braziliense*) and other plants (*Clusia sp.* and *Coprosma sp.*).

II.—Reproduction.

The importance of this process is sufficiently obvious from the enormous expenditure of material and energy plants lavish upon it. *Hodgsonia heteroclita*, an extraordinary Indian climber, with its complicated structure and great beauty, opens for one night only, and shrivels up and falls off the next day. *Amorphophallus Titanum*, with its huge inflorescence (the largest in the world), although it takes months to develop, opens only on one night, and then only for a few hours.

α.—*Flowers.*

(1) *Contrivances to insure fertilization. Masdevallia muscosa* (an orchid) has a sensitive labellum. An insect alighting on it and touching a certain part, is shot into the flower and held a prisoner for some time.

(2) *Protection by means of sticky hairs. Cuphea silenoides* is protected from the attacks of insects by very numerous hairs secreting a gum resin. Many insects are caught, and as many as 7280 may be counted on one plant.

(3) *Plant protected by ants, but flower fertilized by some other insect. Plumbago rosea* has nectaries on the leaves and flower-bracts which attract ants, but the ants are prevented by sticky hairs on the calyx from obtaining access to the honey in the flower.

β.—*Seeds and Fruits.*

Some plants depend upon the enormous quantity of seeds produced—*e.g.* the wild carrot (*Daucus carota*), which, moreover, sows its seeds by instalments and at different times. Others—*e.g. Voandzeia subterranea*—sacrifice the advantages obtained from a wide dispersal, and depend upon the formation of a few seeds suitably placed in the soil. This plant, in fact, has a mechanism for itself, sowing its own seeds beneath the soil.

For purposes of distribution, *Uncinia brevicaulis* (a sedge) has its fruit provided with small hooks. Small birds, unable to pull out the fruits, are occasionally caught and killed in Jamaica. The fruits of *Stipa pennata*, a grass, bore their way into the ground; and another species, *Stipa spartea*, is even liable to bore its way into the bodies of sheep which are so unfortunate as to come in its neighbourhood (prairies west of Red River Colony).

Contrivances for assisting plants to maintain themselves in the struggle for existence are by no means limited to the higher plants. They exist also in the Fungi and the Algæ, even in the smallest and most microscopic of them. Examples—

I. *Fungi.*—*Clathrus triscapus*, a Queensland fungus, has an orange-red colour, and the spores smell strongly and are embedded in a sweet mucilage. Colour, scent, and sweetness are the usual advertisements used by the higher plants in connection with pollen dispersion.

Erysiphe Alni. The mildew of the alder has wonderfully hooked fruits, which are possibly carried about by tiny *Acari*, &c. Spores are shot out with some force from the mycelial filaments of the fungus, which attacks and kills flies, *Empusa muscæ.* The ergot *Claviceps purpurea*, at the time of spore-formation, secretes a sugary nectar, so that flies are attracted, and eat and disseminate the spores, just as birds do stone fruits. The spores of *Sclerotina Vaccinii* have an almond smell; are gathered by bees with the pollen, and, being placed on the stigma of healthy flowers, infect the ovary and prevent the formation of seed. In the race between the pollen-grain tube (the rightful owner) and the fungus-spore mycelial-tube, the fungus always wins, and soon spreads itself throughout the tissue of the entire ovary, producing more spores for the bees to gather in mistake again.

II. *Algæ.*—The resting-spores of *Desmids*—microscopically small green Algæ—are frequently covered by a spiny siliceous coat. These probably prevent them from being eaten by *Amœbæ*, *Rhizopods*, &c. The protoplasm of certain cells of *Œdogonium ciliatum* (a fresh-water filamentous Alga) are in the habit of escaping from the cell-wall and beginning life anew. This production of the so-called *swarm-spore* is probably not wholly unconnected with the existence of unfavourable conditions, *e.g. Bacteria* on the cell-wall, deposits of lime on the cell-wall, &c.

Mesocarpus sp., another filamentous Alga, carefully protects its chlorophyll plate from too bright light by turning it so that it shall receive the proper amount only. Should external conditions be exceptionally unfavourable, the protoplasm of the various cells powerfully contracts, and the filament resolves itself into its various constituent units, which sink to the bottom of the river or pond, and there divide up and start afresh.

Special Points worthy of notice.

(1) *Various adaptations by members of the same order, e.g.* the *Cucurbitaceæ* (Cucumber family), in the matter of seed distribution.

In *Schizocarpum filiforme* the seeds escape through a number of slits in the wall of the fruit.

In *Ecbalium elatine* the seeds are violently and explosively shot out in consequence of the sudden rupture of the fruit stalk.

Sechium edule is indehiscent and contains only one seed.

Zanonia macrocarpa dehisces at the apex by means of valves, and lets out winged seeds of extraordinary beauty, which, aided by the wind, can cover very appreciable distances.

(2) *Various adaptations by members of the same genus, e.g.* the *Clerodendrons.*

Clerodendron Koemferi attracts ants by small glands on the leaf and calyx.

Clerodendron fistulosum does the same, but also provides a home for the ants in its hollow stem.

Clerodendron cephalanthum climbs by means of peculiarly modified leaf stalks; has a multiplicity of buds on the axil of each leaf (instead of the usual *one*) and also possesses glands upon its leaves.

Such families as this may well be regarded as accomplished, but at the same time their various contrivances are simply so many marks of a cruel and fierce fight.

(3) *Protective contrivances associated with new annual growth and germination.*

Dioscorea, sp. nov., at each new period of growth produces at first inconspicuous shoots with small leaves which are peculiarly modified into climbing organs. When well established and in the possession of a proper support large green leaves appear.

Hodgsonia heteroclita.—Here again the shoot on its first appearance is dark purple and inconspicuous, with the leaves present merely as scales. It can then scarcely be seen in the tropical forest. Moreover it is a lateral shoot and not the main terminal shoot which it first protrudes above ground. A second lateral and the main terminal are held in reserve against possible accident. When it has reached a certain height, it produces the normal large leaves.

(4) *The accumulation of protective contrivances in the same individual.*

Blumenbachia Hieronymi.—The flower is at first upright and is fertilized in that position. As the fruit develops, the flower-stalk elongates and the fruit is gradually and gently placed upon the ground. Until quite ripe, it is protected by stinging hairs. Later on, these wither, and the fruit is distributed by means of a second series of grapple hairs, which cling firmly to any passing animal.

Strophanthus hispidus.—Fruit, when ripe, opens, and lets out a number of magnificent plumed seeds, which are carried by the wind. The hairs forming the plume are sensitive to moisture and dryness, and are each capable of moving through an arc of 180°. The hairs spread out in dry weather, so that the seed

may be carried by the wind. They close up tightly when the rains come, so that they may not interfere with the placing of the seed close to the ground and its consequent germination. Sooner or later they break from the seed.

(5) *Particular adaptations contrived for particular classes of insects, &c.*

Ants are caught and killed at Kew by flowers of *Eria stricta* (an orchid). The ants are too large for the flower, but they visit it for the sake of the honey and get caught in the mucilage. Thus both flower and ant suffer.

(6) *The mutual adaptation of plants and animals.*

In some instances animals and plants appear to strive with each other, and, as the one develops a particular protective contrivance, the other likewise adopts some plan to counteract it and annul its efficiency: thus the canari nut (the fruit of *Canarium commune*) develops a hard shell which protects it from most enemies, but the black cockatoo (*Microglossus aterrimus*) reciprocates by developing a wonderfully strong beak, which appears indeed to be developed with a special view to the canari nut. Insects also often imitate parts of plants for their own benefit, *e.g.* leaf insects.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—The Senate has formally thanked Prof. Sedgwick for his munificent gift towards the new buildings for physiology, and the Museums and Lecture Rooms Syndicate has been authorized to contract for the buildings to be immediately begun.

The following stipends have been augmented: Dr. Gaskell, F.R.S., University Lecturer in Physiology, from £50 to £150; Mr. Gardiner, University Lecturer in Botany, from £50 to £100.

The Special Board for Biology and Geology, recommend the appointment of an additional University lecturer on botany, at a stipend of £100 per annum, after considering a strong appeal for increased teaching power, from the professor and lecturers in the subject. No teacher had practically been added since the departure of Prof. Vines for Oxford, and the regretted death of Mr. Vaizey.

Mr. W. Bateson, the Balfour Student, will give a course of lectures during Lent term, on the study of variation—a distinct and attractive novelty in the biological courses.

SCIENTIFIC SERIALS.

American Journal of Science, November.—This number opens with an interesting address by Mr. R. S. Woodward at the last meeting of the American Association, on the mathematical theories of the earth, in which emphasis is laid on the incompleteness of those hitherto advanced.—From a simple investigation, Mr. R. Hooke concludes that for planetary bodies assumed to have the same surface density (*i.e.* those in which solidification has taken place), the increase of the difference between the mean and surface density is proportional to the increase of the diameter. He tests this by computation of the mean densities of the inner planets from their assigned diameters, and further confirmation is derived from the case of Jupiter's satellites. He also applies the law to computing the ultimate diameters and mean densities (*i.e.* after solidification) of the sun and outer planets.—Regarding Tschermak's theory of the mica group as inadequate, Mr. F. W. Clarke offers the view that all the micas, vermiculites, chlorites, margarite, and the clintonite group, may be simply represented as isomorphous mixtures, every constituent being a substitution derivative of normal aluminium poly- or ortho-silicate.—Mr. E. O. Hovey studies the low trap ridges (some six lines of them) of the East Haven-Branford region in Connecticut; he considers all the trap intrusive, and the western dikes, at least, of later origin than the tilting of the sandstone.—Mr. C. Lea contends that subchloride, and not oxychloride, is the product of the action of light on silver chloride.—There are also papers on an improved standard Clark cell with low temperature coefficient, by Mr. H. S. Carhart; on pseudomorphs of native copper after azurite, from Grant County, New Mexico, by Mr. W. S. Yeates; and on the relation of volume, pressure, and temperature, in case of liquids, by Mr. C. Barus.

THE *American Meteorological Journal* for October contains:—A reprint of Prof. C. Abbe's paper on the determination of the amount of rainfall, read before the recent meeting of the British Association; the object of the paper is to determine the possible errors arising from the different shapes of the rain-gauges, and their height above the sea-level and the ground, &c.—Tornado statistics, by Lieut. Finley: (*a*) for the State of Louisiana, for the thirty-seven years 1852-88,—the total number of storms was only thirty, the month of greatest frequency being April; (*b*) for Texas, for the thirty years 1850-88,—the total number of storms was ninety-six, the month of greatest frequency being June.—Distribution of wind velocities in the United States, by Dr. F. Waldo. In the Eastern States there is a principal maximum and minimum in March and August respectively, with a secondary maximum in autumn, and a winter maximum. The same regularity which exists in the Eastern States does not occur in the other districts, but the region of the Lower Lakes has a little more wind in winter and a little less in summer than the region of the Upper Lakes. He also investigates the secular variation at selected stations, and finds that a period of about nine years is not improbable.—An analysis of a paper, by Dr. H. B. Baker, Secretary of the Michigan Board of Health, on the connection of intermittent fever with atmospheric temperature. For some years that Board has made a special feature of the collection of vital statistics, and publishes valuable reports on sanitary matters in general.

THE *Botanical Gazette* continues to publish valuable original contributions to botanical science, especially in the department of cryptogamy. The August number contains the first of a series of Prof. Farlow's notes on Fungi, and the September number an illustrated paper on the Uredo-stage of *Gymnosporangium*, by Mr. H. M. Richards.—Mr. H. L. Russell also contributes observations on the temperature of trees, illustrated by a diagram; his general conclusion being that the direct absorption of heat is the main cause of the higher temperature of trees, and that it is largely dependent on the character of the bark.

A LARGE proportion of the *Journal of Botany* for August, September, and October, is occupied by the conclusion of Mr. G. Murray's Catalogue of the marine Algæ of the West Indian region, and the continuation of Messrs. Britten and Boulger's Biographical Index of British and Irish botanists.—Mr. W. West's paper on the fresh-water Algæ of North Yorkshire is a valuable contribution to a department of botany in which there are but few workers; it is illustrated by a good plate, and contains descriptions of several new species.—Mr. W. H. Beeby contributes a useful account of some of the difficult and critical British forms of *Viola*.—There are other papers of interest, especially to students of British botany.

THE number of the *Nuovo Giornale Botanico Italiano* of October is entirely occupied by papers read at the meetings of the Italian Botanical Society. They are chiefly devoted to records of local floras, and to descriptions of remarkable teratological forms.—Signor U. Martelli contributes a note on the injury inflicted on the peach by *Taphrina deformans*.

Bulletin de la Société Impériale des Naturalistes de Moscou, 1889, No. 1.—On the origin of the shooting-stars, by Th. Bredichin (in French), being an application of the author's theory of the *comes anomales* to the origin of shooting-stars. The paper will be continued by another on the origin of periodical comets.—On the Jurassic and Cretaceous deposits in Russia; Part 1, on the Upper Jurassic and Lower Cretaceous deposits in Russia and Great Britain, by Prof. A. Pavloff (in French, with three plates). The author's conclusions are to the effect that the Upper Jurassic deposits of Russia are so intimately connected with those of England that a common classification could easily be established. Several fossil species are described and figured on plates, three of them being new (*Olcostephanus blaki*, *O. swindonensis*, and *O. stenomphalus*).—Zoological exploration in the Transcaspian region, by N. Zaroudnoi (in French), being notes of travel, full of interesting information about the nature and fauna of the country.—On a natural way of penetration of superficial water into the depths of the earth, by Stanislas Meunier (in French).—On the *Sparganiæ* of Russia, by K. F. Meinshausen (in German). Ten species are described, two of them (*Sp. ratis* and *Sp. septentrionale*) being new.

SOCIETIES AND ACADEMIES.

LONDON.

Entomological Society, November 6.—Prof. J. O. Westwood in the chair.—Mr. J. W. Douglas sent for exhibition specimens of *Anthocoris visci*, Dougl., a new species taken at Hereford, in September last, by Dr. T. A. Chapman; also specimens of *Psylla visci*, Curtis, taken by Dr. Chapman at the same time and place.—Mr. R. McLachlan, F.R.S., exhibited coloured drawings of a specimen of *Zygæna filipendulæ*, in which the left posterior leg is replaced by a fully-developed wing, similar to an ordinary hind wing, but less densely clothed with scales. Mr. McLachlan also exhibited a female specimen of the common earwig, *Forficula auricularia*, with a parasitic Gordius emerging from between the metathorax and abdomen. He said that it had been placed in his hands by Mr. A. B. Farn, by whom it was taken, and that other instances of similar parasitism by Gordius on earwigs had been recorded.—Mr. W. F. Kirby exhibited a gynandromorphous specimen of *Lycæna icarus*, having the characters of a male in the right wings and of a female in the left wings, caught at Keyingham, Yorkshire, in June last; also a specimen of a variety of *Crabro interruptus*, De Geer, found at Uxbridge.—Mr. W. L. Distant exhibited a male and female specimen of a species belonging to a new genus of *Discocephalinæ*, from Guatemala, in which the sexes were totally dissimilar, the female having abbreviated membranes, and being altogether larger than the male.—Dr. D. Sharp stated that he had observed that in the *Ipsinæ* division of *Nitidulidæ* there was present a stridulating organ in a position in which he had not noticed it in any other Coleoptera—viz. on the summit of the back of the head. He had found it to exist not only in the species of *Ips* and *Cryptarcha*, but also in other genera of the subfamily. He exhibited specimens of *Ips* and *Cryptarcha*, mounted to show the organ. Dr. Sharp also exhibited a number of *Rhynchota*, chiefly *Pentatomidæ*, in which the specimens were prepared so as to display the peculiarities of the terminal segment in the male sex.—Mr. R. Adkin exhibited for Mr. H. Murray, a fine series of *Polia xanthomista*, var. *nigrocincta*, from the Isle of Man, and *Cidaria reticulata* and *Emmelesia tæniata* from the Lake District.—Mr. W. White exhibited a living larva of *Zeuzera æsculi*, and called attention to the thoracic segments with several rows of minute serrations, which evidently assist progression. He stated that the larva exudes from its mouth, when irritated, a colourless fluid, which he had tested with litmus-paper and found to be strongly alkaline.—Captain H. J. Elwes exhibited a number of insects of various orders, part of the collection formed by the late Otto Möller, of Darjeeling.—Mons. A. Wailly exhibited the cocoon of an unknown species of *Antheræa* from Assam; also a number of cocoons and imagos of *Anophe venata* from Acugua, near the Gold Coast; specimens of *Lasiocampa otus*, a South European species, which was said to have been utilized by the Romans in the manufacture of silk; also a quantity of eggs of *Epeira madagascariensis*, a silk-producing spider from Madagascar, locally known by the name of "Halabe." He also read extracts from letters received from the Rev. P. Camboué, of Tananarivo, Madagascar, on the subject of this silk-producing spider.—Mr. H. Goss read a communication from Dr. S. H. Scudder, of Cambridge, Mass., U.S.A., on the subject of his recent discoveries of some thousands of fossil insects, chiefly Coleoptera, in Florissant, Western Colorado, and Wyoming. Prof. Westwood remarked on the extreme rarity of fossil Lepidoptera, and called attention to a recent paper by Mr. A. G. Butler, in the Proc. Zool. Soc., 1889, in which the author described a new genus of fossil moths belonging to the family *Euschemidæ*, from a specimen obtained at Gurnet Bay, Isle of Wight.—Mr. F. P. Pascoe read a paper entitled "Additional Notes on the genus *Hilipus*," and exhibited a number of new species belonging to that genus.—The Rev. Dr. Walker read a paper entitled "Notes on the Entomology of Iceland." Mr. R. Trimen, F.R.S., asked if any butterflies had been found in the island. Dr. Walker said that neither he nor Dr. P. B. Mason had seen any during their recent visit, nor were any species given in Dr. Staudinger's list. Dr. Mason said that during his recent visit to Iceland he had collected nearly one hundred species of insects, including about twenty Coleoptera. He added that several of the species had not been recorded either by Dr. Staudinger or Dr. Walker. Capt. Elwes inquired if Mr. J. J. Walker, with his great experience as a collector in all parts of the world, was aware of any land outside the Arctic Circle from which no butterflies had been recorded. Mr. J. J. Walker replied that the only place in the world which he had visited, in which butterflies were entirely absent was Pitcairn Island.

Royal Microscopical Society, October 9.—Dr. C. T. Hudson, F.R.S., President, in the chair.—The President referred to the deaths of the Rev. M. J. Berkeley and Dr. G. W. Royston-Pigott, the former an honorary, and the latter formerly an ordinary, Fellow of the Society.—Mr. Crisp announced that, owing to certain business arrangements, he was obliged to retire from the secretaryship of the Society and from the conduct of the Journal. It was with the very greatest reluctance that he had found it necessary to resign, but there would, he anticipated, be no difficulty in continuing the Journal on its present lines, while he was sure there were many Fellows both able and willing to undertake the duties of Microscopical Secretary.—Mr. John Meade's communication on stereoscopic photo-micrography was read.—The President brought for inspection three photo-micrographs of one of the new rotifers mentioned in his supplement—*Gomphogaster areolatus*.—Mr. E. M. Nelson exhibited a new elementary centering sub-stage which he thought was likely to be useful. It was fitted in the simplest manner by placing two legs under the main stage, and the movement was given to it with the finger; it was very inexpensive, and was only designed to render the ordinary student's microscope of a higher degree of efficiency by providing it with an easy method of correctly centering the condenser and diaphragm.—The President mentioned that *Pedalion* was to be had in many places in the neighbourhood of London about a month ago, where it had not been previously found.—Mr. Ahrens's description was read of his new patent polarizing binocular microscope for obviating the difficulty of using analyzing prisms with the double tube. The inventor uses for an analyzer a black glass prism, set above the objective with a horizontal side upwards. Two faces are symmetrically inclined to the optical axis at the polarizing angle. The pencil is thus reflected at the proper angle, and at the same time divided into two parts, which are then reflected up the two tubes either by prisms or by plane reflectors.—Prof. Abbe's paper, notes on the effect of illumination by means of wide-angled cones of light, was read.—Mr. T. F. Smith read a paper on the ultimate structure of the *Pleurosigma* valve.

Royal Meteorological Society, November 20.—Dr. W. Marcet, F.R.S., President, in the chair.—The following papers were read:—Second Report of the Thunderstorm Committee. This is a discussion by Mr. Marriott on the distribution of days of thunderstorms over England and Wales during the seventeen years 1871–87. Notices of sheet lightning are included in the term "thunderstorms." The years of greatest frequency were 1880, 1882, 1884, and 1872; and the years of least frequency 1887, 1874, 1879, and 1871. Years of greater or less frequency alternate regularly throughout nearly the whole of the period. The average yearly number of thunderstorms is about thirty-nine. The districts with the greatest yearly frequency are the south of England and extreme northern counties, and those with the least yearly frequency are Cheshire, Lancashire, and Yorkshire. The greatest number of thunderstorms occur in July, and the least in February and December.—On the change of temperature which accompanies thunderstorms in Southern England, by Mr. G. M. Whipple.—Note on the appearance of St. Elmo's fire at Walton-on-the-Naze, September 3, 1889, by Mr. W. H. Dines.—Notes on cirrus formation, by Mr. H. Helm Clayton. The author, who has made a special study of cloud forms and their changes, gives a number of notes and drawings on the formation of cirrus under various conditions, *e.g.* in a previously cloudless sky, cirrus bands with cross fibres, cirrus from cirro-cumulus clouds, cirrus drawn out from cumulus clouds, "mares-tail" cirrus, &c. Curved cirrus clouds when accompanied by decreasing barometric pressure frequently indicate that a storm of increasing energy is approaching.—A comparison between the Jordan and the Campbell-Stokes sunshine recorder, by Mr. F. C. Bayard. As a result of a year's comparison between these two instruments, the author found that the Jordan photographic recorder registered nearly 30 per cent. more sunshine than the Campbell burning recorder.—Sunshine, by Mr. A. B. MacDowall. This is a discussion of the hours of sunshine recorded at the stations of the Royal Meteorological Society.—On climatological observations at Ballyboley, co. Antrim, by Prof. S. A. Hill. This is the result of observations made during the five years 1884–88.

Geological Society, November 6.—W. T. Blanford, F.R.S., President, in the chair.—The following communications were read:—Contributions to our knowledge of the Dinosaurs of the Wealden and the Sauropterygians of the Purbeck and Oxford Clay, by R. Lydekker. The first section of this paper was devoted to the description of the remains of Iguanodonts from the Wadhurst Clay near Hastings collected by Mr. C. Dawson. They were considered to indicate two species, for which the names *Iguanodon hollingtoniensis* and *I. Fittoni* had been proposed in a preliminary notice. In the second section an imperfect metatarsus of a species of *Megalosaurus* from the Hastings Wealden was described, and shown to indicate a species quite distinct from the one to which a metatarsus from the Wealden of Cuckfield belonged. Two cervical vertebræ of a Sauropterygian from the Purbeck of the Isle of Portland were next described, and referred to *Cimoliosaurus portlandicus*, Owen, sp. The concluding section described an imperfect skeleton of a large Pliosaur from the Oxford Clay, in the collection of Mr. A. N. Leeds, which indicated a species intermediate between the typical Kimeridgian forms and the genus *Peloneustes*. These specimens were considered as probably referable to *Pliosaurus ferox*. Evidence was adduced to show that *Pliosaurus Evansi*, Seeley, should be transferred to *Peloneustes*.—Notes on a "dumb fault" or "wash-out" found in the Pleasley and Teversall Collieries, Derbyshire, by J. C. B. Hendy; communicated by the President.—On some Palæozoic Ostracoda from North America, Wales, and Ireland, by Prof. T. Rupert Jones, F.R.S. The specimens were described as nearly as possible in the order of their natural relationship, and thus, besides adding to the known forms, they were shown to illustrate the modifications exhibited by the genera and species of these minute bivalved Crustaceans, both in limited districts and in different regions. Amongst the forms described were the following new species and variety:—*Primitia mundula*, Jones, var. *cambrica*, nov.; *P. humilior*, sp. nov.; *P. Morgani*, sp. nov.; *P. Ulrichi*, sp. nov.; *P. Whitfieldi*, sp. nov.; *Entomis rhomboidea*, sp. nov.; *Strepula sigmoidalis*, sp. nov.; *Beyrichia Hallii*, sp. nov.; *Isochilina lineata*, sp. nov.; *I.? fabacea*, sp. nov.; *Leperditia Claypolei*, sp. nov.; *Xestoleberis Wrightii*, sp. nov.

Zoological Society, November 5.—Prof. W. H. Flower, F.R.S., President, in the chair.—The Secretary read a Report on the additions that had been made to the Society's Menagerie during the months of June, July, August, and September, 1889, and called attention to certain interesting accessions which had been received during that period. Amongst these were specially noted a Short Python (*Python curtus*), from Malacca, presented on July 2 by Mrs. Bertha M. L. Bonsor; and a Prêtre's Amazon (*Chrysotis prætrii*), purchased July 23: both new to the collection.—Mr. J. H. Gurney, Jun., exhibited and made remarks on a hybrid Wagtail, bred in confinement, between the Grey Wagtail (*Motacilla melanope*) and the Pied Wagtail (*M. lugubris*).—Mr. W B. Tegetmeier exhibited and made remarks on some variations in the plumage of the Partridge (*Perdix cinerea*).—Prof. Bell exhibited and made remarks on two specimens of *Virgularia mirabilis*, recently dredged by the Hon. A. E. Gathorne Hardy, M.P., in Loch Craignish. He also exhibited two young living specimens of *Palinurus vulgaris*, received from Mr. Spencer, of Guernsey, in which the stridulating-organs were still capable of making sounds.—A communication was read from the Rev. Thomas R. R. Stebbing, containing an account of the Amphipodous Crustaceans of the genus *Urothoë*, and of a new allied genus proposed to be called *Urothoides*.—A communication was read from Colonel C. Swinhoe, containing descriptions of a large number of new Indian Lepidoptera, chiefly Heterocera.—Mr. P. L. Sclater gave an account of the birds collected by Mr. Ramage in St. Lucia, West Indies, which were referred to thirty species.—Mr. G. A. Boulenger read a note on the Short Python (*Python curtus*), a specimen of which was stated to be living in the Society's reptile house.—A communication was read from Dr. E. C. Stirling, of the University of Adelaide, on some points in the anatomy of the female organs of generation of the Kangaroo, especially in relation to the acts of impregnation and parturition.—Mr. F. E. Beddard read some notes on the anatomy of an Oligochætous Worm of the genus *Dero*, relating principally to its reproductive system.—A communication was read from Mr. Scott B. Wilson, in which were given the descriptions of four new species of Hawaiian birds, proposed to be called *Chrysometridops cæruleirostris, Loxops flammea, Himatione montana*, and *H. stejnegeri*.

Mathematical Society, November 14.—Sir J. Cockle, F.R.S., Vice-President, in the chair.—The following gentlemen were elected to form the Council for the ensuing session:—President: J. J. Walker, F.R.S. Vice-Presidents: Sir J. Cockle, F.R.S., E. B. Elliott, and Prof. Greenhill, F.R.S. Treasurer: A. B. Kempe, F.R.S. Honorary Secretaries: M. Jenkins and R. Tucker. Other members: A. B. Basset, F.R.S., Prof. W. Burnside, Prof. Cayley, F.R.S., Dr. Glaisher, F.R.S., J. Hammond, Dr. Larmor, C. Leudesdorf, Major Macmahon, R.A., and S. Roberts, F.R.S.—The following papers were read:—Isoscelian hexagrams, by Mr. R. Tucker.—On Euler's ϕ-function, two notes by Mr. H. F. Baker and Major Macmahon (the former communicated by Mr. Jenkins).—On the extension and flexure of a thin elastic plane plate, by Mr. A. B. Basset, F.R.S.

PARIS.

Academy of Sciences, November 18.—M. Hermite in the chair.—On animal heat and the heats of formation and of combustion of urea, by MM. Berthelot and P. Petit. In connection with the production of animal heat the study of urea is of special interest, for next to carbon dioxide it is the chief form under which carbon is eliminated from the system, while almost all the nitrogen is eliminated as urea. Hence it is important to know how the production of urea in the organs is related to the heat of formation of urea, and of the substances from which it is derived. In the present paper the authors deal with the first problem, for the heat of combustion of urea in free oxygen has not yet been measured. Three concordant combustions in the calorimetric bomb yielded 151·8 C. per gram-molecule, and the molecular heat of solution of urea at about 11° C. is found to be − 3·58 C., whence the heat of formation of urea is 80·8 C., and of its solution in water or urine is found to be + 77·2 C.—On the orbit of Winnecke's periodical comet, by M. H. Faye. These remarks are made in connection with a memoir presented to the Academy by Baron von Haerdtl, on the movements of Winnecke's periodical comet. He arrives at the conclusion that there is no trace of acceleration in the mean movement. He finds that the mass of Jupiter must be raised to 1 : 1047·152, and determines that of Mercury in round numbers at 1 : 5,010,000 ± 700,000. This agrees pretty closely with the value 1 : 5,310,000 already obtained by Le Verrier.—Experimental study of the transits and occultations of Jupiter's satellites, by M. Ch. André. These observations have been made by means of an apparatus specially constructed by MM. Brunner, and here fully described. Particular attention was paid to the phenomenon of the luminous ligament which is formed near the point of contact. It begins to appear when the satellite is about 2½ minutes from real contact, gradually increasing in size and intensity as the two bodies draw near, so that at the instant of geometrical contact they appear to be connected by a veritable luminous bridge about one-third the breadth of the diameter of the satellite. The moment of geometrical contact is accompanied by optical appearances sufficiently distinct to serve as a base for the direct observation of the phenomenon.—Researches on the application of the measurement of rotatory power to the study of compounds resulting from the action of malic acid on sodium molybdate, by M. D. Gernez. In a previous communication (*Comptes rendus*, cix. p. 151) the author showed that solutions of malic acid, with molybdate of ammonia added, show sundry changes in rotatory power, which may easily be explained by assuming that definite compounds are formed between the substances. His present researches, made with the same acid and neutral sodium molybdate, lead to still more varied results, clearly showing the production of compounds between simple numbers of molecules of these bodies. The results, which are here tabulated and described, demonstrate that definite compounds are formed in solution on increasing the amount of one of the compounds regularly. They also show the defect of analytical methods claiming to deduce the composition of an active liquid from the measurement of its rotation, at least so far as regards substances analogous to those here under consideration.—On the ophthalmoscopic examination of the base of the eye in hypnotic subjects, by MM. Luys and Bacchi. Nine subjects (six women and three men) were examined, first in the normal state and then in various phases of catalepsy, lucid somnambulism, and hallucination. In some instances the iris was found to be excessively dilated and almost insensible to light. Other appearances are described, but no general inferences are drawn from these preliminary observations.—The second part of vol. i. of MM. Houzeau and Lancaster's "Bib-

liothèque générale de l'Astronomie," was presented by M. Faye, who remarked that this great compilation would not be interrupted by the death of M. Houzeau. The present volume comprises biographies, didactic and general works, spherical and theoretical astronomy, astronomical tables for all epochs, and treatises on calendars.

BERLIN.

Physical Society, October 25.—The President, Prof. Kundt, opened the meeting by a warm expression of regret at the loss sustained by the Society in the death of its late member, Dr. Robert von Helmholtz.—Prof. von Bezold spoke on the various causes which lead to the production of clouds and aqueous precipitates. Using the graphic methods which he had himself introduced into meteorology, he showed by means of diagrams that the older ideas on this subject are insufficient, and that, even in the case where both masses of air are saturated with aqueous vapour, the precipitation which may occur when they are mixed is not due to the mere mixing of warm and cold air: the temperature of the mixture is not the mean of that of the respective masses of air, but is somewhat higher, and the amount of water which is condensed on their mixing is very small. By means of his diagrams a simple solution is at once obtained of many problems which have reference to the temperature and humidity of masses of air when they are mixed together in unequal quantities. It appeared that under the most favourable conditions, when air saturated with aqueous vapour at 0° C. is mixed with air saturated at 20° C., under a pressure of 700 millimetres of mercury, only 0·6 grams of water is condensed out of 2 kilograms of the mixed portions of air. The same mass of water would be condensed out of the same mass of air saturated at 20° C. if its temperature were reduced to 19°·3 C., or if the air were to ascend through a height of 200 metres, in which case its temperature would fall to 18°·9 C. Much more massive aqueous precipitates are formed when moist air is either cooled directly, or has its pressure reduced by rising upwards, in which case a simultaneous cooling occurs. When air saturated at 20° C. is cooled down to 10°·7 C.,—a temperature which results from mixing air at 24° C. with air at 0° C.,—4·4 grams of water are precipitated out of each kilogram of air, and if the temperature is reduced to 0° C., 8 grams are precipitated. Similar falls of temperature may be obtained during an adiabatic rise in altitude. The conditions which hold good for super-saturated air may similarly be comprehended by this graphic method. Notwithstanding that the formation of aqueous precipitates by the mere mixing of two masses of air is thus shown to be very minimal in amount, still it does occur in nature as the result of this cause, as, for instance, in the case of cloud-caps formed when different winds meet, and in the case of the formation of ground-fogs. According to the speaker, clouds ought to be distinguished by reference to the way in which the precipitate of which they consist is formed, rather than by the casual appearance which they present to the eye; in any case, mist and clouds must in the future be studied from the above new point of view.—Prof. von Helmholtz added to the above communication some remarks on the way in which the mixing of two fluids of different specific gravities is brought about. Such mixing is only possible as the result of vortex movements or of "breaking" waves. He had already dealt with the production of vortices, and the production of waves has recently engaged his attention, inasmuch as this problem has, up to the present, only been regarded from a one-sided point of view with reference to water, without taking into account the influence of the air which is moving over its surface. When wind blows over the surface of water, or when lighter air streams over a mass of heavier air, waves are formed, whose size and rate of propagation depend upon the relationship of the two fluids which are moving one over the other. To obtain the mechanical equations of these movements was the problem which he had set before himself for solution in a communication which he had recently made to the Berlin Academy. This dealt first with waves on water, and then the conditions involved in these were transferred to the consideration of waves in air. Waves 1 metre long on the surface of water, which are frequently met with in nature, correspond to waves in air 21 metres long—that is to say, to air-waves which extend over a considerable stretch of land. Waves in air are only visible in the cases where they are accompanied by condensations of vapour, the latter occurring in the case where the air rises several hundred metres to the crest of a wave. Prof. Helmholtz pointed out that the most important outcome of the whole theoretical consideration of the problem was the following: a quiescent surface of water over which a wind is blowing is in a state of unstable equilibrium; as the result of this, waves are produced as soon as the wind acquires a sufficient velocity, and the energy required to raise the water from the trough to the crest of each wave, as well as to produce the onward motion of the wave, is derived from the more rapidly-moving lower layers of air of which the wind consists. Friction plays a very subordinate part in the whole process.

November 8.—Prof. du Bois Reymond, President, in the chair.—Dr. Pernet demonstrated the latest and newest form of Edison's phonograph, and gave a minute description of the apparatus, illustrating his remarks by means of two instruments which were exhibited to the Society. He prefaced his description by a short historical introduction, from which it appeared that several years before Edison's discovery, a Frenchman named Gros had deposited with the Paris Academy a sealed packet containing a statement of the essentials for the construction of a phonograph.

Physiological Society, November 1.—Prof. du Bois Reymond, President, in the chair.—Dr. René du Bois Reymond spoke on the striated muscles which occur in the small intestine of the tench. The exceptional occurrence of striated muscles in the small intestine of this fish has long been known, as also that when the intestine is stimulated electrically it contracts suddenly, as does a skeletal muscle. The whole intestine is surrounded by these striated fibres arranged both longitudinally and circularly. Further examination revealed a very thin layer of both longitudinal and circular non-striated muscle-fibres, lying internally to the striated fibres. The only other known case of a similar occurrence of striated muscle-fibres in the walls of the small intestine is found in Cobitis; but in this fish the fibres do not extend as far as the rectum, as they do in the tench. The speaker set aside the idea that these striated muscle fibres are connected with the respiratory function of the intestine, by showing that other fish are also in the habit of swallowing air, and that in such fish the mucous membrane of the small intestine is extremely rich in blood-vessels, whereas this is not the case in the tench. He put forward the suggestion that the striated fibres in the intestine of the tench are a transitional form between unstriated and striated muscle-fibres, and based his views upon the observation that, firstly, the reaction of these muscles is alkaline, and, secondly, upon an analysis of an aqueous extract of them. An aqueous extract of striated muscles contains, as is well known, three different proteids; one which coagulates at 47° C., one which comes down at 56° C., and a third coagulating at 70° C. The proteid which coagulates at 47° C. does not occur in unstriated muscles, and was similarly found to be absent in the extract of the striated muscles of the intestine of the tench. The function of these last-named muscles has not as yet been made out.—Prof. Fritsch spoke on the sensory organs in the skin of fishes. Starting from the simplest forms in which they occur as end-bulbs or tiny dilatations in the nerves which supply the several somites in the embryos of fishes, the speaker described their gradual change of form during growth. The end-organ is always characterized by sensory cells—that is to say, by cells which have a pear-like shape and are provided with a sensory filament or hair, and are connected with nerve-fibres. The developmental change which takes place is as follows: at first the organ becomes protected by being set deeper into the skin, spaces are then developed superficially to the organ, and these are finally placed in communication with the surface of the skin by means of a minute orifice or somewhat lengthy canal. The lateral-line organs of fishes in several modified forms is developed as above described; the sense-organ, with its sensory cells and nerves, lying at its base. A further modification leads to the development of the closed vesicles of Savi, which are completely filled with a mucous secretion. In the further modification of structure met with in the ampullæ of Lorenzini, a change of functional activity is already marked, as shown by the fact that the sensory cells have lost their hairs and have been converted into secretory cells. The speaker expressed his concurrence with that view of the function of dermal sense-organs, according to which they are to be regarded as auditory organs in a low stage of evolution, set aside for the perception of vibrations and waves which are propagated through the water.

Meteorological Society, November 5.—Dr. Vettin, President, in the chair.—The President spoke on the interchange of air which takes place between regions of high and regions of low pressure. He first described his own measurements of the altitudes of the various most characteristic forms of clouds, finding them in complete accord with those of Abercromby and Ekholm. He then passed on to his determinations of the velocity of the wind at those several altitudes, using as a means of measurement the records afforded by the direction and rate of motion of the clouds. The mean values thus obtained for the rate of flow of the air-currents were compared in each case with the positions of maximal and minimal air-pressure; from this comparison the speaker found that the motion of the air between points of maximum and minimum pressure does not take place in the way in which it has usually been supposed to occur. He then gave a detailed account of the results of his observations, but these do not admit of being reproduced within the limits of a brief abstract.

SYDNEY.

Royal Society of New South Wales, August 21.—A "reception" of the members of the Society was held for conversational scientific discussion, and the exhibition of various objects of interest: upwards of 100 members were present.

September 4.—Prof. Liversidge, F.R.S., President, in the chair.—Mr. H. G. McKinney read a paper on irrigation in its relation to the pastoral industry in New South Wales, which was freely discussed.—Sir Alfred Roberts, Vice-President, exhibited a large collection of photo-micrographs taken by the late Captain Francis.

October 2.—Prof. Liversidge, F.R.S., President, in the chair.—The following papers were read:—The analysis of prickly pear; on the occurrence of arabin in the prickly pear (*Opuntia brasiliensis*), by W. M. Hamlet.—Personal recollections of the aboriginal tribes once inhabiting the Adelaide Plains of South Australia, by Edward Stephens.—The Chairman exhibited some interesting fungoid growths which had formed in water containing finely-divided gold in suspension. The gold had been precipitated from a weak solution of the chloride by phosphorus dissolved in ether; the mycelium of the fungoid growths had acquired a purple colour from the gold which it had absorbed; on incineration, a skeleton outline of the mycelium is left in gold.

AMSTERDAM.

Royal Academy of Sciences, October 26.—M. Mulder presented, for the Reports and Communications, an essay on tartarate of ethyl, and its relations to ethylate of sodium and potassium.—M. Grinwis spoke on two forms of energy occurring in rolling motion, and presented an essay on this subject for the Reports and Communications.—M. Rauwenhoff presented for the Transactions an essay in quarto, with plates, on the sexual generation of the Gleicheniaceæ, and communicated briefly the results to which his researches had led him.—M. van der Waals spoke of the equilibrium of solid compounds in presence of fluid and vapour mixtures, illustrated by the ψ surface of a mixture of two kinds of matter.

DIARY OF SOCIETIES.

LONDON.

THURSDAY, NOVEMBER 28.

INSTITUTION OF ELECTRICAL ENGINEERS, at 8.—Electrical Engineering in America: G. L. Addenbrooke.

FRIDAY, NOVEMBER 29.

INSTITUTION OF CIVIL ENGINEERS, at 7.30.—Principles of Iron Foundry Practice: G. H. Sheffield.

SATURDAY, NOVEMBER 30.

ROYAL SOCIETY, at 4.—Anniversary.

ESSEX FIELD CLUB, at 7.—How to commence the Study of Botany: George Massee.

SUNDAY, DECEMBER 1.

SUNDAY LECTURE SOCIETY, at 4.—Invisible Stars; the Use of the Camera in the Observatory (with Oxyhydrogen Lantern Illustrations): Sir Robert S. Ball, F.R.S., Astronomer Royal, Ireland.

MONDAY, DECEMBER 2.

SOCIETY OF ARTS, at 8.—Modern Developments of Bread-making: William Jago.

SOCIETY OF CHEMICAL INDUSTRY, at 8.—Some Notes on Variations in the Products of the Destructive Distillation of Different Gas Coals, Heated Separately in the same Retort, and under Similar Conditions: Watson Smith.—Cresontinic Acid and its Applications: I. Hauff.

VICTORIA INSTITUTE, at 8.—Instinct and Reason: Dr. C. Collingwood.

ARISTOTELIAN SOCIETY, at 8.—The Æsthetic Theory of Ugliness: B. Bosanquet.

ROYAL INSTITUTION, at 5.—General Monthly Meeting.

TUESDAY, DECEMBER 3.

ZOOLOGICAL SOCIETY, at 8.30.—On the Anatomy of Burmeister's Cariama (Chunga burmeisteri).—On the Relations of the Fat-bodies of the Sauropsida: G. W. Butler.—List of the Reptiles, Batrachians, and Fresh-water Fishes, collected by Prof. Moesch in the District of Deli, Sumatra: G. A. Boulenger.

INSTITUTION OF CIVIL ENGINEERS, at 8.—Ballot for the Election of Members.—Water-Tube Steam-Boilers for Marine Engines: John I. Thornycroft. (Discussion.)—The Triple-Expansion Engines at the Owens College, Manchester: Prof. Osborne Reynolds, F.R.S.

WEDNESDAY, DECEMBER 4.

SOCIETY OF ARTS, at 8.—Rabies and its Prevention: Dr. Armand Ruffer.

GEOLOGICAL SOCIETY, at 8.—On Remains of Small Sauropodous Dinosaurs from the Wealden: R. Lydekker.—On a Peculiar Horn-like Dinosaurian Bone from the Wealden: R. Lydekker.—The Igneous Constituents of the Triassic Breccias and Conglomerates of South Devon: R. N. Worth.—Notes on the Glaciation of Parts of the Valleys of the Jhelum and Sind Rivers in the Himalaya Mountains of Kashmir: Captain A. W. Stiffe.

ENTOMOLOGICAL SOCIETY, at 7.—Systematic Temperature Experiments on some Lepidoptera in all their stages: Frederic Merrifield.—Notes on Indian Longicornia, with Descriptions of New Species: Charles J. Gahan.—On the Peculiarities of the Terminal Segment in some Male Hemiptera: Dr. D. Sharp.—Notes on a Species of Lycænidæ: Lionel de Nicéville.

THURSDAY, DECEMBER 5.

LINNEAN SOCIETY, at 8.—Life History of a Stipitate Fresh-water Alga: G. Massee.—On the Anatomy of the Sand Grouse: G. Sim.

FRIDAY, DECEMBER 6.

GEOLOGISTS' ASSOCIATION, at 8.—*Conversazione*.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Proposed Method of Recording Variations in the Direction of the Vertical: H. C. Russell.—The Storm of September 21, 1888: H. C. Russell.—O Therii Forem Bilinearných: E. Weyr (V. Praze).—Journal of Physiology, vol. x., No. 6 (Cambridge).—Proceedings of the Linnean Society of New South Wales, vol. i., Part 1 (Sydney).—Quarterly Journal of the Geological Society, November 1889 (Longmans).—Papers and Proceedings of the Royal Society of Tasmania, 1888 (Hobart).—Proceedings of the Physical Society of London, vol. x., Part 2 (Taylor and Francis).—Transactions of the Seismological Society of Japan, vol. xiii., Part 1 (Yokohama).

CONTENTS.

THURSDAY, DECEMBER 5, 1889.

THE MANCHESTER CONFERENCE.

THE Manchester Conference on the working of the Technical Instruction Act was as important a representative gathering as has taken place for some years to consider an educational question. The Conference was called by the Technical Association, and the Executive Committee and the branch Associations throughout the country were strongly represented. Invitations were also addressed to the chief local authorities and School Boards in large centres, and the principal technical schools and institutions. It says much for the change which has come over public opinion in the last two years on educational matters, that a circular, unadorned by promises of party speeches by prominent M.P.'s, but merely inviting discussion on the details of the operation of an Education Act, should have sufficed to cram the Mayor's parlour with a body of nearly 300 delegates, representing more than sixty local authorities and institutions.

"Conferences," Mr. Acland said at the outset, "are usually disappointing," and it would be absurd to expect that so large and miscellaneous a gathering would dispose satisfactorily, within little more than a couple of hours, of the four difficult questions raised on the agenda sheet. But such progress as was possible was made, and the remorseless bell sounded with impartiality when a speaker's limit of five minutes had been reached. In this way a good many expressions of opinion from many different points of view were compressed into the afternoon, and few could have gone away without any new ideas suggested by the Conference. That is, if they had previously taken the trouble to acquaint themselves with the provisions of the Act, for no time was wasted in the room in explaining its general scope, though literature in abundance on the subject could be had from the bookstall at the door.

The subjects discussed were: the relation of the Act to elementary schools; the mode of its adoption and the preliminary proceedings connected therewith; the mode in which, and the conditions under which, grants may best be made by local authorities to institutions giving technical instruction, and the principle on which such grants should be apportioned among institutions of different grades; and the mode of re-organization by which the Science and Art Department may meet the new duties imposed upon it by the Act. The four speakers who introduced these subjects happily represented the four chief "interests" involved—education, politics, manufactures, and science.

Without following in detail the order of the discussion, we may briefly sum up the impression which it left.

The chief interest centred in the question of the relation of the Act to public elementary schools. It is no secret that a certain amount of misunderstanding and difficulty has arisen over the interpretation of the sections of the Act which bear on this knotty point. The Act forbids the application of rates raised under it to the instruction of scholars working in the "obligatory or standard subjects" of the Code. The meaning so far is clear. No scholar of an elementary school at the time working in any of the standards can take advantage of the Act. But how about ex-seventh standard scholars, or indeed of any children in elementary schools, above the exemption standard, to whom the managers may wish to give technical instruction? It is well known that, in many Board and some voluntary schools, a large number of children are retained who have passed all the standards, but are receiving science and art instruction, and earning grants from South Kensington. What are the powers of Boards and managers with respect to these children? One thing is certain—whatever Boards could do before the Act, that at least they can do still. There is no restrictive clause in the Act, which purposely enacts that "nothing in this Act shall be so construed as to interfere with any existing powers of School Boards with respect to the provision of technical and manual instruction." But there has always been some little doubt as to the exact status of School Boards with respect to higher elementary schools, and this the Act does nothing to remove. Sir Henry Roscoe's Bill, if carried, would have placed the whole position of higher elementary instruction on a sound and satisfactory basis. It is a great flaw in the present Act that it leaves matters where they were. It is, however, an ill wind that blows nobody any good, and it may be that certain advantages will, after all, result from this anomalous state of things. Opinions of experts not being unanimous about the meaning of the Act, it is clearly a time for experiments to be made. Liverpool is already moving in the matter, after obtaining Sir Horace Davey's opinion that it is within the power of the School Board to provide technical and manual instruction out of the rates under their general powers, and other School Boards need have little fear in taking a comprehensive view of the Act and applying to the City Councils for their share of the proceeds of the special rate.

The Conference also discussed the question whether a local authority is bound to distribute any grant which it may make among the different qualified schools which apply for aid, or whether it may take the initiative and adopt the course (in many cases the wisest) of concentrating its efforts on making one central school efficient. This question, on which some doubt was previously felt owing to the obscurity of the wording of the Act, was satisfactorily cleared up at Manchester. The town clerk of Blackburn threw down the challenge, by declaring that he intended to advise his Council that they had the power to build a technical school and give it all, or the greater part, of the proceeds of the rate. To this General Donnelly replied that there was nothing in this to which he could take exception, so that local authorities have—so far as the Science and Art Department is concerned—greater liberty of action than some had supposed; and who can object except the Science and Art Department?

But, perhaps, a question of more real importance even than this, is the nature of the qualification entitling a technical school to rate-aid. Here, again, the wording of the Act is not very clear, and it must be confessed that the discussion at the Conference still left it in doubt. In Section I., Sub-section (*a*), we read: "A Local Authority may, on the request of a School Board for its district or

any part of its district, or of any other managers of a school or institution within its district *for the time being in receipt of aid from the Department of Science and Art*," make provision for technical education in its district. The narrowest interpretation of this clause would confine the whole benefit of the Act to schools already receiving grants from South Kensington, and this view was understood by some members of the Conference—we hope wrongly—to be endorsed by General Donnelly.

We need hardly point out that such an interpretation would seriously restrict and cripple the operation of the Act. If there is one conclusion clearer than another from the Manchester Conference, it is that there is a general wish to use the rate for what we may venture to term its legitimate purpose—the assistance of those technical subjects which are not at present included in the Science and Art Directory. The worst thing that could be done would be to fritter it away in the form of doles to existing science and art classes; and yet, if only grant-earning schools can profit by the Act, this is what will inevitably tend to take place. Such an institution as the Leicester Technical School, which has classes in bootmaking, lace-making, &c., but no science and art classes, could get no help. The same would be true of such a school as the Finsbury Technical College.

We are glad to believe that so narrowing a meaning cannot fairly be given to the wording of the section. It is true that the words we have italicised make it necessary that the first institution to make a request to the local authority to put the Act in force must be already in connection with South Kensington, if it is not a School Board. But this condition only applies to the initial proceedings. When the request is made and granted, the local authority may make, "to such an extent as may be reasonably sufficient having regard to the requirements of the district, but subject to the conditions and restrictions contained in this section, provision in aid of the technical and manual instruction for the time being supplied" (not only in the school which makes the request, but) "*in schools or institutions within its district*."

That is, it may aid all higher schools already giving instruction which falls within the four corners of the Act, and this instruction includes very much more than the list of subjects on which grants can at present be earned.

And this leads us to the further question, What is meant by technical instruction in the Act? Some people, even at the Conference, understood it to mean merely the subjects in the Science and Art Directory, and any others which may be sanctioned by the Department on the representation of a local authority. This interpretation, again, would severely cripple the usefulness of the Act. At a time when the public is beginning to realize the mechanical nature of much of the teaching subsidized by South Kensington, and the want of elasticity and local adaptability which inevitably results from over-centralization, it would be nothing less than a disaster to tie down all science and art, and perhaps even technological teaching, to the rigid syllabus of a Government Department. Chemistry *quâ* chemistry would not be a "technical" subject, unless, forsooth, it were taught according to a certain syllabus, and followed by a certain examination. No really "technical" subject (except the four or five which are included in the Directory) would be "technical" under the Act until the local authorities in each district (not, be it noted, the managers of schools) had made a representation on the subject to the Science and Art Department, and a minute had been laid before Parliament.

But here, again, we are strongly of opinion that no such meaning can fairly be attached to the definition. "Technical instruction," so runs Clause 8, "shall mean instruction in *the principles of science and art applicable to industries, and in the application of special branches of science and art to specific industries and employments. It shall not include teaching the practice of any trade or industry or employment*." There is the definition. What follows is not a restriction, but an amplification, intended to provide a mode of clearing up doubtful cases. Some one might hereafter declare that some subject, as, for example, mathematics or landscape-painting, though included in the Directory, was not contemplated by the Act, as not being "instruction in the principles of science and art applicable to industries." The section therefore expressly declares that the definition *shall* include all such subjects; and if there be any other subject outside the Directory about which doubt is entertained, that doubt may be set at rest by a representation from a local authority. The Science and Art Department is umpire in doubtful cases, but no appeal to the Department is necessary with reference to subjects—say the principles of weaving, dyeing, plumbing, &c.,—which fall unmistakably within the definition. That, at least, is our view, and we believe the only rational one. It seems to us as clearly the meaning of the letter of the Act, as it was certainly the intention of its promoters.

The Science and Art Department, however, will have the power to define the mode of teaching of technical subjects for the purpose of earning Imperial, though not local, grants. The Department might, as was suggested at Manchester by Principal Garnett, take over the whole system of grants and examinations now controlled by the City and Guilds Institute. But we venture to hope—and Principal Garnett himself would, we believe, agree in this—that the authorities at South Kensington will think very carefully before embarking on a new system of payments on results, in the case of subjects which admit far less of such a test than most of those included in the Science and Art Directory.

They would do well to rely far more on efficient inspection than on individual examinations, and if the inspection were made a reality, instead of being, as now, too often a farce, they might, perhaps, ultimately base their grants for technical instruction on the amount of local contributions, in some such way as that provided for in the Welsh Intermediate Education Act. The Manchester Conference was strongly opposed to any increase of centralization, and the greatest possible freedom ought to be allowed to localities from the outset to develop their own system to suit their own needs.

If the Conference was decided on this point, it was, we think, equally decided that, under a broad interpretation of the Act, the powers conferred on local authorities are really very extensive. It is little short of a scandal that an Act for the improvement of English industry should itself offer such an exhibition of bad workmanship. But

it is clear that the right way to solve the problem is for local authorities and School Boards to push ahead, as we believe they can do without fear. The list read by Sir Henry Roscoe at the opening of the proceedings shows what progress in this direction has already been made towards adopting the Act, and the Conference can hardly fail to result in a still more vigorous attempt to make a wise and extensive use of its provisions.

AMERICAN ETHNOLOGICAL REPORTS.

Sixth Annual Report of the Bureau of Ethnology to the Secretary of the Smithsonian Institution, 1884-85. By J. W. Powell, Director. (Washington: Government Printing Office, 1888.)

FROM the introductory remarks of the Director of the Bureau, we learn that the results of the research prosecuted among the North American Indians, as directed by Act of Congress, were of special interest during the continuance of the work in the fiscal year 1884-85.

As in forme years, the labourers in the mound explorations were remarkably successful, more especially in the territories east of the Rocky Mountains, where Prof. Cyrus Thomas, in 1885, and his coadjutors, Messrs. Middleton and Thing, subsequently, made important finds in Indian pottery, which were unique of their kind. Even more valuable are the results of the explorations carried on in New Mexico by Mr. and Mrs. Stevenson, the latter of whom succeeded in obtaining the largest and most important collection extant of objects relating to the sociology of the Zuni tribes. This rare treasury of Indian relics includes specimens of woven fabrics, pottery, stone implements, both ancient and modern, pictured urns, shrines, altars, sacred masks, fetishes, plume sticks, and other objects connected with the ancient mythology and religious practices of these people. Owing to the great variety of the objects, their true character cannot be determined without prolonged investigation, and in the meanwhile they have been deposited in the U.S. Museum, where they await their final classification. According, however, to Mr. Curtis, these, as well as the still more numerous collections of pottery, stone implements, and other objects, amounting to 4000 specimens, which have been obtained in New Mexico, all belong to the indigenous arts and industries of the ancient tribes who occupied the almost unknown tracts of Central America in which the Pueblo Indians are now located.

In the department of linguistic research, prosecuted by the various *employés* of the Bureau, none have perhaps been more successful than Mrs. Ermine Smith, who was fortunate enough to discover two Onondaga MSS., and one MS. in the Mohawk dialect, all of which she has annotated and translated with the assistance of a half-caste of Tuscaroran descent. The origin and history of these MSS. are not distinctly known, but it is conjectured that they are copies of originals which have been lost or destroyed. In their present form, they are, however, alike interesting from a sociological and a linguistic point of view, for while the Mohawk MS. gives an account of the religious rites and chants of the Iroquoian League which represented the elder members of the entire nation, one of the Onondaga MSS. records the ritual in use among the younger members of the same council, and the other the form of address used by the chief Shaman on the initiation of a newly elected chief.

These curious records have been turned to good account by Mrs Smith in the completion of her Tuscarora dictionary, and in filling up her vocabulary for the "Introduction to the Study of the Indian Languages" now preparing for publication.

In the Far West, and especially in California, the results of linguistic field-work are not equally satisfactory; and in the latter province, it would appear from the report of Mr. Henshaw, who was charged with the inquiry, that a number of the native dialects are extinct. Only a month before his arrival, an old woman had died who was the last person to speak the language of the Indians of Santa Cruz. The search for still surviving members of the several families of Indian languages current on the arrival of the Spaniards has not, therefore, begun too soon. The general results of these linguistic researches are embodied in a work entitled "Proof-Sheets of a Bibliography of the Languages of the North American Indians." This volume, a quarto of more than 1100 pages, was compiled by Mr. Pilling, and issued in 1884 by the Institute, which, with its usual liberality, has distributed the hundred copies printed to other public institutions, and to the various collaborators in the work.

In turning from the highly interesting explanatory remarks of the Director to the various monographs contained in the volume before us (a folio of more than 800 pages), we have first to notice the comprehensive and profusely illustrated treatise of Mr. Holmes, "On the Ancient Art of Chiriqui on the Isthmus of Panama."

Here the author supplies the technologist with an exhaustive history of the rise and development of plastic and textile art in this part of the continent, while he also treats fully of the literature and geography of this hitherto little-known province, whose position between North and South America imparted to the people some of the characteristics of the civilization of both sections of the western hemisphere.

Almost the whole of the enormous mass of clay and metal objects found in Chiriqui was extracted from tombs in the various *huancals*, or cemeteries, which are scattered over the Pacific slope of the province. These were first made known to science by Mr. Merritt, the director of a gold mine in Veragua, who, on hearing of the accidental discovery of a gold figure in Chiriqui, visited the district, and published a report of his explorations in 1859. From him we learn that in 1858, after it became known that a golden image had been discovered at Bugava, more than 1000 persons flocked to the spot, who it was estimated had in that year collected 50,000 dollars' worth of gold from one cemetery alone, which had an area of only 12 acres. A curious fact connected with the plastic decorations of the Chiriqui vases and other objects is that no vegetable forms have served the artificers as models, animals alone having been used for the purpose, as crocodiles, armadillos, monkeys, lizards, alligators, owing probably to their zoo-mythic conceptions of their divinities. Among the various groups of vases, the one comprising the so-called "alligator ware" is the most interest-

ing; this animal being not only represented as a surface ornament, but serving as a model for the form of such dissimilar objects as whistles, rattles, tables, stools, jars, vases and other utensils. Occasionally the human figure appears under some grotesque form, and less frequently it is used to represent a divinity. According to Mr. Holmes, the entire system of the scrolls, frets, and other devices used in Chiriqui art have been derived from various parts of the body of an animal, probably the alligator, and he regards this system of ornamentation as indigenous to the district. In a separate article, the author treats of textile art in its relations to the development of form and ornament, and more especially with respect to the industries of the early American people.

The article on the Central Eskimo, by Dr. Franz Boas, although complete and admirable of its kind, has comparatively little interest for the English reader conversant with the results of Arctic research, since a very large and important part of the information given has been derived from the narratives of Franklin, Ross, Parry, and other more recent British explorers. Yet some additions have been made to our older knowledge of the Eskimo by Dr. Boas, who gives much interesting information regarding their tribal laws and customs, the musical art of the people, and their capacity for drawing; while he relates several curious tales and traditions, which present so remarkable a similarity to those of the Greenlanders and the Behring Straits' tribes as to make it probable that all these people are of one race.

The Rev. O. N. Dorsey, to whom the Bureau is indebted for the compilation of seventeen vocabularies of the different dialects used by the Oregon Indians, adds an interesting contribution to this volume, in which he describes the results of his visit, in 1883, to the Osages in the Indian Territory. During his short stay he obtained information regarding the existence of a secret society of seven degrees, in which a knowledge is preserved of the grades and general history of the various gentes and subgentes, with their taboo and names which are regarded with reverence and not spoken of. Owing to the strict secrecy usually maintained by members of this society, it was with extreme difficulty that he induced two of the initiated to recite to him the traditions referring to the mythic history of their tribe, which had been imparted to them on their initiation. These traditions, which the author gives with an interlinear translation, record the passage of the primæval Osages from higher worlds before they bore the semblance of birds, or had acquired from a beneficent red eagle the bodies and souls with which they alighted on the earth. The sacred chart on which their descent was symbolized by a river flowing beside a cedar, the tree of life, surrounded by sun, moon, and stars, was observed by Mr. Dorsey to be tattooed on the throats and chests of some of the elder men; but the younger Osages knew nothing of such symbols, and he was asked not to speak to them on the subject. From all he saw and heard among these and various tribes of Iowa and Kansas, he believes that in this traditional record of the descent of their gentes from different birds and animals, we have a clue not only to the names by which they are distinguished, but to the meaning of the chants and war-songs which only members of the seven degrees of their sacred societies have the right to sing. It would appear that an arrangement by sevens is common to various kindred tribes, and there is reason for assuming that wherever mythic names or taboos are in use there are, or have been, secret societies or mysteries, which have been derived from early traditional history.

In an elaborate article by Prof. Cyrus Thomas, entitled "Aids to the Study of the Maya Codices," we have an interesting account of the far-famed Maya Codex, which was acquired by the Royal Library of Dresden in 1739, and a large portion of which was collated for Lord Kingsborough's great work on "Mexican Antiquities," of which it forms the larger part of the third volume. According to Dr. Thomas, this unique document consists not merely of one, but of several original MSS., while it presents no evidence, as often asserted, that its symbols, figures, and signs are to be accepted as alphabetical, or phonetic, characters, its series of dots and lines seeming to indicate a close relationship with the pictographic system in use amongst the North American Indians. He is of opinion that these series have a chronological significance, based on the method of counting time common to the Mexicans and Mayas, in which a religious, or hierarchical, cycle of 260 days was recognized, as well as the solar year calendar of 360 days in use among the people. This interpretation must, however, for the present rank as merely conjectural, although his elaborate analyses of the Maya symbols cannot fail to be of use to the few interested in the solution of the curious philological problem involved in the elucidation of this unique codex, to which special notice was first drawn by Alexander von Humboldt. His acquaintance with ancient South American MSS. enabled him to show that, while its symbolic characters presented a close affinity with those used by the Mexicans, the material of which the MS. was composed was the Mexican plant metl, *Agave mexicana*.

EXACT THERMOMETRY.

Traité pratique de la Thermométrie de précision. Par Ch. Ed. Guillaume. Pp. xv. and 336. (Paris: Gauthier-Villars, 1889.)

THE thermometer, practically as we now have it, is an instrument several centuries old, and by far the most popular of all scientific apparatus. Yet probably much less is generally known about it than about its companion implements the barometer and the telescope. The reason for this want of knowledge lies doubtless in the fact that the common use of the thermometer is chiefly for rough observations on the temperature of the air, and for this the ordinary instruments are sufficiently accurate as they leave the maker.

Meteorologists and physicians, however, occasionally have the zeros of their thermometers tested; and, for factory work, other points have sometimes to be examined. But in chemical and physical laboratories, investigations not unfrequently require that thermometers should be corrected with all possible delicacy, if the resulting measurement is to be exact and valuable. For such operations there has hitherto been no exhaustive guide; and M. Guillaume, whose ample experience in the Bureau international des Poids et Mesures is a guarantee for the practical value of what he writes, has done good service by issuing the present work at an opportune moment.

It is natural for a "Traité pratique" to refer mainly to the mercurial thermometer; for the great majority of practical thermometric measurements lie within its scope. Having a range from -40° to at least 360° C., and a possible sensitiveness of about $0^{\circ}\cdot001$, it rarely has to be exchanged for more delicate or larger-scaled appliances. Even the air thermometer—a sort of general appeal court in measurements of heat—is always accompanied by a number of ancillary mercurial thermometers.

To begin at the beginning (which, by the way, the author has not done), a thermometer has to be made; and the method of making it has a serious influence on the result. One maker will overheat his glass, and thus make the bulb harder than the stem; another will leave irregularities in the bulb which will cause the zero to rise irregularly; a third can never perfectly "deprive," as it is termed, the stem of air; the breath of a fourth is constantly leaving fatty matter in the capillary tube. In short, there are endless variations in technique, to which, for delicate instruments, attention should be drawn.

The division of the thermometer is, as might have been expected, well described; and minute details of calibration (chiefly by the method of broken threads) are duly set forth. Then follows a notice of a less familiar correction—that, namely, which depends on internal pressure when the thermometer is in a vertical position, and that which is produced by the (external) pressure of the air. Two methods of ascertaining the thickness of the bulb are given, but they are both inferior to Stokes's, which turns upon measuring angularly the distance between a spot on the outside of the glass and its reflection from the inner surface. Then ensues a description of the usual apparatus for determining the zero (which M. Guillaume seems to read somewhat too soon after immersing the bulb in the bath); and the method of ascertaining the boiling-point of water accompanies this. In the comparison of thermometers, which is next treated, the present writer prefers an air current to the metal plunger figured on p. 125.

If we observe the zero of a thermometer soon after manufacture, and subsequently at frequent intervals, we shall find that it is continually rising. The late Dr. Joule observed this ascent in one of his thermometers for more than seven-and-twenty years. There can be no doubt that it is due to a kind of setting of harder silicates in presence of softer or more viscous silicates in the mixture of which the bulb is composed. The softer glasses show it more than the harder ones; but in all exact work, it has to be determined and allowed for. This variation takes place at the ordinary temperature. If now we heat the thermometer moderately (say to 100°) and cool it, we shall notice a temporary depression due to a temporary set. If, again, as is often the case in factory work, we heat the thermometer for a long time to a high temperature (say 180°) the glass of the bulb (especially if soft) will become sensibly more plastic; and will sometimes yield sufficiently to external pressure to cause an ascent of 6°. At higher temperatures the ascent is still greater. Measurements of zero are therefore exceedingly important, even for moderately accurate work, and the author does not fail to draw minute attention to them. We should have been glad if at this point he had said something about the form of thermometer bulbs. Bulbs, for instance, which have their sides concave vary readily in capacity with barometric changes.

The exposure correction has exercised the minds of physicists for a great many years. When the bulb but not the stem of a thermometer is in a bath, the stem may clearly have a different temperature from the bulb, and the reading as a whole will be too low. In most chemical and physical laboratories, it is usual to follow Regnault, and to add, to the otherwise corrected reading T., the quantity

$$\alpha(T-t)N.$$

(N is the length in degrees of the exposed column, t is its mean temperature, and α is the difference between the expansion coefficients of glass and mercury.) There can be no doubt that this correction gives too low a result at high temperatures. It has been shown that if instead of α we simply write $(\alpha+\beta N)$—calculating α and β from the results— the demands of experiment are fulfilled with all desirable accuracy. The author, however, is disposed to leave the reader pretty much to his own devices for this correction.

The remainder of M. Guillaume's work is chiefly devoted to the comparison of the mercurial with the gas thermometer, and the measurement of dilatation of solid bodies: there are some valuable tables at the end.

A perusal of this "Traité pratique" will perhaps cause some regret that in most of our measurements of temperature we should be obliged to employ a material that is constantly undergoing physical change, and that necessitates in instruments constructed of it so many corrections. It is, on the other hand, a fortunate circumstance that we have in the mercurial thermometer an admirable means of establishing and measuring the corrections necessary to be imposed wherever glass is accurately worked with. For it cannot be too emphatically pointed out that every lens, cylinder, flask, or other glass instrument we employ is more or less amenable to these corrections. M. Guillaume's work, therefore, should command, as it deserves to command, a very wide interest.

EDMUND J. MILLS.

THE FAUNA OF BRITISH INDIA.

The Fauna of British India, including Ceylon and Burma. Edited by W. T. Blanford. Vol. I. Fishes. By Francis Day. Pp. 548; 164 Figs. (London: Taylor and Francis, 1889.)

THE first volume of this, the last work of the well-known Indian ichthyologist, Francis Day, was issued under particularly painful circumstances, viz. almost on the very day of the author's death. The state of Mr. Day's health during the last few months had prevented him from attending to the correction of the proofs beyond the middle of this volume, which deals with the Chondropterygians, the Physostomes, and the Acanthopterygian family *Percidæ;* and the task of carrying the remainder through the press has fallen on the editor. This work is but a condensation of the author's quarto "Fishes of India," completed in 1878, so valuable for the copious and beautifully-executed lithographic plates which accompany it. And, fortunately, a number of these excellent illustrations (one for every

genus) have been reproduced, intercalated in the text, in a manner which is highly creditable to the Lithographic Etching Company.

Considering how much remains to be done in the investigation of the fish-fauna of India and its British dependencies, it is a matter of regret that so little attention has been paid to the subject since Mr. Day's departure from India. The supplement to the "Fishes of India," published in 1888, records no more than sixty additions to the number of species, a figure which might easily have been doubled in the same lapse of ten years but for the unaccountable want of interest shown in this most important branch of study. As an example of the results which may be attained by an enthusiastic collector in those regions, we may allude to the collections of fishes brought together during the last three or four years by Mr. Jayakar, a surgeon stationed at Muscat, at the entrance of the Persian Gulf, and presented by him to the British Museum, by which no less than twenty-five species, many of large size and of commercial importance, have been added to the record of the fishes of the Indian Ocean. It is to be hoped, therefore, that this new and well got up issue of the "Fishes of India" in a more portable form will give a fresh stimulus to the study of that fauna. A little more, however, might have been done to facilitate the identification of species, a particularly arduous task, the difficulties of which would have been greatly lessened by the preparation of satisfactory "keys." Such as they appear in this work, viz. mere abbreviated tabulations of characters, without or with scarcely any groupings under special headings, the synopses fail in their object, and it is really a matter of regret that the editor did not bring his influence to bear for a thorough recasting of this portion of the work, especially in the case of such extensive genera as *Barbus*, *Nemachilus*, *Lutjanus*, or *Serranus*, where the work of identifying species by means of the synopsis given is perfectly discouraging. With the enormous multitude of species which our present knowledge requires us to grasp, it is of primary importance that every possible facility should be given to the naturalist who uses a manual of this kind, which after all is intended chiefly for those who have but an elementary knowledge of the special subject.

The above notice was in type when we received a copy of the second and concluding volume (509 pp., 177 figs.). We are glad to see that the editor has, in many cases, recast the synopsis of genera and species. The total number of fishes known from Indian waters is given as 1418.

In concluding, we congratulate Mr. Blanford on having, under difficult circumstances, so successfully brought out this portion of the "Fauna of India"; and we join in his tribute to the memory of the late author, who, as he justly says, has rendered signal service to Indian zoology.

OUR BOOK SHELF.

La France Préhistorique. Par Émile Cartailhac. (Paris: Félix Alcan, 1889.)

THIS volume forms one of the well-known series, "Bibliothèque Scientifique Internationale," published under the direction of M. Ém. Alglave. The subject, we need scarcely say, is one with which M. Cartailhac is eminently competent to deal, and all who are interested in the study of prehistoric times will be glad to have so compact and lucid an account of the facts to which the work relates. He begins with a good sketch of the rise and progress of modern ideas with regard to primitive civilizations and the antiquity of the human race; and this is followed by a discussion of the questions connected with man's place in Nature, his origin, and the supposed traces of his existence during the Tertiary period. An admirable chapter is devoted to the striking manifestations of artistic impulse by men of the Palæolithic age. The monuments of the Neolithic era in France are grouped with perfect clearness, and M. Cartailhac has not failed to do justice to any one of the various questions which these monuments have forced upon the attention of students. The scientific value of the book is enhanced by the fact that he avoids as much as possible the use of purely hypothetical reasoning. When he comes to sets of phenomena which cannot be simply and naturally accounted for, he thinks it better to offer no theory at all than to suggest purely conjectural explanations. The illustrations, although in no way remarkable, will be of considerable service to readers who have not made themselves familiar with the aims and methods of archæological science.

Experimental Science (Elementary, Practical, and Experimental Physics). By George M. Hopkins. (New York: Munn and Co. London: E. and F. N. Spon, 1890.)

THE subject of experimental physics is here set forth in a manner calculated to afford to the student, the artisan, and the mechanic, a ready and enjoyable method of acquiring a knowledge of this fascinating subject. Although the popular style adopted by the author perhaps makes the book better suited to the general reader than to the student, it may safely be said that all classes of readers will find much to interest them. All the subjects usually included in the comprehensive term "physics" are discussed; and, in addition, photography, microscopy, and lantern manipulation. By carefully performing each experiment at the time of writing the description, the author guarantees certain success if his instructions are followed. There is an excellent chapter on "mechanical operations," containing many valuable hints on glass working, simple apparatus for laboratory use, soldering, and moulding. Mathematical expressions are almost entirely excluded.

The book is chiefly remarkable for its hundreds of excellent illustrations, very few of which are diagrammatic. Many of them, like a considerable portion of the text, have already appeared in the *Scientific American*, which is alone sufficient guarantee of their quality. Some of the latest inventions, including Edison's new phonograph, are described and illustrated.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

"Modern Views of Electricity."

THE only point really at issue between Prof. Lodge and myself seems to be whether the difference of potential between two metals in contact can be measured by the Peltier effect or not. He asserts that he regards the statement that it can as an axiom, while I maintain that the only reason for calling it an axiom is that it cannot be proved. Let us take a simple case. Suppose we have a condenser, the plates of which are made of two different metals metallically connected, and that this con-

denser is placed in a vacuum, then, so far as I can see, Prof. Lodge's principle must lead to the conclusion that the difference of potential between the plates of the condenser is proportional to the Peltier effect; but if this is so, it is quite easy to show by the second law of thermodynamics that if the system is regarded as a heat-engine, the Peltier effect cannot vanish except at the zero of absolute temperature.

On the other points mentioned by Prof. Lodge in his letter, there does not seem sufficient difference of opinion between us to make it worth while discussing them.

In conclusion, let me assure Prof. Lodge that I am thoroughly in sympathy with the view that the consideration of the behaviour of the medium in the electric field is absolutely essential. I do not think there is anything inconsistent with this in the paragraph he quotes, which was intended to express what is well known to have been the opinion of Maxwell himself—that the key to the secret of electricity would be found in the "vacuum" tube.

THE REVIEWER.

The Physics of the Sub-oceanic Crust.

IN your article on the above subject in NATURE of November 21 (p. 54), the important proposition that the earth's crust rests on a liquid layer is once more brought to the front. The question reaches to the very basis of geology, but, like most of those of real importance, is not now recognized by the Society which occupies apartments in Burlington House, rent free, for the purpose of forwarding the study of geology.

Nothing is more obvious to the geological student than that enormous thicknesses of strata have been formed at practically one level. We do not find that, when a thousand feet of sediment has been deposited under water, the deposition began in water which was 1000 feet deep, and went on gradually lessening the depth until the sea or lake was filled up; but we do find, as in the coal-measures, that the entire 1000 feet was deposited in most uniformly shallow water; that therefore the crust of the earth must have sagged foot by foot as additional feet of burdens were laid upon it. Deltas that have not yet been bottomed show hundreds of feet of silt, every yard of which was deposited at only a few feet from the surface level of the water; estuaries and river valleys slowly sink where there is sedimentation; ice-caps tell of accumulation accompanied by depression and submergence, and re-elevation when the burden is melted and dissipated; coral formations and submergence are regarded as well-nigh inseparable, and even lava-flows flowing on to a plain have sunk its level in a degree corresponding with their mass. Where there is fifty or a thousand feet of piled-up lava-sheets you may look for a fault of like amount on its flanks, like that which, still unsuspected by geologists, cuts the Isle of Mull in half. Whether we look at the past or the present, we seem to see evidence of a crust resting in equilibrium on a liquid layer, and sensitive to even apparently insignificant readjustments of its weight. And if the crust did not respond to, and make room for, the burdens laid upon it by the removal of disintegrated land and its redeposition as silt under water, would not the seas be choked for miles round every coast? The abrading action of the waves cuts down the land, be it high or low, to a dead uniform level, and sooner or later it must become first beach, and then sea-bottom. There it is covered with silt or sea-weed, and is no longer abraded, and would, therefore, form great level tracts, instead of almost uniformly shelving coasts, unless it yielded *pari passu* to the increasing weight of sediment and water. The immediate effect of cutting down cliffs, say of 100 feet in height, and removing them in solution or by wave action, is to relieve the pressure at their base; and I claim that, wherever I have excavated for the purposes of collecting under such conditions, I have found a decided slope inwards away from the sea, if the strata were at all horizontal, no matter what direction their general inclination might be at a distance from the sea margin. But on the beach, a little way from the base of the cliffs, the slope is, on the contrary, towards the sea, and whatever the general inclination may be, we see the harder ledges, even if but a few inches thick, sloping away into deeper and deeper water until lost to view; and if you choose to follow them and dredge, you trace them tending downwards into yet deeper water. This appears to me to be simply because the relief from pressure has made the beach-line the crown of a slight arch, and an arch that continues to grow and travel, else how could we collect day after day and year after year, on the same spots, such as Eastware or Bracklesham Bays, fresh crops of fossils after every tide? I have hundreds of times picked up every vestige of a fossil on perhaps an acre of Eocene or Gault, yet a couple of tides have removed so appreciable a layer that the area has appeared studded with fresh specimens that were twenty-four hours previously wholly covered and concealed under matrix. Yet this ceaseless waste does not lower the level of the beach as it ought to.

And if such slight displacements as result from coast denudation have so appreciable an effect, what must take place in ocean, if subsidence is going on, and the weight of water on the increase? Darwin saw that the vast rise of land, which he so graphically describes in South America, must have been accompanied by a corresponding depression in the bordering oceans; and in turning his pages you almost expect to come on the view that depression in the Pacific must be the cause of the upheaval of the coast-line. It only wanted the liquid layer theory to make the dependence of one on the other obvious. No general rise of land has, or ever can, take place, under the overwhelming pressure of the great ocean depths, and oceans are thus permanent; the struggle is confined to whether the liquid layer shall overcome lateral resistance and find relief along the coast-lines, which are the nearest lines of least resistance, and already weakened by abrasion, forming coast ranges, or rending the crust, and pouring over thousands of square miles from fissure eruptions; or whether it shall overcome vertical resistance, and raise the beds of shallower ocean eventually, perhaps, into land.

Thus the tendency, as noticed by the writer of your article, is for deep oceans to become deeper, under pressure which may increase but never relaxes, and for mountain-chains to grow into higher peaks, the more weight is lessened by valleys being cut up and denuded.

This theory accounts for innumerable facts in the physics of the earth which space would not permit me to enter on, and is, so far as I know, opposed to none.

J. STARKIE GARDNER.

Area of the Land and Depths of the Oceans in Former Periods.

IN a letter to NATURE (p. 54), entitled "Physics of the Sub-oceanic Crust," by my friend, Mr. Jukes-Browne, the following passage occurs:—

"We are at liberty to imagine a time when there was much more land than there is at present, and when all the oceans were comparatively shallow."

I wish to point out that such a condition of things could not obtain if the bulk of the ocean water was the same as now. To get more land, the ocean would have to be *deeper* than now, not shallower. An easy way of conceiving the effect of shallowing the oceans is to mentally lift up the present ocean-floors, the result being an overflow of water and decrease of land area. The only possible way of shallowing the oceans and increasing the area of the land would be to make the ocean-floors perfectly flat, and to surround the continents with vertical walls of rock—in fact, to make the oceans into docks, which nevertheless would exceed two miles in depth.

I pointed out this geometrical fact in "Oceans and Continents"[1]—an article which has provided some of the stock arguments against their fixity. If, therefore, theorists feel it necessary that the land areas should be greater, and the oceans shallower, in former ages, they are bound at the same time to provide some means of decreasing the bulk of the ocean waters. This seems difficult, as other theorists tell us that the amount of water on the globe goes on decreasing, being used up in the hydration of the crust of the earth, and point to the condition of things on the moon as the final stage of our planetary existence.

T. MELLARD READE.

Park Corner, Blundellsands, near Liverpool, November 23.

Distribution of Animals and Plants by Ocean Currents.

SOUS ce titre, vous donniez naguère (vol. xxxviii. p. 245) une correspondance de M. A. W. Buckland concernant divers phénomènes observés à Port-Élisabeth, dans l'Afrique du Sud. Entre autres choses il y était relaté que, vers la fin de l'année 1886, un fruit analogue à celui du cocotier avait été porté par la mer sur le rivage de Port-Élisabeth en même temps que des quantités considérables de pumites ou pierres-ponces.

[1] *Geological Magazine*, 1880, p. 389; also, see letter in same magazine, 1881, p. 335, headed "Subsidence and Elevation."

Le fruit ramassé par un *boy*, "*Il* y porte la dent, fait la grimace. . . . Le moindre ducaton serait bien mieux *son* affaire." Notre *boy* se décide dès lors à porter le fruit au jardinier de "North End Park." Le végétal confié à la terre poussa et donna un arbre, *Barringtonia speciosa*, qui avait atteint 4 pieds de hauteur vers le milieu de l'année 1888.

M. A. W. Buckland émettait l'hypothèse que fruit et pumites, comme aussi quelques poissons et serpents appartenant à des espèces jusque-là inconnues dans le pays, et arrivés en même temps, provenaient des parages de la Sonde, et, à la suite de la grande éruption de Krakatoa en 1883, avaient été portés par les flots jusque sur les rivages de la côte Sud-Africaine.

Il n'y a plus à douter, je crois, de la provenance des pumites. Je n'ai rien à dire au sujet des poissons et serpents. Mais pour ce qui est du fruit de *Barringtonia sp.ciosa*, il me semble qu'on pourrait lui donner une autre origine ou point de départ, et diminuer ainsi de beaucoup la durée de sa traversée sur l'océan.

L'arbre *Barringtonia speciosa* croît, en effet, à Madagascar, où je l'ai vu à Tamatave, sur les bords de la mer. Il ne serait donc point du tout improbable que le fruit porté par les flots à Port-Élisabeth provint de la grande île Africaine. En même temps que je signalais l'arrivée sur nos plages Malgaches des pumites de Krakatoa, en Septembre 1884 et en Février 1885 (*Cosmos*, nouvelle série, No. 12, p. 320), j'envoyais en Europe divers spécimens de ces pumites ramassés sur la plage de Tamatave. Parmi les spécimens adressés à la Société Nationale d'Acclimatation de France s'en trouvait un dans lequel s'était logé une partie de végétal,—une fleur, si je ne me trompe, d'une espèce de *Terminalia*, qui croît aussi à Tamatave sur les bords de la mer (*Bulletin de la Société Nationale d'Acclimatation de France*, Décembre 1884, p. 983).

Un fruit de *Barringtonia speciosa* arbre qui, comme je l'ai fait remarquer, croît au bord de la mer sur la côte orientale de Madagascar, a très bien pu, de même, prendre "passage" sur une pumite ou un banc de pumites atterrées sur la plage Malgache ; puis, à la première haute marée, avoir cinglé sur ce "transport" d'un nouveau genre vers la côte Sud-Africaine, poussé par le Courant Indien, jusqu'à son arrivée à Port-Élisabeth, où il a enrichi le "North End Park" d'un nouvel arbre exotique.

Mais, même dans cette hypothèse, le phénomène observé à Port-Élisabeth n'aurait pas un moindre intérêt. L'île de Madagascar y gagnerait de pouvoir être considérée comme une grande "escale," établie par le Dieu Créateur et Ordonnateur des Mondes, pour le service des "Messageries maritimes" de la Nature entre les Archipels de la Malaisie et la côte Sud-Africaine.

Veuillez agréer, Monsieur le Rédacteur, les respectueuses salutations de votre humble serviteur,

PAUL CAMBOUÉ, S.J.,
Missionnaire apostolique à Tananarive.

Tananarive, Madagascar, 15 Octobre.

A Marine Millipede.

BRITISH naturalists, especially such as work on the south coast, will hear with interest that Mr. J. Sinel has lately found in Jersey the very curious *marine* Millipede, *Geophilus submaritima*, Grube (*Verh. d. schles. Gesellsch.*, 1872). Dr. Latzel, of Vienna, tells me that the specimens differ somewhat from the type, and probably constitute a well-marked variety. Some examples were found close to the low-water mark of very low spring tides, where they could not be exposed more than two days in a fortnight.

The Geophilus occurs associated with two or three beetles, of which at least one appears to be new, and with a remarkable Chelifer which is probably identical with *Obisium littorale*, a new species described by Moniez from Boulogne, in this month's *Revue Biologique*, or with the doubtful species *O. maritimum* of Leach (*Zool. Miscellany*, iii. 1817).

Mr. Sinel's crowbar, a tool the naturalist makes too little use of, is doing wonderful service. D. W. T.

December 2.

A Case of Chemical Equilibrium.

DURING some experiments made in connection with a research recently laid before the Royal Society, we came upon an interesting case of chemical equilibrium.

The object of the research was to determine the rate of evolution of oxidizing material liberated, under varied conditions, in a solution containing dilute hydrogen chloride and potassium chlorate. There was also introduced a little starch solution and a small quantity of potassium iodide to serve as an indicator of the completion of a certain amount of work, which was the conversion of a known small weight of sodium thiosulphate into tetrathionate. The completion of this change was marked by the appearance of a blue colour in the liquid. The operation was then repeated.

In these experiments the amount of substances undergoing change, when compared with the total amount present, was so large that the masses of the substances remained practically constant during each experiment.

In such a mixture the condition of equilibrium may be considered to be represented by the following equation :

$$n\mathrm{HCl} + m\mathrm{KClO_3} = m\mathrm{HClO_3} + m\mathrm{KCl} + (n - m)\mathrm{HCl},$$

where n is greater than m.

We may then regard the oxidizing material as being liberated by the reaction of the $(n-m)$ molecules of hydrogen chloride with the m molecules of hydrogen chlorate so liberated. The presence of the m molecules of potassium chloride will produce its specific effect (in this case acceleration) on the rate of reaction. So that out of the n molecules of hydrogen chloride employed only $n - m$ are actively engaged in liberating oxidizing material, the rest having been employed in saline decomposition. If such be the case, it ought to be possible to obtain a similar rate of oxidation by taking m molecules of hydrogen chlorate instead of potassium chlorate, and then reducing the hydrogen chloride used from n to $(n - m)$ molecules. If we then add the m molecules of potassium chloride we should then be able to build up a system similar to what is obtained in the former case as regards saline equilibrium. The following results were obtained by this method of procedure.

The numbers signify millionth gram molecules per c.c., and the rates, R, denote the number of millionth gram molecules of $\mathrm{ClO_3}$ decomposed per minute in each cc.

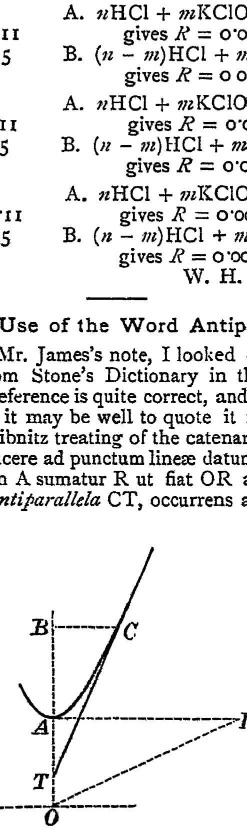
I. $n = 18 \times 65{\cdot}11$, $m = 6 \times 51{\cdot}5$	A. $n\mathrm{HCl} + m\mathrm{KClO_3}$ gives $R = 0{\cdot}0104$
	B. $(n - m)\mathrm{HCl} + m\mathrm{HClO_3} + m\mathrm{KCl}$ gives $R = 0\,0105$
II. $n = 15 \times 65{\cdot}11$, $m = 6 \times 51{\cdot}5$	A. $n\mathrm{HCl} + m\mathrm{KClO_3}$ gives $R = 0{\cdot}00554$
	B. $(n - m)\mathrm{HCl} + m\mathrm{HClO_3} + m\mathrm{KCl}$ gives $R = 0{\cdot}00555$
III. $n = 15 \times 65{\cdot}11$, $m = 2 \times 51{\cdot}5$	A. $n\mathrm{HCl} + m\mathrm{KClO_3}$ gives $R = 0{\cdot}00195$
	B. $(n - m)\mathrm{HCl} + m\mathrm{HClO_3} + m\mathrm{KCl}$ gives $R = 0{\cdot}00191$

Dover College. W. H. PENDLEBURY.

On the Use of the Word Antiparallel.

AFTER reading Mr. James's note, I looked out the reference quoted by him from Stone's Dictionary in the "Acta Eruditorum." Stone's reference is quite correct, and, as the passage is an interesting one, it may be well to quote it in full. It occurs in an article by Leibnitz treating of the catenary.

"Tangentem ducere ad punctum lineæ datum C ; in AR horizontali per verticem A sumatur R ut fiat OR æqualis OB datæ et ipsi OR ducta *antiparallela* CT, occurrens axi AO in T, erit tangens quæsita.



"*Antiparallelas* compendii causa hic voco ipsas OR et TC si ad parallelas AR et BC faciant non quidem eosdem angulos sed tamen, complemento sibi existentes ad rectum, ARO et BCT."

The following quotation is given in Murray's "New English

Dictionary," and is assigned to the year 1660:—"To take the opposite course and to provide our remedy *antiparallel* to their disease." Here it seems intended to convey the idea of "parallel and in the opposite sense."

In Barlow's "Mathematical Dictionary" (1814), the modern meaning is given, and the old error as to the ratios of the segments of the sides of the triangle is pointed out.

In Rees's "Cyclopædia" (1819) the modern meaning is given, but a remark is added that Leibnitz used the word in the sense explained above; as no reference is given, we cannot tell whether the writer meant that he habitually used it or only in the article on the catenary. E. M. LANGLEY.

Bedford.

A Surviving Tasmanian Aborigine.

IN your issue of November 14 (p. 43), you refer to the paper read by Mr. James Barnard before the Royal Society of Tasmania on a Mrs. Fanny Cochrane Smith, who lays claim to be the last surviving aboriginal Tasmanian. Since your note appeared, I have read a report of the paper published in the *Hobart Mercury* of September 10 last, and think my view on the claim may be of some interest to your readers.

Mr. Barnard states that he knew Mrs. Smith forty years ago when she was seventeen years of age, and that during the period which elapsed since then until she called upon him shortly before he wrote his paper, he had not known of her whereabouts. In favour of the claim I can only find that she has, with apparently one exception, always been referred to officially as a pure-bred aborigine, and that Parliament appears to have voted her grants on two occasions (in 1882 and in 1884) on account of her unique position.

The objections to the claim may be briefly summarized as follows:—

(1) From the meagre account given, it appears her hair and complexion are both that of half-castes, and we are not supplied with any other description of her features or stature or peculiarities so as to be able to judge on the question.

(2) Beyond the mere statement as to mutual recognition no evidence is given that the claimant is the same girl Mr. Barnard knew forty years ago at Oyster Cove, nor, indeed, is there anything to show that this woman is the child, or one of the children, referred to by Lieut. Friend in controverting Count Strzelecki's well-known views, which *quasi* fact forms the foundation for the claim.

(3) The woman herself is reported to have no recollection of witnessing, at the age of thirteen, a document sufficiently important to have impressed itself on her memory, and it is somewhat strange that this very document is said to describe her as a half-caste.

It would, no doubt, be interesting were it to be eventually proved that this woman Fanny is a pure-bred aborigine, but for the present Truganina must be considered the last survivor of her race. HY. LING ROTH.

Lightcliffe, November 23.

Brilliant Meteors.

THE brilliant meteor seen at Warwick School and in Cumberland I saw at Folkestone on November 4 a little before 8. It was travelling slowly from north-west to north, about 30° above, and parallel with, the horizon. After travelling some distance it appeared to partly explode, and then went a little farther and burst. At first it was a beautiful green colour, but after it had partly burst it was nearly white. I imagined its colour was through the haze there was in the sky. From what I saw I am certain it would have been a splendid sight had the atmosphere been clear. P. A. HARRIS.

Inchulva, Maidstone, November 27.

LAST night, in clouded moonlight, whilst walking here from Newton by the road over Little Dunnow, my attention was arrested by the glare of what must have been a very bright meteor, seen through clouds which formed a general covering. The quarter in which the light appeared was east by north, at an elevation of about 25°, and it lasted a second and a half. There appeared to be three centres of illumination, but these may have been only thinner portions of the clouds. The time, as nearly as I could get it by comparing my watch by telegraph at the village post office this morning, was 22h. 48m. 45s.

Slaidburn, Clitheroe, December 2. R. H. TIDDEMAN.

REPORT ON THE MAGNETICAL RESULTS OF THE VOYAGE OF H.M.S. "CHALLENGER."

IT will be remembered by readers of the "Narrative of the Voyage of H.M.S. *Challenger*," that Vol. II., published in 1882, contained a report of the magnetic observations made in that vessel in considerable detail. It has, however, been reserved to the present year for a full discussion of the *Challenger* observations and their bearing on our existing knowledge of terrestrial magnetism to be made, and the following is an abstract of the final Report about to be published in Vol. II., "Physics and Chemistry of the Voyage of H.M.S. *Challenger*."

The method of representing the values of the magnetic elements by curves of equal value has, since 1700, when Halley published his map of the declination, found general favour; for in succeeding years we find Mountain and Dodson, Churchman, Yeates, and Barlow, also published maps of the same magnetic element.

In 1819, Hansteen added maps of inclination to the declination for certain epochs, and in 1826 produced a chart of isodynamic lines, revised in 1832.

Following Hansteen, there appeared, in 1840, Gauss and Weber's atlas, the result of calculations from about eighty-four observations distributed over the world, presenting a remarkable approach to the truth, even when viewed in the light of our comparatively extended knowledge of the earth's magnetism in the present day. It may be observed that, if only a fresh magnetic survey of the regions south of 40° S. latitude were now made, a recalculation of the Gaussian constants might be undertaken promising important results.

Between 1868 and 1876 Sir E. Sabine's "Contributions to Magnetism" were read before the Royal Society, forming a series of papers on the magnetic survey of the globe for the epoch 1842·5. Although the maps accompanying these contributions serve as a point of departure for comparison with subsequent maps, an examination of them shows that in Africa and the North and South Pacific Oceans there were large blanks from want of observations

There remained, therefore, a large field for observation, and it will now be shown how largely the *Challenger* Expedition contributed to the filling up of these blanks, and added to our knowledge of the changes going on in the magnetic elements in places visited by previous observers.

The whole of the magnetical results have been embodied with others from every available source in four charts[1] of the magnetic elements, for the epoch 1880, which may prove acceptable to magneticians desirous of noting the changes in the magnetic elements since 1842·5.

The *Challenger* was not an ideal ship in which to conduct magnetic observations at sea, for she was seldom at rest from pitching and rolling, and although the errors in the observations caused by the horizontal component of the ship's magnetism were moderate, and could be eliminated by "swinging" the ship, those proceeding from the vertical component were large, and necessitated a frequent comparison with normal values on land. But by discussing fully a series of observations made in numerous places in both hemispheres where no trace of local magnetic disturbance could be found, the magnetic condition of the ship was readily determined for any period of the voyage. As a consequence of this, normal values of the magnetic elements could be obtained in the neighbourhood of places known or suspected of being affected by local magnetic disturbance, and the amount of such disturbance measured with considerable accuracy.

This method of detecting local magnetic disturbance,

[1] Note published with the "Report of the Scientific Results of the Voyage of H.M.S. *Challenger*," Physics and Chemistry, Vol. II., Part VI.

was applied to the solitary islands of the ocean visited by the *Challenger*, and the following are some of the principal results.

At Madeira there was a difference of $7\frac{1}{2}°$ in the observed inclination between observations made at 1 foot and $3\frac{1}{4}$ above the ground; and at Santa Cruz, Tenerife, the inclination was $2\frac{1}{2}°$ in excess of the normal observed in the ship.

It was at Bermuda, however, that the most remarkable results were obtained. For some years previously, observers in different parts of the group had obtained very different values of the declination, and our men-of-war when swinging for deviations of the compass had found constant errors for every direction of the ship's head which were peculiar to Bermuda. It could only, therefore, be by a properly equipped expedition like that of the *Challenger*, and systematic observation, that the immediate cause of all this local magnetic disturbance could be traced.

For this purpose the declination was observed at seventeen stations, the inclination at ten, and the intensity at seven. Combining these observations with others made by previous observers, it was found that between the Governor's house at Mount Langton and the lighthouse on Gibb's Hill, there is a disturbing magnetic focus attracting the north-seeking end of the needle with a force considerably in excess of that due to the position of Bermuda on the earth considered as a magnet. The normal values of the magnetic elements were obtained by swinging the ship at sea 15′ south of the green outside the dockyard. The difference between the observed declination at Clarence Cove and Barge Island was 5° 44′. The greatest difference in the inclination was 1° 47′, and in the vertical force + 0·314 (Brit. units).

Local magnetic disturbances were also noted at St. Vincent, Cape de Verde Islands, Tristan d'Acunha, Kerguelen Island, Sandwich Islands, Juan Fernandez, and Ascension, but not at St. Paul Rocks.

By applying the same method of obtaining normal values at sea, and observing on other adjacent solitary islands such as St. Helena, similar effects result, and the following general conclusions seem to be supported by fact with regard to local magnetic disturbance:—

(1) That in islands north of the magnetic equator, the north-seeking end of the needle is generally attracted vertically downwards, and horizontally towards the higher parts of the land; (2) south of the magnetic equator the opposite effects are observed, the north-seeking end of the needle being repelled: in both cases by an amount above that due to the position of the island on the earth considered as a magnet.

Interesting as these conclusions may possibly be from a scientific point of view, they are of real importance in practical navigation. Navigators have asserted that their compasses were disturbed when passing the land in certain parts of the world. We learn from the *Challenger* observations that within 5 feet from the soil the greatest magnetic disturbance did not exceed 3° in the declination and $2\frac{1}{2}°$ in the inclination. Remembering the law of magnetic attraction and repulsion, it is impossible that a compass in such case could be disturbed in a vessel passing the land at the ordinary distance. In point of fact, it has been shown that it is to submerged magnetic land comparatively near the ship's bottom that the disturbance of the compass is due. The remarkable instance at Cossack in North-West Australia may be cited in support of this conclusion. Thus in H.M.S. *Meda*, sailing on a line of transit of two objects on land for a quarter of an hour in 8 fathoms of water, it was found that the compass was steadily deflected 30°, no visible land being nearer than 3 miles.

Great as the gain must be to the navigator to be thus warned of a formidable danger in certain places, it also lays upon him the important duty of being on his guard against similar disturbances elsewhere, reporting any new discoveries as he would a rock or shoal.

Large as was the *Challenger's* contribution to the magnetic charts for 1880, it will be readily understood that it required considerable reinforcement from other sources, as their construction was dependent on observation alone. Every available observation between the years 1865–87 was utilized. Beyond the published sources of information on this subject may be mentioned the observations made on the east coast of Africa by the officers of H.M.S. *Nassau* in 1874–76, and on the west coast of Australia in 1885–86 by H.M.S. *Meda*. Also the sea observations between Australia and Cape Horn of the declination in H.M.SS. *Esk*, *Pearl*, and *Thalia*, between 1867–87, not forgetting those of the New Zealand Shipping Company's vessels in 1885–86.

To combine this twenty years' observation usefully, a somewhat extended knowledge of the distribution and amount of secular change became a necessity. Generally speaking, it is only at fixed observatories that this element of terrestrial magnetism is known with precision, for, as already shown, observations a few feet apart often give very different results. In the more frequented parts of the earth this secular change is approximately known, especially in the United States, where valuable work has been accomplished.

One great object of the voyage of the *Challenger* was to visit certain unfrequented positions where previous observers had been, rather than the beaten tracks. Thus Ross's position of 1840 on St. Paul Rocks was visited, and the secular change during thirty-three years obtained. Then Tristan d'Acunha, an important station situated in mid-ocean, rarely visited for magnetic purposes. At Kerguelen Island, another of Ross's positions, observations of all three principal magnetic elements were made, and the secular change found approximately.

In the Indian Ocean generally, north of 30° S., the secular change of the declination rarely exceeds 1′ annually, but at Kerguelen Island the westerly declination is increasing at least 5′ annually.

It was, however, from two positions on the homeward voyage that the most novel and remarkable values of the secular change were obtained—Sandy Point, Magellan Straits, and the Island of Ascension, with its adjacent waters.

At Sandy Point, with the horizontal force nearly stationary, and the declination decreasing 3′ annually, it was hardly suspected until 1876, when the *Challenger* visited the place, that the inclination was apparently changing 11′ annually. Comparing the *Challenger's* results by swinging near the Island of Ascension with Sabine of 1842·5, the following values of the secular change are obtained: declination increasing 8′ annually; south inclination increasing 14′.

From these results the notable fact is made evident, that the north-seeking end of the needle is found to be moving in opposite directions, downwards at Sandy Point, and more strongly upwards at Ascension. Extending the inquiry into the surrounding seas and countries, it was found that these opposite movements of the needle were not confined to the spots where they were discovered.

The author of this Report, after having discussed his collection of a large number of observations of the magnetic elements for all parts of the world—in many cases extending over several years—obtained approximate values of their secular change for the epoch 1840–80.

These several values were weighted according to their relative accuracy, and entered on maps against the places of observation. Lines of equal value were then drawn for each element, and the following general results obtained with regard to the movements of the north-seeking end of the needle.

1. *Declination*.—The principal lines of little or no change were found to take the course from St. John's,

Newfoundland, to the West Coast of Africa, near Cape de Verde, emerging near Cape Palmas, and then to Cape Town; thence curving upwards near Mauritius, downwards south of Cape Leeuwin, again upwards through Adelaide and Cape York to the vicinity of Hong Kong. A second line passed from Sitka through the western portion of the continent of North America, striking South America near Callao, then following the trend of the coast to a point near the western entrance to Magellan Strait.

The foci of maximum value of change were found: (1) between Scotland and Norway, change about 9′ annually, needle moving eastward; (2) on the east coast of Brazil, needle moving westward about 8′. Minor foci were also found: one near Kerguelen Island, the other in the South Pacific. Another focus apparently exists in Alaska. The general tendency was for the values of the change to decrease gradually from the foci to lines of no change.

2. *Inclination.*—Similarly to that of the declination, there are lines of no change, two principal foci of maximum secular change, but only one minor focus. The lines of no change are not so clearly defined as those for the declination, data being still wanting. The principal foci of maximum change in the inclination were found: (1) near the Gulf of Guinea, between Ascension and St. Thomé, which may be called the Guinea focus. Here the north-seeking end of the needle was moving *upwards* about 15′ annually. (2) in the latitude of Cape Horn, and about 80° W. long. This may be called the Cape Horn focus, and the annual change was 11′, needle being drawn *downwards*. It must be distinctly understood that both the positions and values of the change are only approximate, and only the general features in the angular movement of the freely suspended needle are to be accepted, as clearly shown by this investigation.

3. *Magnetic Intensity.*—In the horizontal force, the annual change (B.U.) was about −0·002 near Cape Horn, whilst between Valparaiso and Monte Video the focus of greatest change was about −0·017. Again, on the west coast of Portugal a focus of +0·009 (B.U.) occurred.

Turning to the vertical component of the earth's intensity, some remarkable results were observed. At the Cape Horn focus an annual change of 0·055 (B.U.) was found in the vertical force, the north-seeking end of the needle being drawn *downwards*, the change diminishing in value until the zero line extending from Callao across the American continent to the west coast between Bahia and Rio de Janeiro, and then taking a south-easterly course north of Tristan d'Acunha, was reached. Northward and eastward of this zero line there were found increasing values in the annual change in the *upward* vertical force acting on the north-seeking end of the needle until the Guinea focus was reached, where its full value was increasing 0·025 annually. From the Guinea focus to Northern Europe, Asia, and the Atlantic seaboard the change gradually decreased in amount. There were signs of minor movements in the north-seeking end of the needle in China, Mexico, and the United States.

One of the chief factors in the compilation of the previously mentioned maps of the three elements for the epoch 1880 were the observations taken in the *Challenger*, and these were reduced to the common epoch by means of the investigation of annual change to which reference has just been made.

It may be truly said that the *Challenger's* track was studded with magnetic observations. After successfully traversing the Atlantic Oceans in varying directions, the three magnetic elements were obtained by swinging, in probably the most southerly position since the days of Ross in the *Erebus* and *Terror*, in lat. 63° 30′ S., and long. 90° 47′ E. But the most valuable part of the contributions to terrestrial magnetism obtained in the *Challenger* were the observations made in the North and South Pacific. The route lay from Wellington, N.Z., to Tongatabu, and Fiji, from the Admiralty Islands to Japan, and thence in mid-ocean from nearly 40° N., through the Sandwich Islands and Tahiti to 40° S., nearly at right angles to the curves of equal magnetic inclination.

During the voyage much experience was gained as to the usefulness of the Fox circle as an instrument for use on board ship at sea, the general result being that valuable work may be done with it if frequently compared with the absolute instruments on land, and the instrument mounted on a gimbal stand prepared to withstand the vibrations caused by the engines of the vessel.

Although on the general question of the secular change of the magnetic elements much has been already written in this Report, there yet remain some important points which demand further discussion.

As to the causes of the secular change various hypotheses have been advanced. Thus in the early part of the last century, Halley considered the change was chiefly caused by a terella with two poles or foci of intensity rotating within and independently of the outer shell of the earth, which also possessed two foci of intensity, the axes of the two globes being inclined one to the other but having a common centre.

Again, Hansteen at the beginning of the present century concluded that there are four poles of attraction, and computed both the geographical positions and the probable period of the revolution of this dual system of poles or points of attraction round the terrestrial pole.

In later years Sabine considered the secular change to be caused by the progressive translation of the point of attraction at present in Northern Siberia, this point of attraction resulting from cosmical action. Walker also agreed with Sabine as to the cosmical origin of the change.

Later still, Balfour Stewart gave reasons for attributing the secular variation to the result of solar influence of a cumulative nature.

Keeping in view these hypotheses, and recalling the chief results of observation during recent years, how do they accord?

Observation generally points to the fixity of the magnetic poles—or two limited areas where the needle is vertical—in respect to the geographical poles. Again, in Siberia there is little or no apparent translation of the greatest point of attraction in that region, and the North American focus of intensity is probably at rest.

Thus the results of observation in recent years are not favourable to hypotheses founded on the translation of the poles or foci of magnetic intensity.

Let the terms blue and red magnetism be adopted, and the movements of the red, or north-seeking, end of the needle alone be considered.

The question arises, What have recent observations offered us instead? They tell us that near a line drawn from the North Cape of Norway across the Atlantic to Cape Horn lie some of the foci of greatest known secular change. It was also found that at the Cape Horn focus of vertical force the needle was moving downwards, or there was the equivalent to a blue pole of increasing power of attraction, the freely suspended needle being attracted towards it over an extended region around. At the Guinea focus there was the equivalent to a red pole of increasing power of repulsion, the freely suspended needle being repelled over an extended region of undefined limits. The action of these two poles apparently combine to produce a focus of considerable angular movement in the horizontal needle near Brazil.

In China there is a minor blue pole of increasing power attracting the needle over a large area.

With apparently small secular changes in Siberia, and the horizontal needle moving somewhat rapidly to the eastward at the focus of change in the declination in the German Ocean, and similarly to the westward in Alaska,

analogy points to the probability of there being a decrease in the vertical force in the high latitudes of North America, or the equivalent to a red pole of increasing power repelling the needle for a large area around it.

The variations in the vertical force at and about these poles or foci of attraction and repulsion at different epochs are not yet sufficiently determined, but if the hypothesis of translation be given up, it is not unreasonable to suppose that the secular changes in the declination and inclination are chiefly dependent upon changes in the relative power of these poles.

No satisfactory explanation has yet been given of the remarkable changes in the earth's magnetic force as measured on its surface, and suggestions are only possible in the present instance.

The voyage of the *Challenger* has shown that local magnetic disturbance is found in the solitary islands of the sea, although surrounded by apparently normal conditions, similar to that on the great continents. It has also been suggested that the magnetic portions of these islands causing the disturbance may possibly "have been raised to the earth's surface from the magnetized portion of the earth forming the source of magnetism," and tending to prove Airy's conclusion "that the source of magnetism lies deep."

In view, therefore, of past geological changes and those now in progress, it may fairly be conceived, not only that large changes have likewise occurred in the distribution of the magnetic portions of the earth appearing here and there on the surface and producing local magnetic disturbance, but that there are others of a more progressive character below the earth's surface which are only made manifest by the secular change observed in the magnetic elements. This conception with regard to secular change is not intended to exclude the view that solar influences may have a small share in producing the observed phenomena.

In conclusion, it may be remarked that they who would fully see the substantial gains to terrestrial magnetism which have been obtained by the voyage of the *Challenger* must refer to the original of this abstract Report, with its plates and charts of the magnetic elements. Subsequent research may add to, qualify, or reverse the conclusions drawn from the observations, but the observations will probably retain a long-abiding value to magneticians. E. W. CREAK.

ON THE SUPPOSED ENORMOUS SHOWERS OF METEORITES IN THE DESERT OF ATACAMA.

IT is now universally acknowledged both that meteorites come from outer space and that shooting-stars, whatever they are, have an extra-terrestrial origin. It is further asserted that a meteoritic fireball and a shooting-star are only varieties of one phenomenon. Indeed, after it is once granted that a meteoritic fireball is produced by the passage through the terrestrial atmosphere of a dense body entering it with planetary velocity from without, and that shooting-stars have an extra-terrestrial origin, it is a very fair assumption that a shooting-star is likewise a dense body rendered luminous during its atmospheric flight.

One great objection to this assertion is that, again and again, showers of hundreds of thousands of shooting-stars have taken place, during which no heavy body has been observed to reach the earth's surface. The only known case of the arrival of a meteorite during a shooting-star shower has been that of Mazapil, on November 27, 1885, and that single coincidence may possibly be the result of accident. A sufficient explanation of this difficulty, however, is to be found in the small size of the individuals which produce the appearance of a shooting-star shower. That the individuals are really minute is proved by the fact that, while the total mass of a large swarm, like that producing the November meteors, is so small that there is no perceptible influence on the motion of the planets, the number of separate individuals is almost infinite. It is established that the Leonid swarm must be hundreds of millions of miles in length, and some hundreds of thousands of miles in thickness; and in the densest part of the Bielid swarm, passed through in 1885, the average distance of the individuals from each other was about twenty miles.

Further, it is now acknowledged that comets are themselves meteoritic swarms, and Mr. Lockyer has lately brought forward spectroscopic evidence that the fixed stars and the nebulæ are similar to comets in their constitution.

The question therefore immediately presents itself, Is the size of a meteoritic shower, on reaching the earth's surface, ever comparable with that of a meteoritic swarm, as manifested by a shower of shooting-stars?

During the present century nearly 300 meteoritic falls on the earth's surface have been observed, and on only a single date, namely August 25, 1865, has there been observed a fall on two distant parts of the earth on the same day. On that date stones fell at Aumale in Algeria, and at Sherghotty in India; but as the times of fall differed by about eight hours, and the stones arrived from different directions, it is more than probable that the coincidence of date was accidental. Hence we must infer that a swarm of meteorites, as far as actual observation of tangible objects goes, far from being hundreds of millions of miles long, with individuals a few miles apart, is a comparatively small group, separated from its neighbours, if it has any, by a distance comparable with the earth's diameter.

The extent of surface over which meteoric stones have been picked up after some of the best known and most widely spread falls is given in the following list:—

Limerick, 3 miles long.
Mocs, 3 miles by 0·6 mile.
Butsura, 3 miles by 2 miles.
Pultusk, 5 miles by 1 mile.
L'Aigle, 6 miles by 2·5 miles.
Barbotan, 6 miles long.
West Liberty, 7 miles by 4 miles.
Stannern, 8 miles by 3 miles.
Knyahinya, 9 miles by 3 miles.
Weston, 10 miles long.
Hessle, 10 miles by 3 miles.
New Concord, 10 miles by 3 miles.
Castalia, 10 miles by 3 miles.
Khairpur, 16 miles by 3 miles.

As far as I have yet been able to ascertain, the greatest observed separation has been sixteen miles. In the case of Macao, Cold Bokkeveldt, and Pillistfer, wider spreads have been chronicled, but later information has shown the inaccuracy of the earlier statements.

As regards the meteoric irons, there have only been nine observed falls since the year 1751: in seven of them only a single mass was found; in the remaining two there was in each case a couple of masses, not more than a mile apart. There is thus no recorded instance of an observed shower of meteoric iron. The most convincing proof of the actuality of such showers is furnished by the masses which have been found in the Valley of Toluca, in Mexico; their existence had been chronicled as early as the year 1784, yet in 1856 it was still possible to collect as many as sixty-nine. When etched, they show the Widmanstätten figures in the most excellent way, and in their characters they are typical meteorites. Belonging, as they do, to a single type, they lead to the conviction that they are the result of a single shower. But the region over which the fall took place is not large; the

length of it is said to have been only about fourteen miles.

It is very probable, though not conclusively proved, that large meteoritic showers of stones, like those of Pultusk and L'Aigle, reach the terrestrial atmosphere as swarms of isolated bodies; still, we must have regard to the fact that a mass is much fractured during its passage through the air by reason of the enormous pressure and the violent change of temperature. In the case of the Butsura fall, for example, it was conclusively established that stones picked up some miles apart must originally have formed part of a stone disrupted during the atmospheric flight.

It is a question of a certain amount of interest as to whether there is any evidence of the actual fall of a shower of meteorites over a large extent of the earth's surface.

Such evidence has long been supposed to be furnished by the plentiful occurrence of meteorites in the Desert of Atacama, a term applied to that part of Western South America which lies between the towns of Copiapo and Cobija, about 330 miles distant from each other, and which extends inland as far as the Indian hamlet of Antofagasta, about 180 miles from the coast.

The generally received impression as to the occurrence of meteorites in this desert is well illustrated by the following statement of M. Darlu, of Valparaiso, read to the French Academy of Sciences in 1845:—

"For the last two years I have made observations of shooting-stars during the nights of November 11–November 15, without remarking a greater number than at other times. I was led to make these observations by the fact that in the Desert of Atacama, which begins at Copiapo, meteorites are met with at every step. I have heard, also, from one who is worthy of trust, that in the Argentine Republic, near Santiago del Estero, there is—so to say—a forest of enormous meteorites, the iron of which is employed by the inhabitants."

A study of the literature indicates that "the forest of enormous meteorites" near Santiago del Estero, understood by Darlu as significative of infinity of number, is really a free translation of a native statement "that there were several masses having the shape of huge trunks with deep roots," and that not more than four, or perhaps five, masses had really been seen in the Santiago locality at the time of Darlu's statement. There is a similar misunderstanding relative to the Atacama masses: it is clearly proved that, at a date long subsequent to 1845, the Desert was virtually untrodden and unexplored. In Darlu's time it was only crossed along definite tracks by Indians travelling between San Pedro de Atacama and Copiapo, and between the inland Antofagasta and the coast. In fact, it is established that the only Atacama meteorites then in circulation were all got from a single small area, three or four leagues in length, in the neighbourhood of Imilac, one of the few watering-places on the track between San Pedro and Copiapo.

Since that time the discovery of rich silver-mines in the centre of the Desert, and the working of the nitrate deposits, have led to vast changes; the Desert has been more or less closely examined, and other meteoritic masses have been found. Still, the number of meteorites yet discovered, distinct either in mineralogical characters or locality, is shown to be, at most, thirteen.

One of them, Lutschaunig, is distinct from all the rest as being a chondritic stone; a second, Vaca Muerta, likewise differs from all the others in that it consists of nickel-iron and stony matter, both in large proportion; a third, Imilac, is a nickel-iron with cavities, like those of a sponge, filled with olivine; a fourth, Copiapo, is a nickel-iron with irregularly disposed angular inclosures of troilite and stony matter; the remaining nine consist of nickel-iron, virtually free from silicates, some of them showing no Widmanstätten figures when etched, others showing excellent figures more or less differing in character.

Now, in every meteoritic shower yet observed, the individuals which have fallen simultaneously have been found to belong to a common type. Hence, it is reasonably certain that several distinct meteors are represented in the Desert, and that the above masses are the result of several falls; and this being accepted, the assertion of simultaneity of fall of two or more masses on the purely geographical ground that they have been found in the same Desert, can be allowed no great weight.

But have masses belonging to any one of the above types been found scattered over a part of the Desert so extensive as to indicate a meteoritic fall more widely spread than any of those actually observed? A critical examination of the cases in which such an enormous spread has been asserted proves that the evidence is quite unsatisfactory. The results may thus be summarized:—

(1) *Lutschaunig.*—This was a single stone.

(2) *Vaca Muerta.*—The masses were in great abundance distributed over a small area. But fragments undoubtedly belonging to this type have been brought from two other places far distant from the main locality. Very cogent evidence is brought forward to prove that the said fragments must have been *carried* to those places—the Jarquera Valley and Mejillones—from Vaca Muerta itself.

(3) *Imilac.*—An examination of all the known literature indicates that the whole of the fragments belonging to this type have been got from the immediate neighbourhood of Imilac. Caracoles, Potosi, and Campo de Pucará, from which specimens, belonging to this type, have been brought, are shown to be on regular lines of traffic starting from the Atacama coast. It is further shown that Imilac specimens were in great request, and were certainly carried to very distant places along such lines of traffic.

(4) *Copiapo.*—It is conclusively proved that the two localities, upwards of 400 miles apart, for meteoritic masses belonging to this type, result from a mere interchange of labels, and that all the masses probably came from a single place.

(5–13) There is no satisfactory evidence furnished by similarity of type for any of the meteoric irons being part of a widespread shower.

It is thus clear that the meteorites of the Desert of Atacama afford absolutely no proof that enormous meteoritic showers have ever reached the earth's surface.

The general dryness of the air of the Desert, and the rarity of rain, have been sufficient to ensure the preservation of masses which have fallen in the course of many centuries unto a time when an exploration of a large extent of the Desert has taken place.

That the meteoritic masses are far from being so plentiful as has been imagined is conclusively proved by the experience of Mr. George Hicks, one of the earliest explorers of the 23rd and 24th parallels; although much interested in their occurrence, he never found a mass himself, and he only obtained his first specimen after years of persevering inquiry from the Indians.

Detailed information relative to the Atacama meteorites, with a description and map of the Desert, will be found in the recently published number of the *Mineralogical Magazine.* L. F.

EARLY EGYPTIAN CIVILIZATION.

ALTHOUGH the paintings in the tombs of Memphis, of Beni Hasan, and of Thebes, have preserved to us the knowledge of much of the civilization of Egypt, yet hitherto we have handled but few examples of the im-

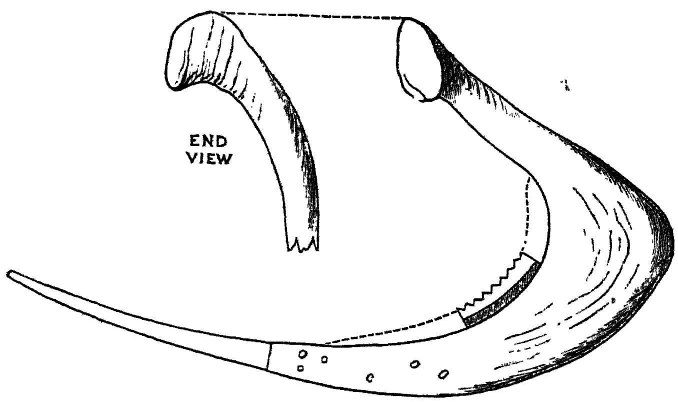
plements used, and those are mostly undated. Broadly speaking, the three sites just named represent respectively the Old Kingdom before 3400 B.C., the Middle Kingdom about 2600 B.C., and the New Kingdom from 1600 B.C.; and though debarred from scientific work in these richest districts of Egypt—owing to national jealousies—I have been fortunate enough to discover two small towns, each only occupied for a couple of centuries, which have thus revealed the works of the Middle and New Kingdoms with chronological exactness. Beside the Egyptian interest of these places, they are of prime importance for Mediterranean history, having been colonies of foreign workmen.

These towns are one on each side of the entrance to the Fayum province, fifty miles south of Cairo. The northern town, now called Kahun, was built for the workmen employed by Usertesen II., for his pyramid and temple, about 2600 B.C. The southern town, now called Gurob, was founded by Tahutmes III., and destroyed by Merenptah, thus lasting from about 1450 to 1190 B.C. Obtaining thus two sites of different ages, close together, we can be certain that all differences are due entirely to time and not to locality. The change in an interval of 1200 years is most marked. Of the pottery, scarcely a single type of form or material is alike in the two periods; of the many varieties of beads of stone and glazed ware, hardly one was continued; the metal tools are every one changed in form; and the use of flints had practically died out. For the first time we are able to trace the definite and decided changes in all the products of two ages so remote. The idea that Egypt was changeless is only due to our lack of knowledge; not only fashions changed—every few years in minor details—but radical rearrangements were made from age to age in the manufactures.

The twelfth dynasty town—Kahun—is the more important, and we will briefly note some of its products. Flint working was carried to a high pitch, the thin flat knives being flaked with much skill: but alloys of copper were also in use, and show ability in their casting and hammering, a thin bowl being wrought out of one piece. We find, then, flint and metal side by side, the flint being the commoner material, but yet influenced in its forms by the types of metal tools. Moreover, we now see the use of the numerous flint saws, formed of serrated flakes; many of them have black cement upon them, and one was found remaining in its socket in a wooden sickle (Fig. 1).

Beside hatchets, adzes, and chisels of bronze, we find needles, barbed and unbarbed fish-hooks, netting-needles, and knives of the straight-backed type. Among wooden tools are hoes, rakes, grain-scoops, a brick-mould, plasterers' floats, bow-drills, plummets, &c. Perhaps the most important of all is a fire-stick, on which five burnt holes

FIG. 1.



Wooden sickle with flint saw (twelfth dynasty).

remain where fire has been drilled by a rotating rod: the drilling was begun by hacking a groove in the side of the stick, down which the heated wood powder might run, until it caught alight. This shows, for the first time, how the Egyptians obtained fire: and familiar as they were with the bow-drill, they doubtless used it for the fire-stick. A very interesting point is the origin of the shoe from the sandal. Two sandal-shoes have been found; both with toe and heel straps, but with an upper of leather across the foot. Tops, tip-cats, clay toys, dolls with jointed limbs, and game boards, were all in use. Among a large number of papyri that I found are two wills, one of which is a recital of a will and a settlement, duly witnessed. The provisions show that the later law of Greek times was much the same in matters of descent as it was two thousand years earlier. On receiving family property the man settles it on his wife; she has a life interest in the dwellings, and may divide all the property among their children at her discretion. The man's official position he left to his son. A guardian was also appointed, excluding the eldest son from that duty. Some numerical notes concerning fractions are also found; and all these papyri are in course of study by Mr. F. L. Griffith.

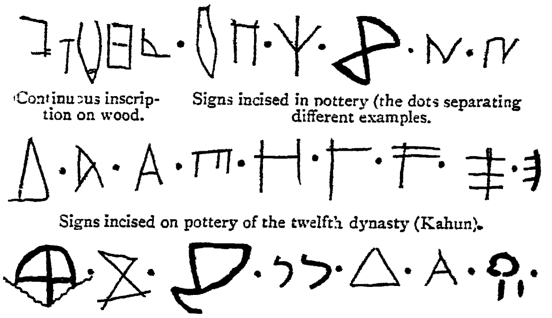
On turning to the later town—Gurob—of about 1300 B.C., we find that the art of flint working was lost; only a few rude leaf-shaped flakes (totally different from the earlier forms) and some little saw-flakes remain, and these are scarce. Thus we may date the fall of fine flint manufacture in Egypt to about 2000 B.C.; though rude flakes continued to be used till late Roman times, and more abundantly in poorer ages. Bronze tools were much modified; hatchets and chisels less finely formed, knives always double edged, fish-hooks not barbed, and punched metal rasps were brought in. Bronze working reached a high level in the making of two large pans, 14 and 9 inches across, exquisitely wrought with difficult curves, and so thin that they can be still bent in and out by the fingers. Glass ornaments were commonly used, though not found in the earlier town. The plain beads of fine blue, violet, &c., belong to about 1300 B.C.; while the coarser beads of black, yellow, green, brown, and white, with eye-patterns, are about a century later.

The presence of foreigners in both of these towns is shown by the weights discovered, which are—with scarcely an exception—of foreign standards, foreign forms, or foreign materials. A commercial intercourse must therefore have been kept up between these foreign colonies and the Mediterranean. Beside this evidence we find at Gurob the burials of one of the Tursha or Turseni (from Asia Minor), and a Hittite; foreign art is seen in a mirror handle with the Phœnician Venus, and a wooden figure of a Hittite; and foreign complexions are shown by the light hair found on some of the bodies. A very strong Mediterranean influence appears in the quantity of pottery

identical with the earliest styles found at Mykenæ, at Thera, and at Mytilene; and we are now able to date those stages of early culture in the Greek lands to 1300 B.C., a fixed point of the greatest value.

The most novel discovery of all is the presence of apparently alphabetic signs in use in both towns (Fig. 2), and by all the circumstances amply guaranteed to be of about 2500 B.C. and 1300 B.C. Our existing theories of alphabetic development require us to suppose that the Phœnician letters were established before 2000 B.C.; as the Egyptian writing from which De Rougé derived them, was extinct after that date; and the Cypriote syllabic signs must be older. Thus, though no known inscriptions can be placed before about 900 B.C., yet the forms must have started about the same period as that of the first of these towns, Kahun. The conditions that we find, therefore, of a great variety of signs in use, many of which have not survived, while others have drifted apart into many different alphabets, are just what might be expected at

FIG. 2.



Continuous inscription on wood. Signs incised in pottery (the dots separating different examples.

Signs incised on pottery of the twelfth dynasty (Kahun).

Signs on pottery of the eighteenth to nineteenth dynasty (Gurob).

these early times. The apparent connection of these signs with some of the mason's marks of Egypt suggests that they may have been adopted by the foreign workmen from their Egyptian fellow-labourers; and the very lack of literary education among such men would lead to their forming alphabets of their own from such materials. We have at least now obtained two well-defined stages, between the finished and segregated alphabets of the period of known inscriptions, of 900 B.C. downward, and the original elements of Egyptian hieroglyphs, hieratic, mason's marks, and perhaps Hittite and cuneiform characters, from which the alphabets were evolved. To discuss the historical descent of the signs, and to form a continuous theory of them, will need much discussion, and more materials. Meanwhile, my work will lie in the complete gathering in of what may still remain in these towns. A full account and drawings of every sign and every object of importance found this year will appear in a few months. W. M. FLINDERS PETRIE.

MR. STANLEY'S GEOGRAPHICAL DISCOVERIES.

THIS week an interesting letter from Mr. Stanley to Colonel Grant has been published. It is dated, "Villages of Batundu, Ituri River, Central Africa, September 8, 1888." Speaking of Lake Albert, Mr. Stanley says:—

"When on December 13, 1887, we sighted the lake, the southern part lay at our feet almost, like an immense map. We glanced rapidly over the grosser details—the lofty plateau walls of Unyoro to the east, and that of Baregga to the west, rising nearly 3000 feet above the silver water, and between the walls stretched a plain—seemingly very flat—grassy, with here and there a dark clump of brushwood—which as the plain trended south-westerly became a thin forest. The south-west edge of the lake seemed to be not more than six miles away from where we stood—by observation the second journey I fixed it at nine miles direct south-easterly from the place. This will make the terminus of the south-west corner at 1° 17′ N. lat. By prismatic compass the magnetic bearing of the south-east corner just south of Numba Falls was 137°, this will make it about 1° 11′ 30″ N. lat. A magnetic bearing of 148° taken from N. lat. 1° 25′ 30″ about exactly describes the line of shore running from the south-west corner of the lake to the south-east corner of the Albert. Baker fixed his position at N. lat. 1° 15′, if I recollect rightly. The centre of Mbakovia Terrace bears 121° 30′ magnetic from my first point of observation, this will make his Vacovia about 1° 15′ 45″, allowing 10° west variation.

"In trying to solve the problem of the infinity of Lake Albert as sketched by Baker, and finding that the lake terminus is only four miles south of where he stood to view it 'from a little hill,' and on 'a beautifully clear day,' one would almost feel justified in saying that he had never seen the lake. But his position of Vacovia proves that he actually was there, and the general correctness of his outline of the east coast from Vacovia to Magungo also proves that he navigated the lake. When we turn our faces north-east, we say that Baker has done exceedingly well, but, when we turn them southward, our senses in vain try to penetrate the mystery, because our eyes see not what Baker saw. When Gessi Pasha first sketched the lake after Baker, and reduced the immense lake to one about ninety miles long, my faith was in Baker, because Gessi could not resolve by astronomical observations the south end of the lake. When Mason Bey—an accomplished surveyor—in 1877 circumnavigated the lake, and corroborated Gessi, then I thought that perhaps Mason had met a grassy barrier or sandbank overgrown with sedge and ambatch, and had not reached the true beyond, because he admitted that he could not see very far from the deck of his steamer, my faith still rested in Baker; but now, with Lieutenant Stairs, of the Royal Engineers, Mr. Mounteney Jephson, Surgeon Parke, Emin Pasha, Captain Casati, I have looked with my own eyes upon the scene, and find that Baker has made an error. . . .

"I am somewhat surprised also at Baker's altitudes of Lake Albert, and the 'Blue Mountains,' and at the breadth attributed by him to the lake. The shore opposite Vacovia is ten and a quarter miles distant, not forty or fifty miles; the 'Blue Mountains' are nothing else but the west upland—the highest cone or hill being not above 6000 feet above the level of the sea, not 7000 or 8000 feet high. The altitude of Lake Albert by aneroid and boiling-point will not exceed 2350, not 2720, feet.

"And last of all, away to the south-west where he has sketched his 'infinite' stretch of lake, there rises, about forty miles from Vacovia, an immense snowy mountain—a solid square-browed mass with an almost level summit between two lofty ridges. If it were 'a beautifully clear day' he should have seen this, being nearer to it by thirteen geographical miles than I was."

Of the snowy Mountain, Mr. Stanley writes as follows:—

"My interest is greatly excited, as you may imagine, by the discovery of Ruwenzori—the Snowy Mountain—a possible rival of Kilimanjaro. Remember that we are in north latitude, and that this mountain must be near on the equator itself, that it is summer now, that we saw it in the latter part of May, and that the snow-line was about (estimate only) 1000 feet below the summit. Hence I conclude that it is not Mount Gordon Bennett, seen in December 1876 (though it may be so), which, the natives said, had only snow occasionally. At the time I saw the latter, there was no snow visible. It is a little further east, according to the position I gave it, than Ruwenzori.

"All the questions which this mountain naturally gives

rise to will be settled, I hope, by this Expedition before it returns to the sea. If at all near my line of march, its length, height, and local history will be ascertained. My young officers will like to climb to the summit, and I shall be glad to furnish them with every assistance."

At the time when this letter was written, Mr. Stanley was uncertain as to the destination of the streams flowing between "the two Muta Nzigés":—

"Many rivers will be found to issue from this curious land between the two Muta Nzigés. What rivers are they? Do they belong to the Nile or the Congo? There is no river going east or south-east from this section, except the Katonga and Kafur, and both must receive, if any, but a very small supply from Gordon Bennett and Ruwenzori. The new mountain must therefore be drained principally south and west. If south, the streams have connection with the Lake South; if west, the Semliki tributary of Lake Albert, and some river flowing to the Congo must receive the rest of its waters. Then, if the Lake South receives any considerable supply, the interest deepens. Does the lake discharge its surplus to the Nile or to the Congo? If to the former, then it will be of great interest to you, and you will have to admit that Lake Victoria is not the main source of the Nile; if to the Congo, then the lake will be the source of the River Lowwa or Coa, since it is the largest tributary to the Congo from the east between the Aruwimi and the Luama. For your comfort I will dare venture the opinion even now that the lake is the source of the Lowwa, though I know nothing positive of the matter. But I infer it, from the bold manner in which the Aruwimi trenches upon a domain that anyone would have imagined belonged to the Nile. It was only ten minutes' march between the head of one of its streams to the crest of the plateau whence we looked down upon the Albert Nyanza.

"From the mouth of the Aruwimi to the head of this stream is 390 geographical miles in a straight line. Well, next to the Aruwimi in size is the Lowwa River, and from the mouth of the Lowwa to the longitude of Ugampaka post in a direct line is only 240 geographical miles."

NOTES.

THE Gilbert Club, to which we referred last week, was formally founded on Thursday, November 28. The following officers were appointed at the first general meeting:—President, Sir William Thomson. Vice-Presidents: Lord Rayleigh, Prof. D. E. Hughes, Prof. Reinold, Mr. Jonathan Hutchinson (President of the Royal College of Surgeons), Dr. B. W. Richardson, and Mr. H. Laver, of Colchester. Mr. Latimer Clark was elected Treasurer, and Mr. Conrad Cooke, Prof. R. Meldola, and Prof. S. P. Thompson, Hon. Secretaries. The resolution finally adopted by the meeting was:—"That the objects of the Gilbert Club be as follows:—(1) To produce and issue an English translation of 'De Magnete' in the manner of the folio edition of 1600. (2) To arrange hereafter for the tercentenary celebration of the publication of 'De Magnete' in the year 1900. (3) To promote inquiries into the personal history, life, works, and writings of Dr. Gilbert. (4) To have power, after the completion of the English edition of 'De Magnete,' to undertake the reproduction of other early works on electricity and magnetism, provided at such date a majority of the members of the Club so desire." At the time of the inaugural meeting eighty-seven members had joined the Club.

PROF. J. BRYCE's speech (read by Prof. Holland) at the presentation of Mr. A. R. Wallace for the degree of D.C.L., *honoris causâ*, at Oxford, on November 26, was one of unusual interest. We may note especially the very masterly way in which the doctrine of the survival of the fittest was expressed. After describing Mr. Wallace's travels in Brazilian forests, and among the islands, "quæ ultra Chersonesum aureum soli nimium propinque subjacent," the speech referred to his discovery of the theory according to which new species are evolved, which was shortly stated as, "ea corpora vigere magis prolemque ex iis lætiorem surgere quæ ipsa nescio quo pacto natura vitæ periculis subeundis aptissima creaverit: sic stirpem a cæteris stirpibus dissimilem et in dies longius discrepantem propagari." The contemporaneous discovery of natural selection by Charles Darwin, and his cordial recognition of Mr. Wallace's merits, were mentioned: "tanta et in hoc et in illo inerat animi nobilitas veritatis quam gloriæ propriæ studiosior." Reference was made to Mr. Wallace's various writings.

WE regret to announce the sudden death of Dr. W. R. McNab. He died at his residence in Dublin on Tuesday morning, the 3rd inst. Dr. McNab was Professor of Botany in the Royal College of Science, Dublin, having succeeded Prof. Thiselton Dyer, F.R.S. He was also Scientific Superintendent and Referee to the Royal Botanic Gardens, Glasnevin, under the Science and Art Department. He appears to have been in his usual health on Monday, and on St. Andrew's Day (Saturday) took an energetic part in the meeting and banquet held by the Scotch residents in Dublin.

THE *Colonies and India* reports the death, in Melbourne, of Mr. Robert Brough Smyth, who was for sixteen years Secretary of Mines in Victoria. He was well known in Australia for his contributions, especially on geological questions, to scientific literature.

THE new Natural Science Museum of Berlin was opened on Monday. The Berlin Correspondent of the *Standard*, describing the proceedings, says that the ceremony was striking. A handsome tent, surmounted by an imperial crown, was erected for the Emperor and Empress, who were present with the Princess Frederick Charles, Prince Alexander, the Hereditary Prince and Princess of Meiningen, and a brilliant suite. Nearly all the Ministers, including Count Bismarck, who has just returned from Friedrichsruh, and the Minister of War, were in attendance. Count Waldersee, representatives of the Academy of Art, and the Professors of the University, were also present. Dr. von Gossler, Minister of Education, delivered an eloquent address, in which he mentioned that the collections were founded a hundred years ago, and expressed the hope that both science and the State would derive equal benefit from the new institution. Prof. Beyrich, the first Curator of the Museum, pledged himself to keep abreast with the progress of science. Their Majesties were conducted through the building by the keepers of the various collections.

THE Paris Museum of Natural History is about to elect a successor to M. Chevreul in the Chair of Chemistry.

AT the general monthly meeting of the Royal Institution, on December 2, the managers reported that they had reappointed Prof. James Dewar, F.R.S., as Fullerian Professor of Chemistry.

THE Academy of Sciences of Vienna has appointed Prof. G. Niemann, of Vienna, and Major Steffan, of Cassel, to be present as impartial witnesses at the excavations at Hissarlik, begun, on November 25, under the direction of Dr. H. Schliemann and Dr. W. Dörpfeld. Captain Ernst Bötticher, who has often called in question the utility of Dr. Schliemann's archæological investigations, has been requested to take part in the excavations.

MR. HUGH G. BARCLAY, in his Report as to the fund for the preservation of birds in the Farne Islands, says he has every reason to believe that the birds were very well protected this season. He visited the islands twice, and each time he satisfied himself, by his own personal investigations, that the birds had

not been unduly disturbed. Last year, at the request of the authorities, he allowed some young birds to be taken from the islands for the purpose of being placed on the lake at St. James's Park, London. The following is an extract from a letter he lately received from Mr. Rilly, the bird-keeper there:—"The only birds alive now of those brought from the Farne Islands are the cormorants, which are thriving. The puffins all died during the first three months. The guillemots lived somewhat longer, the death of the last one being the result of an accident. The one kittiwake also died by an accident. The terns died during the severe frost, being apparently unable to get about on the ice, their tail and wings collected the ice; I suppose on account of their being pinioned and not being able to use their wings freely."

THE Council of the Dundee and District Association for the Promotion of Technical and Commercial Education has issued its first Annual Report, and is able to give a very good account of the results it has achieved. With regard to the future work of the Association, the Council suggests that workshop instruction for lads engaged at unskilled work in factories and during the day should be established in connection with the evening classes of the School Board. It also proposes the drafting of additional courses of instruction, especially in painting, decoration, and pattern designing, and the encouraging of higher classes in these subjects. In this connection the Council appropriately refers to the fact that in 1884 the Technical Instruction Commissioners reported that "the crowded schools of drawing, modelling, carving, and painting, maintained at the expense of the municipalities of Paris, Lyons, Brussels, and other cities—absolutely gratuitous and open to all comers, well lighted, furnished with the best models, and under the care of teachers full of enthusiasm—stimulate those manufactures and crafts in which the fine arts play an important part to a degree which is without parallel in this country."

A SERIES of questions on the effects of London fogs on cultivated plants has been issued by the scientific committee of the Royal Horticultural Society. The experience of the current season only is to be utilized.

A SPECIMEN of the *Rorqual musculus* has just come ashore on the coast of the Médoc district. Dr. Beauregard, *aide-naturaliste* at the Paris Museum, went to the spot to examine this interesting cetacean. Unfortunately, the brain was already in a state of decomposition, but the breasts and ears were dissected off for complete examination. The animal was 14 metres long, and 6 metres in circumference at the thickest part of the body.

PROF. CHAUVEAU has lately published in the *Archives de Pathologie Expérimentale* a contribution to the study of "transformism" in microbiology. His researches relate to *Bacillus anthracis*, and show that by experimental means various important biological alterations may be obtained.

PROF. MARSHALL WARD is about to deliver, at th eCity and Guilds of London Institute, a course of six lectures on timber, its nature, varieties, uses, and diseases. The lectures will be given on Monday and Thursday evenings, at 7.30 (December 12, 16, and 19, and January 23, 27, and 30). The object of the course is to explain as simply and clearly as possible, with the aid of numerous lantern illustrations, the nature, properties, varieties, and uses of the ordinary timbers used in construction, and to give an intelligible account of dry-rot, and allied diseases of timber.

THE second series of lectures given by the Sunday Lecture Society will begin on Sunday afternoon, December 8, in St. George's Hall, Langham Place, at 4 p.m., when Mr. W. Lant Carpenter, B.Sc., will lecture on "The Wonders of the Yellowstone Park—a Personal Narrative," with oxy-hydrogen lantern illustrations from the lecturer's own camera. Lectures will also be given by Commander V. L. Cameron, R.N., Mr. J. F. Blake, Mr. Henry Blackburn, Mr. Wilmott Dixon, Mr. Stanton Coit, and Mr. Eric S. Bruce.

THE annual general meeting of the Institution of Electrical Engineers will be held at the Institution of Civil Engineers, 25 Great George Street, Westminster, on Thursday, December 12, at 8 o'clock in the evening, for the reception of the annual report of the Council, and for the election of Council and Officers for the year 1890. The following paper will be further discussed: "Electric Engineering in America," by Mr. G. L. Addenbrooke.

IT is stated that a scheme is on foot for establishing a Natural History Society in the Punjaub. It is to be hoped that it will be successful, and that the Society will flourish as other Indian scientific societies are doing.

IN the introductory lecture to the agricultural class at the University of Edinburgh, delivered at the opening of the present session, Prof. Wallace chose as his subject some aspects of Australasian agriculture. In this lecture, which has now been printed, Prof. Wallace urges that sheep farmers in this country will shortly feel the effects of rivalry with the flock masters of Australia. There are 100,000,000 sheep in Australia, mostly merinos, which are not, by the way, a flesh-yielding but a wool-giving race. Prof. Wallace hazards the opinion, by a very easy process of arithmetic, that, before many years have passed, Australia will be possessed of over 200,000,000. He makes, also, the astonishing statement that merino mutton is equal in flavour and texture to our best Highland, Welsh, or South Down mutton. Upon these two assumptions, for they are nothing more, he foretells calamities to the meat producers of this country, which he, it is to be hoped, will not live to see.

A STALACTITE cave has been discovered in Ascheloh, near Halle, in Westphalia; it is reported to be more than 100 metres long.

A SHARP shock of earthquake was felt at Oran, Algeria, on November 27, at 3 p.m. It lasted ten seconds, the oscillations being from east to west.

ACCORDING to a telegram sent through Reuter's agency from Belgrade on December 2, violent shocks of earthquake, accompanied by loud subterranean rumblings, were felt on Sunday afternoon at Kregugewatz, Jagodina, and Kupsia. The disturbance generally travelled from east to west, but some of the shocks moved from north to south.

MR. H. C. RUSSELL, Government Astronomer of New South Wales, has published the results of meteorological observations made in that colony during 1887. The number of reporting stations is now 862, being 94 more than in 1886, the increase being almost wholly in rain stations. The arrangement of the tables, which give the most important data for each station separately, is the same as in previous years; but there are also two new tables giving the mean maximum and minimum temperature at Sydney for each month from 1856 to 1887. The mean temperature of the whole colony for the last seventeen years is 61°·2. At Sydney the mean for thirty years is 62°·7. The diagrams appended to the volume give a good idea of the weather conditions at Sydney, and clearly exhibit the peculiarities of certain periods, such as the very short winter of 1873, and the long one of 1874, also the long summer of 1877–78, with four months of hot weather, and the short summer of 1886–87, when there was only one month of hot weather. In 1878 the lowest winter temperature occurred in June, and in 1872 in August. A comparison is made of the rainfall at the principal places in the various colonies. The contrast between the amount at Brisbane and Sydney and that at Melbourne is very striking. At the former places as much rain sometimes

falls in one month as would make a year's rainfall at Melbourne. At Sydney the least annual rainfall on record is 21·48 inches, and the greatest 82·81 inches. The question of evaporation continues to receive considerable attention; the tabular results are published, with the rain and river results, in a separate volume.

THE Meteorological Report of the Straits Settlements has been published for the year 1888, being the fifth year in which meteorological observations in the colony have been made the subject of a general systematic report. The temperature of the air ranged between 67°·2 and 96°, and solar radiation varied from 81° to 179°; the lowest temperature on the grass was 61°. Rainfall observations were received from forty-one stations. The annual amount differs considerably in the various provinces, the mean of the stations ranging from 65·6 inches in Singapore, to 111·7 inches in Penang, and 123·2 inches in Province Wellesley. The greatest fall in twenty-four hours, was 12 inches at Bertam, Province Wellesley, on October 21. The Report also contains a tabular statement of annual and monthly rainfall at Singapore since 1869, and diagrams of annual rainfall and other elements since 1870, at the same place.

THE International Commission for the scientific investigation of the Lake of Constance have nearly finished their task, which consisted of drawing a new and comprehensive map on a scale of 1 : 25,000; investigating the currents, density, temperatures, and chemical composition of the water; and minutely describing the flora and fauna of the lake. A full account will be issued when the researches are complete.

WE have received the latest instalment (pp. 321–84) of vol. xvi. of the Proceedings of the Royal Society of Edinburgh, session 1888–89. It contains:—The solubility of carbonate of lime in fresh and sea water, by W. S. Anderson, chemist at Marine Station, Granton (continued); secretion of carbonate of lime by animals, part ii., by Robert Irvine and Dr. G. Sims Woodhead; theoretical description of a new "azimuth diagram," by Captain Patrick Weir, communicated by Sir William Thomson; note on Captain Weir's paper, by Prof. Tait; on the coagulation of egg and serum albumen, vitellin, and serum globulin, by heat, by Dr. John Berry Haycraft and Dr. C. W. Duggan.

THE fourteenth part of Cassell's "New Popular Educator" has been published. It includes a clearly printed map of the world.

AT a recent meeting of the Bombay Anthropological Society, Mr. W. E. Sinclair, of the Civil Service, read a paper on flint remains in the Kolaba district. Referring to a collection belonging to the Society made in the Ghar Hills, near Sukker, on the Indus, Mr. Sinclair said that these hills were evidently a sort of "Black Country" to the flint-using races. Cones and flakes can be got there literally by the hundredweight. There is no historical evidence of the use of such things in India proper. On the contrary, all historical evidence points to the conclusion that India was one of the first countries to use iron, if not the very first. Amongst the wildest forest tribes to-day the use of stone does not go beyond weighting a fishing-line or bird arrow with a pebble; and although stone spindle-weights are still used on the coast, these are no more barbarous than the stones in an English mill. These cones of flint are covered with long grooves of a curved section; and the flakes show each one face corresponding to such a groove, which shows that they have been struck off such cones. The cones themselves have a peculiar typical form, and the art of producing flake or cone is one lost in the India of to-day. Where a flint shows that peculiar groove, there is good reason to assume that it was made before iron was known in India. On all the agates and chalcedonies in the Kolaba collection there are the same strange grooves, the same long blade-like flakes matching them, as in Sind or in England or France; and we are, in fact, in presence of a lost art, for which there has been no occasion from the time that iron came into common use. That was a long time ago in India. Steel—and very good steel, too—must have been for many generations in the hands of the ancient inhabitants of the Konkan when the first cave temples were hewn—at least 2000 years ago. On the other hand, the position of the flakes, both in Sind and in Kolaba, shows that they belong to a very recent geological period. The Kolaba specimens, except one or two, come from the surface of the lacustrine gravels abundant in the valleys of the Konkan. All search for them in places where sections of these gravels are exposed has hitherto been fruitless, and the few water-worn specimens found came out of a river bed. They most commonly occur at places where fresh water is to be had near an estuary.

THE additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus rhesus* ♀) from India, presented by Colonel J. D. C. Ferrell; two Common Marmosets (*Hapale jacchus*) from South-East Brazil, presented by Mr. Charles Petrzywaski; an Arctic Fox (*Canis lagopus* ♀) from Siberia, presented by Mr. Stuart N. Corlett; a Corn Crake (*Crex pratensis*) from Essex, presented by Mr. Bibby; four Common Snakes (*Tropidonotus natrix*), British, presented by the London, Chatham, and Dover Railway; a European Bison (*Bison bonasus* ♂) from Central Europe, deposited; a Stanleyan Chevrotain (*Tragulus stanleyanus*) from Ceylon, a Prevost's Squirrel (*Sciurus prevosti* ♂) from Malacca, a Common Roe (*Capreolus capræa* ♂), European, a White-faced Tree Duck (*Dendrocygna viduata*) from Brazil, four Black-necked Swans (*Cygnus nigricollis*) from Antarctic America, a Curlew (*Numenius arquata*), British, two Indian Cobras (*Naia tripudians*) from India, an Annulated Snake (*Leptodira annulata*) from Panama, a Hawk's-billed Turtle (*Chelone imbricata*) from the East Indies, purchased; two Crested Pigeons (*Ocyphaps lophotes*) bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at Greenwich at 10 p.m., December 5 = 2h. 59m. 33s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G. C. 648	—	—	3 12 31	+40 7
(2) D M. + 3° 410 ...	7	Yellowish-red.	2 51 19	+ 4 3
(3) γ Persei	3	White.	2 56 48	+53 4
(4) ι Cassiopeiæ	4	Bluish-white.	2 20 0	+66 54
(5) D.M. + 57° 702 ...	8	Red.	3 2 57	+57 29
(6) R Persei	Var.	Reddish.	3 23 3	+35 18
(7) T Geminorum ...	Var.	—	42 42	+24 0

Remarks.

(1) The General Catalogue description of this nebula is as follows:—Pretty bright, pretty small, round, brighter in the middle. The spectrum has not yet been recorded.

(2) This is a star of Group II., in which Dunér records the bands 2–8, and states that the bands 2 and 3 are especially well developed. This latter fact indicates that the star is well advanced, and it accordingly falls in a late species (13) of the group. As I have before pointed out with reference to similar stars, absorption *lines* of metallic substances, and possibly of hydrogen, may be expected at this stage, and it is important to note the presence or absence of these, as they will probably form a connecting link between the stars of this group and the slightly hotter stars of Group III. The intensity of the bright carbon fluting near *b*, as compared with its appearance in other stars of the group, will be an additional check in placing it in position on the "temperature curve."

(3) This is classed with stars of the solar type by Gothard, but there is not sufficient detail in his description of the spectrum to enable us to say whether it be Group III. or V. Further observations with special reference to this point are therefore required (for criteria, see p. 20). Gothard's statement as to the colour of the star should be checked, as most of the stars of Groups III. and V. are yellowish. The stars which are not far removed from Group IV., on either side, are the whitest.

(4) This is a star of Group IV., and the usual observations are suggested.

(5) This is a very fine example of the stars of Group VI., showing the subsidiary bands 4 and 5. The band 6 (λ 564) appears to be most subject to variation in the different stars of the group as described by Dunér, in some cases being wide and pale, and in others wide and dark. As this may subsequently form the basis of a temperature classification, the character of the band in the star under consideration should be carefully noted. The presence or absence of lines in the spectrum should also be recorded. [Dunér's notation for the bands in the spectrum of stars of Group VI. is as follows:—(1) 656, (2) 621, (3) 604·8, (4) 589·8, (5) 576·0, (6) 563·3, (7) 551, (8) 528·3, (9) 516·3, (10) 472·7. (6), (9), and (10) are the dark flutings of carbon.]

(6) The period of this variable is given by Gore as 210 days, and the magnitudes at maximum and minimum as 7·7–9·2 and 12·5 respectively. The spectrum has not yet been recorded. The maximum will occur on December 15.

(7) This variable has a period of 288·1 days, the next maximum occurring on December 14. The magnitude at maximum is given by Gore as 8·1–8·7, and that at minimum as $<$ 13. It is still doubtful whether the star belongs to Group II. or to Group VI., and the approaching maximum may afford an opportunity of settling the question. A. FOWLER.

SUN-SPOT OF JUNE, JULY, AND AUGUST, 1889.—The Memoir of the Società degli Spettroscopisti Italiani for October contains a series of observations by Prof. Riccò of this spot. The latitude of the spot from its appearance on June 16 and during the first semi-rotation, varied between the limits $-5^{\circ}\cdot 9$ and $-7^{\circ}\cdot 5$. At the second appearance, the variation was between $-7^{\circ}\cdot 5$ and $-10^{\circ}\cdot 8$, whilst at the third appearance, in August, the limiting latitudes were $-8^{\circ}\cdot 5$ and -10°.

The group of spots that appeared on June 30 was found to have a latitude as high as -41°. The following day, however, the latitude was found to be $-40\frac{1}{2}^{\circ}$, and on July 2 the group disappeared.

Prof. Spörer, in a communication to Prof. Riccò, notes that the following bright lines were measured at Potsdam on June 28 in a prominence that appeared as the above large spot was disappearing over the sun's edge.

Wave-length.		Origin.	Wave-length.		Origin.
672·6	...	Calcium	558·8	...	Calcium
671·6	...	Calcium	531·6	...	Coronal line
C	...	Hydrogen	526·9	...	Calcium
649·2	...	Calcium	518·8	...	Calcium
616·2	...	Calcium	b_1	...	Magnesium
D_1	...	Sodium	b_2	...	Magnesium
D_2	...	Sodium	b_3	...	Magnesium
D_3	...	?			

PHOTOGRAPHIC STAR SPECTRA.—As a portion of the Henry Draper memorial, the spectra of stars are being photographed at Chosica in Peru. Of the photographs that have been received at Harvard College, Prof. Pickering notes (*Astr. Nachr.*, No. 2934) several have similar spectra to the "bright line" stars in Cygnus. The hydrogen line F is bright in θ Muscæ, the same as in γ Cassiopeiæ, and the presence of bright hydrogen lines in η Argûs and R Hydræ is also confirmed by the photographs.

Numerous photographs have been taken at Harvard College of the spectra of the stars in the Pleiades, and an examination of them shows that the hydrogen line F in the spectrum of Pleione D.M. + 23° 558, consists of a narrow bright line superposed on a broader dark line. The other hydrogen lines, especially that near G, show some indications of a similar effect.

With respect to this, Prof. Pickering observes that an interesting analogy between the Pleiades and θ Orionis appears in the fact that in both cases extensive nebulosities surround stars with bright lines in their spectra.

COMET BROOKS (*d* 1889, JULY 6).—The following elements and ephemeris have been computed by Dr. Knopf from observations made at Mount Hamilton, July 8; Dresden, August 25; and Vienna, October 24:—

$$T = \text{September } 29{\cdot}7436 \text{ Berlin Mean Time.}$$

$$\left.\begin{aligned} \omega &= 343^{\circ}\ 18'\ 56''{\cdot}5 \\ \Omega &= 17\ \ 58\ 29{\cdot}6 \\ \iota &= 6\ \ \ 3\ 59{\cdot}6 \end{aligned}\right\} \text{Mean Eq. } 1889{\cdot}0.$$

$$\begin{aligned} \phi &= 28\ \ \ 4\ 13{\cdot}3 \\ \mu &= 501''{\cdot}8156 \\ U &= 7{\cdot}071 \text{ years.} \end{aligned}$$

Ephemeris for Berlin Midnight.

1889.	R.A. h. m. s.	Decl. ° ′	1889.	R.A. h. m. s.	Decl. ° ′
Dec. 7 ...	0 7 58 ...	+ 2 48·1	Dec. 19 ...	0 22 54 ...	+ 4 55·2
8 ...	9 7 ...	2 58·4	20 ...	24 15 ...	5 6·1
9 ...	10 17 ...	3 8·8	21 ...	25 36 ...	5 17·0
10 ...	11 28 ...	3 19·2	22 ...	26 58 ...	5 27·9
11 ...	12 41 ...	3 29·7	23 ...	28 21 ...	5 38·9
12 ...	13 55 ...	3 40 2	24 ...	29 45 ...	5 49·9
13 ...	15 9 ...	3 50·8	25 ...	31 9 ...	9 0·9
14 ...	16 24 ...	4 1·4	26 ...	32 34 ...	6 12·0
15 ...	17 40 ...	4 12·1	27 ...	34 1 ...	6 23·1
16 ...	18 57 ...	4 22·8	28 ...	35 28 ...	6 34·2
17 ...	20 15 ...	4 33 6	29 ...	36 55 ...	6 45·4
18 ...	21 34 ...	4 44·4	30 ...	38 23 ...	6 56·5
19 ...	21 54 ...	4 55·2	31 ...	39 52 ...	7 7·7

Mr. Chandler notes (*Astr. Jour.* No. 204) that the result of an inquiry into the corrected elements of this comet is extremely interesting. The descending node of the comet's orbit upon that of Jupiter lies at 185°·5 long., Jupiter's aphelion at 191°, and the comet's aphelion at 183°. The aphelion distances are 5·4541 and 5·3992 respectively, the mutual inclination of the orbits is 3°, and the orbital velocities nearly the same; so that when both bodies happen to be near this region they will remain together many months.

COMET SWIFT (*f* 1889, NOVEMBER 17).—The following elements and ephemeris are given by Dr. Zelbr in Circular No. 69, issued by the Vienna Academy of Sciences, November 25, 1889, and have been computed from observations made at Rochester, November 17; Vienna and Palermo, November 20; and at Vienna, November 22:—

$$T = 1889 \text{ December } 10{\cdot}5665 \text{ Berlin Mean Time.}$$

$$\left.\begin{aligned} \Omega &= 309^{\circ}\ 51'\ 12'' \\ \omega &= 109\ \ 24\ \ 70 \\ \iota &= 7\ \ 14\ \ 1 \end{aligned}\right\} \text{Mean Eq. } 1889{\cdot}0.$$

$$\log q = 0{\cdot}07554$$

$$\Delta\lambda \cos\beta = +132'' \ \ldots\ \Delta\beta = -14''.$$

Ephemeris for Berlin midnight.

1889.	R.A. h. m. s.	Decl. ° ′	Log Δ.	Log r.	Brightness.
Dec. 7 ...	23 41 56 ...	+18 32·4 ...	9·6509 ...	0·0759 ...	1·29
11 ...	23 58 44 ...	20 2·7 ...	9·6457 ...	0·0756 ...	1·32

The brightness at discovery has been taken as unity.

S CASSIOPEIÆ.—The Rev. T. E. Espin, examining the spectrum of this star on November 27, found that it resembled in appearance that of R Andromedæ, the bright F line blazing out upon the background of the continuous spectrum, and being plainly visible even with the least dispersion used. The star is not included by Dunér in his classical work, "Les Étoiles à Spectres de la Troisième Classe," but its general spectrum is apparently of that type—Group II. of Mr. Lockyer's classification. Mr. Espin adds that "the yellow is brilliant, suggesting (bright) lines, but the star is at present too faint to be sure."

The star is a variable of very long period, 607·5 days; the next expected maximum falls on December 26, so that it may show some further and interesting developments during the next three weeks. Chandler, however, records his suspicion that the period is shortening, so that the actual maximum may be very close at hand. The maximum brightness varies from 6 7 mag. to 8·6. Mr. Espin estimated it as 7·8 on the night of observation. Place for 1890: R.A. 1h. 11m. 34s.; Decl. 72° 1′·9 N.

THE ANNIVERSARY MEETING OF THE ROYAL SOCIETY.

ON Saturday last, St. Andrew's Day, the Royal Society held its anniversary meeting. The President read the anniversary address, a copy of which has not yet reached us. The medals were then presented as follows: the Copley Medal to the Rev. Dr. Salmon (received by Sir R. S. Ball); the Davy Medal to Dr. Perkin; a Royal Medal to Dr. Gaskell; and a Royal Medal to Prof. Thorpe. The Society next proceeded to elect the Officers and Council for the ensuing year. The selected names we have already published.

In the evening the Fellows and their friends dined together at the Whitehall Rooms, Hôtel Métropole, the President in the chair. Over two hundred Fellows and guests were present.

The toast of "The Royal Society" was proposed by the Speaker of the House of Commons. He said:—Sir George Stokes and Gentlemen,—If I thought the audience whom I have the honour to address, took the same view as I do of my own want of qualifications for proposing this toast, I think I should at once sit down; but it is because I trust to your generous forbearance for a few moments that I ask you to allow me to propose a toast which needs no advocacy of mine, the toast of the Royal Society. I suppose the reason why your President has selected me to propose this toast is owing to the fact of the official position that I hold in the House of Commons, and also partly owing to the fact that the holder of one chair has been willing to pay a compliment to the holder of another. There are very few members of the House of Commons, I believe, who are entitled to put three letters to their name to indicate membership of your Society. I omit those Privy Councillors who, I believe, by virtue of their office, have a claim to be looked upon as members of this Society. I am speaking now of the strictly scientific men, and I believe I could number the strictly scientific members of the House of Commons who are members of the Royal Society on the fingers of one hand. But I may say that those members of the House of Commons make up for their numerical weakness by the qualities they display, the high place they have filled, by their pre-eminence in debate, and by the records they have left upon the Statute-book of the country. It may be said that five members is a small infusion to leaven the whole lump of the House of Commons, and I am very conscious that scientific gentlemen may regard at times with a feeling of displeasure, if not with a more contemptuous feeling, some of our modes of procedure and some of our habits of thought in the House of Commons. You may think that we do not display that calmness of judgment, that patient investigation of detail, which characterize the scientific mind. You may think that we import into our discussions too much of a very unscientific heat, and that we are diverted from our objects by a great many cross-currents of prejudice and of party. However that may be, Sir, I believe that the object that you and we have in view is the same. The great historian Hume, speaking of the inception of this Society, said that it was the part of scientific men to lift the veil from the mysteries of Nature. It is a humbler function which the House of Commons has to discharge—to solve the great social and political questions of the day. But the object of both is the same, the attainment of truth, and, by whatever means we can attain that object, that object ought to be the main purpose of our lives. I believe I am right in saying this Society owes its inception and its origin to the University of Oxford. In these later days it owes a debt to the great sister University, in the fact that that University has sent to the chair of your Society a gentleman who combines in his own person, not for the first time, the functions of a Professor, of a member of the University of Cambridge, and of President of this great scientific body. Sir, I am very loth, indeed, to trespass any longer upon your time. I have no claim whatever to do so. I will only very humbly express my views. My own individual opinion is worthless and insignificant; but possibly invested for a few moments with a representative character, and speaking for the House of Commons, and that great public who are behind it, I would say that the public of the present day regard not only with that vague astonishment, which they might well do, the great achievements of science, but they look with admiration upon the great men who have illustrated the history of your Society, and they see in that very lengthened list one of the greatest tributes to the greatness of their country. I do, Sir, very much feel the imperfection with which I have addressed to you these few words. But if I have said that the scientific mind is needed in the House of Commons, I will also say this, that the House of Commons has in these days to face not only great political problems, but some of those questions which are surging up and coming ever more to the front, I mean the great social problems—problems connected with the aggregation of vast multitudes in towns, problems connected with the question how to make the lot of the poor happier, how to make it easier for men to support a life of continuous labour, how, in short, to sweeten life, and to make that toil which falls upon us all lighter to the poor with some ray of hope, and easier with some degree of comfort and convenience. But it is to science that the public must look for aid in solving these questions. You have done much already, but you will add a still nobler title to the admiration of the world if you deal with these subjects, as I am sure you will, in such a manner as to make it impossible for the practical politician to separate himself from the nobler follower of science. It is with a very deep sense of the value of this Society and of the feeling which is abroad with regard to it, that I beg to propose to you —and I thank you most cordially for the toleration with which you have listened to my few remarks—the toast of "The Royal Society."

In response, the President said:—My Lords and Gentlemen, —On behalf of the Society which I have the honour to represent on this occasion, I beg to return our thanks for the honour you have done us in drinking the toast. This Society is by far the oldest scientific Society in the Kingdom, but it cannot for a moment compare in antiquity with that other institution over which the Speaker presides. Our aims are of course naturally very different, and our modes of procedure are different too. We have, as the other House has, discussions in our body, but our discussions are usually carried on with calmness, and we endeavour—those of us who pursue different branches of science —to assist one another. I do not think that that is always the case in the other Society. Perhaps there is nowadays at times something akin to obstruction rather than assistance. However, in order that truth may be elicited, it is necessary that there should be contact between mind and mind, and contact sometimes produces severance. It is better that that contact should take place in order that we should understand one another. Our Society does not exactly deal with social problems such as the Speaker has alluded to, still there are many cases in which questions of great interest to the bulk of the population are capable of being illuminated by scientific researches. To take one remarkable example which has been brought prominently before us. Let us consider the investigations, so important in their results, so purely scientific in inception, which have been carried on by M. Pasteur in France. As the result of a long series of scientific experiments, he has now succeeded in protecting in a great majority of instances those persons who have been so unfortunate as to have been bitten by rabid animals from that terrible disease which ordinarily follows in the wake. His merits in that respect have been duly acknowledged in this country. We know that recently, within the course of the present year, the Lord Mayor called a meeting at the Mansion House to make some recognition on the part of this country of the great debt which we owe to M. Pasteur for those researches. I mention that as one, but it is only one, of many instances in which great social advantages have accrued from purely scientific investigation. I trust that harmony will long continue to exist between the Society which I have the honour to represent, and that which the Speaker represents. I can say this much—that, whatever Government may have been in power, there have frequently been applications made to the Royal Society for advice on some purely scientific questions on which the Cabinet of the day did not feel that they had the requisite knowledge to pronounce an opinion; and this I must say, that the Royal Society has freely given the best of their knowledge on these subjects to the Government of the day, without any consideration of what the politics of that Government might be. I trust that this will ever continue to be the case, and that the Royal Society may go on in a peaceful way doing the duties which belong to it, and that the country may reap the benefits resulting therefrom.

Responding for the toast of "The Medallists," proposed by the President, Prof. Thorpe said:—Mr. President, my Lords, and Gentlemen,—We must all regret, I am sure, that Dr. Salmon's duties as Provost of Trinity College, Dublin, should have prevented him from being present amongst us to-day to receive the Copley Medal in person and to respond to the toas

which has just been so cordially drunk by you. For reasons which my brother medallists at least can fully appreciate, no one feels that regret more keenly than I do. I may confess that it was with a feeling akin to astonishment that I received through a good-natured friend the intimation that the Council of the Society had seen fit to honour such chemical work as I had been able to do by the signal recommendation of the award of a Royal Medal; but that feeling culminated into something like consternation when you, Sir, informed me of your wish that I should reply, in the absence of the Copley Medallist, to the toast with which you have connected my name; and I began to realize the full force of the truth that there are occasions when it is more blessed to give than to receive. Dr. Salmon's absence, however, enables me to attempt to give expression to the feeling of satisfaction and pleasure with which, I am informed, the mathematical world regards this year's award of the Copley Medal. The worker in the field of pure mathematics appeals for recognition to a very select few; his work is, indeed, *caviare* to the general; his are not the triumphs which appeal to the popular fancy or which strike the popular imagination. If he labours for fame, he must be content to wait with the certain knowledge that, if his work be good and true, it will at length meet with the recognition it merits from a tribunal which is unmoved by prejudice and is insensible to the forces of fashion or faction. For nearly half a century Dr. Salmon has so worked, and to-day he receives his reward at the hands of the highest scientific tribunal in the world by the award to him of the most precious gift which it is in the power of that tribunal to bestow. The other medallists, Dr. Gaskell and Dr. Perkin, are happily with us to-night to receive the congratulations of their fellow-workers in science, and to be witnesses of the cordiality with which their health has been drunk by you. But I cannot forego the opportunity of saying also, in their case, how entirely your awards have been appreciated by the great body of scientific opinion, both within and without the Royal Society. To be praised by men who are themselves praised is, we all know, the very highest form of approbation that a man can enjoy, and such, to my knowledge, is the happy lot of the gentlemen whom you have been pleased to honour to-night. It is, however, one of the penalties to a man who is in the position in which I now find myself, and who does not pretend to be an Admirable Crichton, that he is unable from his own knowledge, or rather from the imperfection of it, to do adequate justice to the claims which such men have upon your regard. Dr. Gaskell's work is so entirely outside my own province that it would be in the highest degree presumptuous on my part to offer you any expression of my own opinion as to its merits. Of my colleague and fellow-worker, Dr. Perkin, to whom your Council has awarded the Davy Medal, I trust I may be allowed to speak with greater freedom, because in his case I am more or less upon my own ground, and am talking about matters which are within my own knowledge. It is exactly ten years since that Dr. Perkin was placed by your Council in the position in which I find myself to-day. In awarding him a Royal Medal on that occasion, our former President, the late Mr. Spottiswoode, took the opportunity to say that Dr. Perkin had then been, during more than twenty years, one of the most industrious and successful workers in organic chemistry, and he added that it was seldom that an investigator had extended his researches over so wide a range as was the case with Dr. Perkin, whose work had always commanded the admiration of chemists for its accuracy and completeness, and for the originality of its conception. There is not a chemist here present who will not cordially re-echo these words. Dr. Perkin is, no doubt, known to you all as the originator of one of the most important branches of modern chemical industry—that of the manufacture of colouring matters from coal-tar derivatives—an industry which has acquired almost colossal proportions, and which has effected a complete revolution in the tinctorial arts. I say it with bated breath to you, Sir, as the member for the University of Cambridge, but we all remember the famous saying of Swift as to the value to mankind of the whole race of politicians put together when compared with that man who has made two blades of grass to grow where only one blade grew before. I do not know that Dr. Perkin has achieved that feat, but I claim for him that he has done even more than this, for he has succeeded in demolishing an entire agricultural industry. By his researches he has shown us that we have practically at our own doors, or at least in our own coal-pits, all the richness and beauty of colour which were formerly only to be obtained from the madder fields of Avignon and the Levant. A beneficent fortune, we are glad to know, has not been unmindful of Dr. Perkin's success in thus enriching the world, and she has endowed him with a share of that material benefit which his skill and genius as an investigator has conferred upon us all. That competency, and the well-earned leisure which has sprung from it, Dr. Perkin has dedicated, with a directness and singleness of purpose which merits our warmest appreciation, to the service of science. Nothing, I think, more clearly indicates the truly scientific character of his mind, and his love of science for its own sake, than that he should, whilst comparatively a young man, have turned aside from the pursuit of the great wealth which all his friends thought would ultimately be within his grasp in order that he might follow, undisturbed, his innate desire for pure scientific research. The ten years which have elapsed since our late President alluded in such characteristically graceful terms to Dr. Perkin's labours in the domains of pure and applied chemistry have been rich in scientific achievement, and they have now culminated in that laborious series of researches on one of the most abstruse points of physical chemistry which has been so fittingly rewarded by you by the gift of the Davy medal. I have already alluded to the feeling with which I received the intimation from my good-natured friend that the Council of the Royal Society had been pleased to confer upon me a distinction which is my sole excuse for trespassing upon your indulgence to-night. I will only again refer to that feeling to say that in deference to the express wish of my distinguished friend I am doing my best to get over it. I am bound to add that my friend has himself supplied a reason which in some measure serves to explain the circumstance. Among the pieces of work which the Council have thought worthy of notice was a redetermination of the atomic weight of gold made in conjunction with Mr. Arthur Laurie. I shall not trouble you with the reasons which made that redetermination seem specially desirable, but that it was desirable will be evident from the fact that no fewer than three independent investigations were in progress at the same time in Germany, England, and America. All the results have now been published, and they are, I think, in very fair accord. But my distinguished friend, whose good-nature is only equalled by his candour, has reminded me that there is a discrepancy of a remote decimal place or so in our several values for the atomic weight, and, in default of any other probable hypothesis, it had occurred to him that the real motive of the Council in making the award was to give me both the hint and the opportunity to clear up the disparity. The Gold Medal, he pointed out, would afford an ample supply of the material on which to base a new determination, and the Silver Medal would come in handy for the preparation of the necessary standard solutions. This seemed to me to put the whole matter in a new light, but, on turning to the official intimation of the award forwarded to me by Dr. Foster, and then to a friendly letter which the President has been so good as to send me, I have not gathered that this intention was ever in the mind of the Council, and until I receive a further official intimation that such was the case, I mean to do my best to preserve intact the counterfeit presentment of the gracious lady which adorns the medals. There is just one other matter connected with my work to which, with your permission, I would allude. Reference was made in the terms of the award to a series of researches on fluorine compounds on which I have been engaged for some years past. I wish to mention, and I do so with a very special pleasure, that much of this work has been carried out in co-operation with some of my senior students at the Normal School of Science. This work has been at all times difficult, often disagreeable, and occasionally dangerous, and I am glad to seize this opportunity of testifying to the zeal, assiduity, and, I may add, courage, which my *collaborateurs* have shown in the progress of the investigations. It is a further satisfaction to me to add that the qualities thus evoked and the training thus acquired have been of material benefit to them in their professional advancement, and I can wish them no greater good fortune than that it may be their lot in time to come to occupy my place here, and to be received by you with that indulgence which you have extended to me to-night.

A NEW METHOD OF PREPARING FLUORINE

A NEW method of preparing fluorine has been discovered by M. Moissan. This discovery is the outcome of the success which has attended M. Moissan's efforts to prepare anhydrous fluoride

of platinum. During the process of his memorable work upon the isolation of fluorine by the electrolysis of hydrofluoric acid containing hydrogen potassium fluoride, one of the most remarkable phenomena noticed was the rapidity with which the platinum rod forming the positive electrode was corroded by the action of the liberated gaseous fluorine. It was surmised that a fluoride of platinum was the product of this action, but hitherto all efforts to isolate such a body have proved unsuccessful. In fact, for a reason which will be discussed subsequently, it is impossible to prepare platinum fluoride in the wet way. M. Moissan has, however, been enabled to prepare anhydrous platinum fluoride by the action of pure dry fluorine itself upon the metal. It was found at the outset that, when fluorine is free from admixed vapour of hydrofluoric acid, it exerts no action whatever upon platinum, even when the latter is in a finely-divided state, and heated to 100° C. But when the temperature of the metal is raised to between 500° and 600° C., combination readily occurs with formation of tetrafluoride of platinum and a small quantity of protofluoride. The moment the gas is mixed with a little vapour of hydrofluoric acid, the action is immensely accelerated, and then occurs readily at ordinary temperatures. The same rapid action occurs when platinum is placed in hydrofluoric acid saturated with free fluorine, which accounts for the disappearance of the positive terminal during the electrolysis. In order to prepare the fluoride of platinum, a bundle of wires of the metal is introduced into a thick platinum or fluor-spar tube, through which a current of fluorine gas from the electrolysis apparatus is passed. On heating the tube to low redness, the wires become rapidly converted to fluoride, when they are quickly transferred to a dry stoppered bottle. If the operation is performed in a platinum tube, a large quantity of fused fluoride remains in the tube. The tetrafluoride of platinum, PtF_4, formed upon the wires, consists either of fused masses of a deep red colour, or of small buff-coloured crystals resembling anhydrous platinum chloride. It is exceedingly hygroscopic. With water it behaves in a most curious manner. With a small quantity of water it produces a fawn-coloured solution, which almost immediately becomes warm, and decomposes with precipitation of hydrated platinic oxide and free hydrofluoric acid. If the quantity of water is greater and the temperature low, the fawn-coloured solution may be preserved for a few minutes, at the expiration of which, or immediately on boiling the solution, the fluoride decomposes in the manner above indicated. This peculiar behaviour with water explains the impossibility of preparing the fluoride in the wet way. When the anhydrous fluoride is heated to bright redness in a platinum tube closed at one end, fluorine at once begins to be evolved as gas, and if a crystal of silicon be held at the mouth of the tube it takes fire and burns brilliantly in the gas. The residual platinum is found on examining the contents of the tube to consist of distinct crystals of the metal. Hence by far the most convenient method of preparing fluorine for lecture purposes is to form a considerable quantity of the fluoride first by passing the product of the electrolysis over bundles of platinum wire heated to low redness, and afterwards to heat the fluoride thus obtained to full redness in a platinum tube closed at one end. It only remains now to discover another method of preparing fluoride of platinum in the dry way, to be able to dispense with the expensive electrolysis apparatus altogether. M. Moissan has also prepared a fluoride of gold in the same manner. It is likewise very hygroscopic, decomposable by water, and yields gaseous fluorine on heating to redness.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, November 21.—"On the Local Paralysis of Peripheral Ganglia, and on the Connection of Different Classes of Nerve-Fibres with them." By J. N. Langley, F.R.S., Fellow of Trinity College, and W. Lee Dickinson, Caius College, Cambridge.

We found that in the rabbit, 30 to 40 milligrams of nicotin injected into a vein stopped the effect of stimulating the sympathetic in the neck, not only on the pupil, but also on the vessels of the ear. It occurred to us that this action of nicotin might be due to a paralysis of the nerve-cells of the superior cervical ganglion, and not to a paralysis of the peripheral endings of the sympathetic nerve. On testing this view, we found that, after a certain dose of nicotin, stimulation of the sympathetic fibres below the ganglion does not produce dilation of the pupil or constriction of the vessels of the ear, whilst stimulation of the sympathetic nerve-fibres above the ganglion produces these changes in the normal manner.

The method of action of nicotin can be tested in a more direct manner by local application to the isolated nerve and ganglion. When the sympathetic in the neck has been brushed over with a 1 per cent. solution of nicotin, stimulation of it produces the usual dilation of the pupil and constriction of the vessels of the ear; but when the superior cervical ganglion and the filaments proceeding from it have been brushed over with the 1 per cent. nicotin, stimulation of the sympathetic in the neck is found to be completely without effect, while stimulation of the filaments running from the ganglion to the carotid arteries produces the normal action.

Hence nicotin paralyzes the cells of the superior cervical ganglion.

On the fibres of the cervical sympathetic, which are vaso-motor for the head generally and secretory for the salivary glands, we have made a few experiments only; but so far we have been unable to detect any effect from stimulating the sympathetic in the neck after nicotin has been applied to the ganglion.

We conclude that *the dilator fibres for the pupil, the vaso-constrictor fibres for the ear (probably also those for the head generally), and the secretory fibres for the glands, end in the cells of the superior cervical ganglion.*

Ganglion of the Solar Plexus.—In the dog, cat, and rabbit, the splanchnic nerve on the left side runs to two chief ganglionic masses, which we may call respectively the cœliac and superior mesenteric ganglia. The renal ganglia are scattered, but in the dog the chief one often lies underneath the supra-renal body, and in the cat the chief one is placed between the artery and vein about ½ inch from the superior mesenteric ganglion.

To determine whether the inhibitory fibres of the splanchnic end in the nerve-cells of the solar plexus we proceeded as in the case of the superior cervical ganglion. Having ascertained that the application of 1 per cent nicotin to the splanchnic leaves its inhibitory power unaffected, we found that nicotin applied to the whole plexus at once abolishes the inhibitory power of the splanchnic; but inhibition can still be produced by stimulating the fibres proceeding from the ganglia. Hence, *the inhibitory fibres of the splanchnic end in the cells of the solar plexus.*

Our experiments are not sufficiently numerous, especially with regard to the connection of the cœliac ganglion with the stomach, to make it certain that the one ganglion is entirely connected with fibres to the intestine, and the other with the fibres to the stomach; but we think they show *that in the main, and possibly altogether, the stomachic inhibitory fibres of the splanchnic nerve end in the cells of the cœliac ganglion, and the intestinal inhibitory fibres of the splanchnic end in the cells of the superior mesenteric ganglion.*

We find, however, that *the motor fibres of the vagus for the stomach and intestines do not end in the nerve-cells of the solar plexus.*

The connection of the vaso-motor fibres of the splanchnic with the nerve-cells of the solar plexus can be determined by taking a tracing of the arterial blood-pressure and stimulating the splanchnic before and after the application of nicotin to the ganglia. By applying nicotin to both ganglia, the rise of blood-pressure caused by stimulating the splanchnic is reduced to very small limits, and by applying it to the renal plexus as well, the effect of splanchnic stimulation on the blood-pressure is abolished. Since in this case there is no fall of blood-pressure, we conclude *that the vaso-dilator as well as the vaso-constrictor fibres of the splanchnic end in the cells of the solar and renal plexuses.*

Combining oncometer observations on the dog with blood-pressure observations on the rabbit and cat, we think there is fair evidence *that the splanchnic vaso-motor fibres for the kidney end in the cells of the renal plexus.*

We have experimented upon various peripheral ganglia other than those mentioned above, and, though our results are as yet incomplete, with essentially similar results; that is, we have obtained an abolition of the effect of some one or more of the classes of nerve-fibres running to them. We think, then, there is fair ground to conclude that *by stimulating the nerve-fibres running to and those from any peripheral ganglion, before and after the application of dilute nicotin to it, the class of nerve-fibres which end in the nerve-cells of the ganglion can be distinguished from those which run through the ganglion without being connected with nerve-cells.*

Linnean Society, November 7.—Mr. W. Carruthers, F.R.S., President, in the chair.—Mr. H. Veitch and Rev. Prof. Henslow exhibited a beautiful series of East Indian hybrid rhododendrons, on which Prof. Henslow made some valuable remarks on the effects of cross-fertilization in regard to colour and alteration of structure, upon which some critical observations were made by Mr. Veitch, Prof. Bower, and Captain Elwes.—Mr. E. M. Holmes exhibited and made remarks upon some new British marine Algæ, describing their origin and affinities.—Dr. St. George Mivart, F.R.S., exhibited a drawing by a surgeon, who had been consulted as to amputation of a tail-like process in the human subject, being a prolongation of the coccyx to the extent of 4½ centimetres. Dr. Mivart also exhibited a photograph, showing a remarkable resemblance between two arm stumps; one the result of an amputation, the other a congenital defect in the child of a nurse who had attended the patient whose arm was amputated. Both cases were commented on and explained by Dr. W. O. Priestley, and further remarks were offered by Dr. Murie, and Mr. W. Thiselton-Dyer.—Mr. W. B. Hemsley then read a paper by General Collett, C.B., and himself, on a collection of plants made in the Shan States, Upper Burmah. An interesting discussion followed, in which Messrs. J. G. Baker, C. B. Clarke, and Captain Elwes took part.

Anthropological Institute, November 12.—Dr. J. Beddoe, F.R.S., President, in the chair.—Dr. Beddoe read a paper on the natural colour of the skin in certain Oriental races. Dr. Beddoe's observations showed that the parts of the skin covered by clothing were very much lighter than those exposed to the sun and air; and that those people whose skin was the darkest in the covered parts, were not those who tanned to the blackest hue.—A paper by the Rev. James Macdonald was read on the manners, customs, superstitions, and religions of South African tribes.

Paris.

Academy of Sciences, November 25.—M. Hermite in the chair.—On the November number of the *American Meteorological Journal*, by M. H. Faye. With this number begins the publication of a complete exposition of the author's theory of cyclonic movements, translated into English by Mrs. W. Harrington. The first part deals with storms, the second with tornadoes, while the third is occupied with the relations of tornadoes and storm phenomena to cyclones properly so called.—On animal heat, by M. Berthelot. In continuation of his previous paper on this subject, the author here discusses the question of the heat liberated by the action of oxygen on the blood. The quantity thus set free, referred to the molecular weight of oxygen (O_2 = 32 gr.), is found, by the extremely delicate experiments here described, to average 14 77 calories.—On the exhaustion of soils cultivated without manure, and on the value of the organic matter in the soil, by M. P. P. Dehérain. A series of experiments carried out at the Agricultural School of Grignon clearly shows that the substance chiefly lost by continuous cultivation without manure is carbon, the proportion of phosphoric acid, potash, and nitrogen eliminated being comparatively slight. It also appears that the organic matter itself is as important a fertilizing element for beetroot as are the nitrates, phosphates, or potash.—On the freno-secretory fibres, by M. Arloing. Experiments are described which demonstrate the existence of these fibres in the cervical chord of the large sympathetic nerve.—Observations on Swift's new comet (November 17) made at the Paris Observatory with the equatorial of the west tower, by M. G. Bigourdan. On November 21 the comet had the appearance of a very faint nebulosity (about 13·4), nearly round, diameter about 50″, without marked condensation. Observations made by Mdlle. D. Klumpke with the equatorial of the east tower on November 23 yielded similar results.—Generalization of Makeham's law of probabilities, by M. A. Quiquet. The chief property of Gompertz's formula as generalized by Makeham has been demonstrated in a very simple way by M. J. Bertrand. M. Quiquet in his turn now inquires whether this property may not itself be a particular case of a still more general principle, and whether the function discovered by the two eminent English actuaries may not therefore be capable of further generalization.—On the employment of electric conducting mediums in studying the displacements and distribution of acids with complex nature, by M. Daniel Berthelot. Of the numerous substances acting both as acid and as alkali one of the simplest is aspartic acid. The author here studies the equilibria that are produced in the presence of this acid in diluted saline solutions. The measurements have been made with the Lippmann capillary electrometer, by M. Bouty's electrometric method.—Variations of the electric resistance of nitric peroxide at different temperatures, by M. J. J. Boguski. Measurements obtained by several methods lead to the conclusion that an increase of temperature of nitric peroxide produces an increase of its electric resistance, the most abrupt variations occurring between 0° and 17° C. Above 70° this acid forms an almost perfect insulator. During the process of heating two consecutive phenomena were observed which call for special attention. To a rise of temperature up to a given limit generally corresponds a *static* and definite increase of resistance; but this increase itself *is preceded by a dynamic* (passing) *decrease* of resistance, whose momentary value is at times no more than $\frac{1}{100}$ or $\frac{1}{2000}$ of the static and normal resistance.—Preparation and properties of the anhydrous platinous fluoride, by M. H. Moissan. In continuation of his previous researches, the author here shows that platinous fluoride, $PtFl_2$, decomposes water at the ordinary temperature, which accounts for the impossibility of preparing it by the wet process. At red heat it is decomposed into crystallized platinum and fluorine.—Contribution to the study of double decompositions between the halogen salts of mercury and zinc, by M. Raoul Varet. The author has studied (1) the action of cyanide of mercury on bromide of zinc; (2) the action of cyanide of zinc on bromide of mercury.—On a new sugar of the aromatic group, by M. Maquenne. To inosite and quercite, the only saccharine substances hitherto obtained from benzene, the author adds a third, provisionally named β-inosite, which he obtains from a pinite derived from the resin of *Pinus lambertiana*, of Nebraska.—Synthesis of metaphenylene-diamine, by M. Alphonse Seyewitz. The author has succeeded in effecting this synthesis by heating, to 280° or 300° C., a mixture of resorcine and calcium chloride under conditions here described.—Papers were submitted by MM. A. Béhal and Choay, on the action of heat on chloral-ammonia; by M. Raphael Dubois, on the mechanism of awakening in hibernating animals; by M. E. Couvreur, on the pulmonary circulation of the frog, as affected by the excitation of the pneumogastric nerve; by M. R. Moniez, on the larva of the new species *Tænia Grimaldii*, a parasite of the dolphin; by MM. Appert and Henrivaux, on the devitrification of the ordinary glass of commerce; by MM. E. A. Martel and G. Gaupillat, on the formation of springs in the interior of the limestone plateaux of the *causses* of Languedoc; and by M. J. Thoulet, on the quantitative analysis of the fine sediment held in suspension in natural waters.

Berlin.

Physiological Society, November 15.—Prof. du Bois-Reymond, President, in the chair.—After the appointment of officers for the year 1889-90, Dr. Virchow spoke on the spiracle gill of Selachians. With the assistance of drawings and a series of diagrams he discussed the varying arrangements and divisions of the blood-vessels which go to form the gills of Selachians; he also described the frequent occurrence, confined to certain regions of the head, of blood-vessels which are elaborately convoluted; the physiological significance of these vessels is quite unknown, but their morphological interest is so great that an extended investigation of them in other groups of animals is a matter of great importance. In all probability they are rudimentary structures, whose significance would be understood if the above extended investigations were carried out.—Dr. I. Munk spoke on the absorption of fats and fatty acids in the absence of bile in the intestine. The older classical experiments on animals with a biliary fistula had taught that, in the absence of bile, proteids and starch are digested as completely as in a normal animal, whereas, on the other hand, the absorption of fat is largely interfered with. In correspondence with this view, the later observers were of opinion that all fat which is not absorbed does not leave the body as neutral fat, but as fatty acids, and from this the conclusion was drawn that the fats of food are decomposed into fatty acids (and glycerin) before they are normally absorbed. The speaker had carried out a series of experiments on dogs with biliary fistulæ, during the past summer, with a view to clearing up several obscure points in the whole question of the absorption of fats. After he had confirmed the older views as to the normal digestion of proteids and starch, and the appearance of unabsorbed fat in the form of free fatty acids in the fæces, he proceeded to determine quan-

titatively the absorption of fat from the intestine in the absence of bile. He found, first, that in such animals there is a relatively large absorption of fat from the alimentary canal as long as they receive the fat in company with proteids and starch, but that the absorption is much less when the fat is administered—as it was in the experiments of the older observers—mixed only with proteids. It was found that the animals absorbed more than 70 per cent. of such a fat as pig's lard, whose melting-point is low, without the assistance of bile; they also absorbed an almost proportionately large quantity of the free fatty acids of the lard, thus corresponding exactly to the behaviour of normal animals, which can absorb about 94-98 per cent. of any fat whose melting-point is low, whether it be administered in the form of neutral fat or of the fatty acids which it contains. When a fat was administered whose melting-point is high—especially such a fat as only begins to soften at the temperature of the body (*e.g.* mutton fat)—the amount absorbed was considerably less, and it was still less when the free fatty acids of this fat were given with the food. The speaker pointed out, with regard to the fæces of animals with a biliary fistula, that they may be dark-coloured, or even black, on a proteid diet, and only appear light-gray in colour when carbohydrates are given with the food. This dark colour is not, however, due to any derivative of the bile-pigments, but to hæmatin. The speaker had not been able to detect, with certainty, any further advanced decomposition of the contents of the intestine in animals with a biliary fistula, neither did he observe any increase of putrefactive products, such as indol, skatol, &c., in their urine.

IN our report, last week (p. 95), of the meeting of the Berlin Physical Society on October 25 (first column, fifth line from foot), *for* "waves in air 21 metres long" *read* "waves in air 2 kilometres long."

DIARY OF SOCIETIES.

LONDON.

THURSDAY, DECEMBER 5.

ROYAL SOCIETY, at 4.30.—Remarks on Mr. A. W. Ward's Paper on the Magnetic Rotation of the Plane of Polarization of Light in Doubly-Refracting Bodies: O. Wiener and W. Wedding.—Researches on the Chemistry of the Camphoric Acids: J. E. Marsh.—The Internal Friction of Iron, Nickel, and Cobalt, studied by means of Magnetic Cycles of very Minute Range: H. Tomlinson, F.R.S.—A Compound Wedge Photometer: Dr. Spitta.

LINNEAN SOCIETY, at 8.—Life History of a Stipitate Fresh-water Alga: G. Massee.—On the Anatomy of the Sand Grouse: G. Sim.

FRIDAY, DECEMBER 6.

PHYSICAL SOCIETY, at 5.—The Electrification of a Steam Jet: Shelford Bidwell, F.R.S..—Notes on Geometrical Optics, Part II.: Prof. S. P. Thompson.—On the Behaviour of Steel under Mechanical Stress: C. H. Carus-Wilson.—On a Carbon Point in a Blake Telephone Transmitter: F. B Hawes.

GEOLOGISTS' ASSOCIATION, at 8.—*Conversazione.*

SUNDAY, DECEMBER 8.

SUNDAY LECTURE SOCIETY, at 4.—The Wonders of the Yellowstone Park, the Recreation Ground of America; a Personal Narrative (with Oxyhydrogen Lantern Illustrations from the Lecturer's own Camera): Wm. Lant Carpenter.

MONDAY, DECEMBER 9.

SOCIETY OF ARTS, at 8.—Modern Developments of Bread-making: William Jago.

TUESDAY, DECEMBER 10.

ANTHROPOLOGICAL INSTITUTE, at 8.30.—The Natives of Mowab, Daudai, New Guinea: Edward Beardmore. Communicated by Prof. A. C. Haddon.—Fire-making in North Borneo: S. B. J. Skertchley.—On the Origin of the Eskimo: Dr. H. Rink.

INSTITUTION OF CIVIL ENGINEERS, at 8.—On the Triple-Expansion Engines and Engine Trials at the Owens College, Manchester: Prof. Osborne Reynolds, F.R.S. (Discussion.)

WEDNESDAY, DECEMBER 11.

SOCIETY OF ARTS, at 8.—The Paris Exhibition: H. Trueman Wood.

ROYAL MICROSCOPICAL SOCIETY, at 8.—On the Freshwater Algæ and Schizophyceæ of Hampshire and Devon: A. W. Bennett.

THURSDAY, DECEMBER 12.

ROYAL SOCIETY, at 4.30.

MATHEMATICAL SOCIETY, at 8.—On the Radial Vibrations of a Cylindrical Shell: A. B. Basset, F.R.S.—Note on 51840-Group: G. G. Morrice.—On the Flexure of an Elastic Plate: Prof. H. Lamb, F.R.S.

INSTITUTION OF ELECTRICAL ENGINEERS, at 8.—Annual General Meeting.—Election of Council and Officers for 1890.—Electrical Engineering in America: G. L. Addenbrooke. (Discussion.)

FRIDAY, DECEMBER 13.

ROYAL ASTRONOMICAL SOCIETY, at 8.

INSTITUTION OF CIVIL ENGINEERS, at 7.30.—Hydraulic Station and Machinery of the North London Railway, Poplar: John Hale.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Giornale di Scienze Naturali ed Economiche, 1887 and 1888 (Palermo).—*Challenger* Report—Zoology, vol. xxxii (Eyre and Spottiswoode).—Collotype and Photo-lithography: Dr. J. Schnauss; translated by E. C. Middleton (Iliffe).—A Text book of Human Anatomy: Dr. A. MacAlister (Griffin).—A Naturalist in North Celebes: Dr. S. J. Hickson (Murray).—Algebra, Part 2: G. Chrystal (Edinburgh, Black).—A Hand-book of Modern Explosives: M. Eissler (Lockwood).—Contributions to Canadian Palæontology, vol. i., Part 2: J. F Whiteaves (Montreal, Brown).—Modern Thought and Modern Thinkers: J. F. Charles (Relfe).—The Land of an African Sultan: W. B. Harris (Low).—Index of British Plants: R. Turnbull (Bell).—Manual for Beginners and for the London University Matriculation Examination.—The Anatomy of the Frog: Dr. A. Ecker; translated by Dr. G. Haslam (Oxford, Clarendon Press).—A Narrative of Travels on the Amazon and Rio Negro: A R. Wallace (Ward, Lock).—Pawnee Hero Stories and Folk Tales: G. B. Grinnell (New York).—Palestine: Major Conder (Philip).—Tractatus de Globis: R. Hues; edited by C. R. Markham (Hakluyt Society).—Among Cannibals: C. Lumholtz (Murray).—Im Hochgebirge: Dr. E. Zsigmondy (Leipzig, Duncker and Humblot).—Niels Klein's Wallfahrt in die Unterwelt: L. Holberg; edited by E. H. Babbitt (Boston, Heath).—Practical Observations on Agricultural Papers, &c., 2nd edition: H. Wilson, Jun. (Simpkin).—Du Transformisme et de la Génération Spontanée: C. A. Rohant and Dr. M. Peter (Paris, Baillière).—Einiges über die Entstehung der Korallenriffe in der Javasee und Branntwunsbai, und über Neue Korallenbildung bei Krakatau: Dr. C. Ph. Sluiter (Batavia, Ernst).—Journal of the Royal Microscopical Society, October (Williams and Norgate).—The Asclepiad, No. 24, vol. vi.: Dr. Richardson (Longmans).—Proceedings of the Boston Society of Natural History, vol. xxiv., Parts 1 and 2 (Boston).—Journal of Morphology, vol. iii. No. 2 (Boston, Ginn).

CONTENTS.

THURSDAY, DECEMBER 12, 1889.

THE TEACHING OF FORESTRY.

A Manual of Forestry. By William Schlich, Ph.D. Vol. I. (London: Bradbury, Agnew, and Co., 1889.)

PROBABLY it will not for some time be generally recognized in England that forestry is a profession in the sense in which we speak of the profession of law or of medicine. And it is a bold step to publish a manual of forestry for English readers in a systematic and strictly technical form. This is the task which Dr. Schlich has undertaken, and the volume before us is the first instalment of a large work, which, when completed, will be the first comprehensive manual of forestry in the English language.

Before going out to India in 1866, Dr. Schlich had passed the examinations for the superior forest service in his own country (Hesse Darmstadt), he had been the pupil of one of the most eminent Professors of Forestry in Germany, the late Gustav Heyer, and he held a distinguished place among his fellow students. At the commencement of his career, the changes which had taken place in Hesse Darmstadt in consequence of the Austrian war were believed to affect injuriously the chances of promotion for the younger members of the forest service. This induced him to accept the offer of an appointment in India. Here he was designated at an early date for important positions, and thus, after he had served several years in Burmah, he was sent to Sind, where, under completely different conditions of climate and forest, he did excellent work. He served successively as Conservator of Forests in Lower Bengal and in the Punjab, until he rose to the post of Inspector-General of Forests. In 1885 he consented to relinquish his important position in India, in order to become Professor of Forestry at the Forest School which it had been decided to form in connection with the Royal Indian Engineering College at Coopers Hill.

The volume before us contains the general and introductory part; in a second volume the author proposes to set forth in detail the different sylvicultural operations; while the protection of forests, the utilization of timber and other forest produce, the systematic arrangement of the plans for working, and the financial aspect of forest management, will complete the work. Not the least of the advantages which will be gained by the publication of this manual will be to settle the English forest terminology. The technical terms which had been tentatively used since methodical forest management was begun in India may now be expected to receive general currency, and will be more correctly understood than before.

The primary object of the Coopers Hill Forest School is the training of officers for the Indian Forest Service, but others also may attend the forestry classes in order to qualify for the management of forests and woodlands in Great Britain and in the colonies. It may therefore be hoped that Dr. Schlich's manual will eventually promote the good management of forests in many parts of the world. In Great Britain and Ireland the author states the area of woods and forests at 2,790,000 acres, and in British India the area of Government forests at 70,000,000. No data are available for estimating the forest area in the British colonies. But the area stated is sufficient to demand the systematic teaching of forestry in England.

In the German Empire the total forest area only measures 34,346,000 acres, of which 11,243,000 acres belong to the State. Yet there are no less than nine forest schools in the different States for educating the superior officers in the State and other public forests and the principal wood managers in private estates. The books published on the subject of forestry in all its branches during the three years 1886-88 amounted to 177, or fifty-nine a year on an average. Besides these, there are ten periodicals on forestry, some quarterly, most monthly. One general association of German foresters meets annually, and ten local societies hold their meetings either annually or once in two years. And all these associations publish their transactions. Perhaps it will be urged that this large and daily-growing forest literature is not necessarily an advantage; that German foresters had better attend to the management of their forests instead of writing books. As a matter of fact, however, the management of the German forests, public as well as private, is excellent, and is improving steadily. The best proof of this is the large and steadily growing income derived from these estates by the Government, by towns and villages, and by private proprietors, and, more than that, the improved condition and the increased capital value of these properties.

A commencement, however, of forest literature has been made in the English language. The Transactions of the Royal Scottish Arboricultural Society have attained their twelfth volume, and they frequently contain papers of considerable importance. The *Indian Forester*, commenced as a quarterly by Dr. Schlich in 1875, is now a monthly magazine, of which fifteen volumes have appeared. In addition to these a number of valuable publications on different branches of forestry might be named that have been published within the last twenty-five years.

German forest literature, though it has attained such large dimensions, is of comparatively recent origin. During the eighteenth century sylviculture and the management of forestry had made great progress in many parts of the country, but the methodical and scientific treatment of the subject dates from the labours, during the first thirty years of the present century, of Hartig in Prussia, Cotta in Saxony, and Hundeshagen at Giessen. Scientific forestry in England must necessarily be built upon what has been accomplished in this respect in Germany, and with becoming modesty Dr. Schlich acknowledges that the principal German works have been his guide in the preparation of the present book. Great Britain does not stand alone in this respect. In France also the development of scientific forestry has to a great extent been based upon the progress previously made in Germany. The same may be said of forestry in Italy, Russia, Scandinavia, and other European countries.

Part I. of the manual treats of the utility of forests, directly in producing wood and other forest produce, and indirectly in influencing the climate, in the distribution of rain-water, in the preservation of the soil on sloping ground, in the binding of moving sands, and in affording shelter against winds. All these matters are clearly and ex-

haustively treated, and in regard to the climatic influence of forests the author gives a most useful summary of the researches which have been made to determine the effect of forest growth upon the temperature of air and soil, rainfall, humidity, and evaporation, in Germany, Switzerland, and France, mainly by the establishment of parallel stations, one being situated inside a fully stocked forest and the other at some distance in the adjoining open country.

Part II. sets forth the fundamental principles of sylviculture. The author maintains, with justice, that the principles of sylviculture hold good all over the world, but adds that the illustration of these principles must be taken from a limited area. For this purpose he has chosen the timber trees of Western Europe on the 50th degree of north latitude, and the countries immediately to the north and south of it—in other words, the forest trees of England, Northern France, and the greater part of Germany. These species the author does not attempt to describe; he assumes that his readers are familiar with them. The first chapter dwells upon the external conditions which influence the development of forests. He says:—

"Soil, including subsoil, and atmosphere are the media which act upon forest vegetation, and they together are in sylviculture called the 'locality.' The active agencies, or factors, of the locality depend on the nature of the soil and the climate, the latter being governed by the situation. The sum total of these factors represents the quality or yield-capacity of the locality. The forester requires to be well acquainted with the manner in which soil and climate act on forest vegetation, in order to decide in each case which species and method of treatment are best adapted, under a given set of conditions, to yield the most favourable results."

Every forester knows that on good soil, and under conditions otherwise favourable, a timber crop is heavier than one of equal age grown under less favourable conditions. In the concluding section of this chapter the author shows how one may use this fact in order to assess the quality of a locality. Numerous measurements of woods of different species and ages, grown under different conditions, have been made in Germany on a systematic plan, and from the data thus obtained yield tables have been calculated, showing the volume of timber produced at different ages on a given area by the principal species on localities of different quality classes. Using the yield tables published for the Scotch pine by Wilhelm Weise, now Professor at the Forest School of Karlsruhe, the author shows that at the ages of 50 and 120 years the volume per acre of timber only, not including faggots, in localities, which according to their yield-capacity are classed as first, second, and third class, is as follows:—

	I.	II.	III.
Cubic feet at the age of 50 years	5060	3940	2700
" " 120 "	9060	6950	5340

The figures of these yield tables Dr. Schlich has found to a certain extent to be applicable to Scotch pine forests in England. They can therefore be used in order to assess the yield-capacity of any locality stocked with Scotch pine. Eventually, similar yield tables will doubtless be prepared for the Scotch pine and other forest trees in Great Britain, and it will then be possible with certainty to say what yield of timber may be expected from plantations made in a certain locality.

The second chapter deals with the shape and development of forest trees, but we can refer only to what the author says regarding height-growth. Building again chiefly upon researches made in Germany, Dr. Schlich explains how the different species have a different mode of height-growth. On p. 163 an instructive diagram will be found exhibiting the relative height-growth of spruce, silver fir, beech, and Scotch pine, in a locality of the first quality. At the age of 50 years the mean height attained by each species is as follows:—

Scotch pine	64 feet
Beech	60 "
Spruce	55 "
Silver fir	40 "

At a later age spruce and silver fir take the lead, while beech and Scotch pine remain behind in the race; and when 120 years old the order of the species stands as follows:—

Spruce	118 feet
Silver fir	108 "
Beech	102 "
Scotch pine	97 "

Scotch pine and beech therefore make the principal height-growth during the first period of their life, whereas spruce and silver fir continue to grow vigorously in height to a much greater age, spruce more so than silver fir. The progress of height-growth of the different species is much affected by the character of the soil, by elevation, the more or less crowded state of the wood, and other circumstances, but under otherwise similar conditions it will always be found that deep, fresh fertile soil produces much taller trees than shallow, dry, or rocky soil.

In the third chapter, which deals with the character and composition of woods, the author points out that the object of sylviculture is not to rear isolated trees, but considerable masses of trees, forming more or less crowded woods. Pure woods consist of one species only, or of one species with a slight admixture of others, whereas mixed woods contain a mixture of two or more species. The advantages of mixed woods are clearly set forth, and the author's remarks on this subject may be specially recommended to the attention of proprietors and managers of woodlands in Great Britain.

The last and most important chapter deals with the sylvicultural systems—that is, the different methods under which the creation, regeneration, tending, and utilization of woods are effected. The three well-known classes are: first, high forest, originating in seedlings, either self-sown or artificially raised; second, coppice, which regenerates itself from coppice shoots; and third, coppice with standards, a combination of seedling and coppice forest. The modifications of these three main systems are numerous, and particularly the treatment of high forest has developed in a great variety of ways. On this subject we must refer the reader to the manual. These are matters which can hardly be fully understood without opportunities for obtaining practical experience of forests treated under the various systems described. Such opportunities may, to some extent, be found in Great Britain. The high forests of larch and Scotch pine in Scotland, raised by planting, are excellent, and in some

districts Scotch pine woods are regenerated by self-sown seedlings. The oak woods of the Forest of Dean, and the beech woods on the chalk downs of Buckinghamshire, are instances of high forests with different character and different method of treatment. Most instructive, again, are the natural oak forests in Sussex—coppice, with a large proportion of standards. So are the coppice woods of ash and sweet chestnut for the production of hop-poles in Kent, and the osier beds on the banks of the Thames. The difficulty is, that the treatment of these woods is entirely empirical, and that, without authentic statistical data regarding yield in timber, regarding income and outlay, no forest can properly be used for purposes of instruction. If the student wishes fully to understand this and other portions of the excellent manual before us, he must study the forests of Germany, public and private. This may be a disadvantage, but under the circumstances of the case it cannot be helped.

Appended to the first part of the book are two treatises which will be read with interest by those who may not care to study the more technical portion of the manual. They deal with forestry in Great Britain and Ireland and in British East India. The physical configuration of India, its climate and rainfall, the distribution of the forests, and the forest policy pursued by the Government of India during the last thirty years, are clearly set forth. The protection and systematic management of its forests are matters of the utmost importance for the welfare of the millions inhabiting the British Indian Empire, of infinitely greater importance than good forest management is for Germany or other countries of Europe. Enthusiastic foresters in India have long maintained that, by improving the condition of existing forests, so as to make them more dense and compact, by extending their area, and by creating forests where none exist at present, the rainfall in seasons of drought might be increased, and famines might thus be averted. Dr. Schlich fully discusses this subject, and states several cases in which the presence of dense forest growth seems to accompany an increased rainfall; but at the same time he fully explains the reasons why a final conclusion does not seem justified. The result is that, though the local influence of forests in lowering the temperature and preserving moisture is undeniable, we are not justified in hoping for an improvement of the Indian climate. The favourable influence of forests in India upon the irrigation from wells and tanks is, however, beyond doubt, and this is a vital question.

To illustrate the effect of forest growth in protecting loose soil on hill-sides, the author mentions the Siwalik hills at the foot of the North-West Himalaya. We quote his words:—

"Anyone who has ever stood on the hills behind Hushiarpur in the Punjab, and looked down upon the plain stretched out towards the south-west, has carried away an impression which he is not likely to forget. In that part the Siwalik range consists of an exceedingly friable rock, looking almost like sand baked together. Formerly, the range was covered with a growth of forest vegetation, but a number of years ago cattle owners settled in it, and under the combined attacks of man, cows, sheep, and goats, the natural growth disappeared, while the tread of the beasts tended to loosen the soil. The annual monsoon rains, though not heavy, soon commenced a process of erosion and of carrying away the surface soil. Gradually, small and then large ravines and torrents were formed, which have torn the hill range into the most fantastic shapes, while the *débris* has been carried into the plains, forming, commencing at the places where the torrents emerge into the plain, fan-shaped accumulations of sand which reach for miles into the plain, and which have already covered and rendered sterile extensive areas of formerly fertile fields. Indeed, one of these currents or drifts of sand has actually carried away a portion of the town of Hushiarpur. The evil has by no means reached its maximum extent, and if curative measures are not adopted at an early date, the progress of transporting the hill range into the plain will go on, until the greater part of the fertile plain stretching away from its foot has been rendered sterile."

The author might have added the denuded hills, and the rivers, formerly navigable, but now silted up, in the Ratnagiri district of Western India, and other similar instances.

That a country so populous as India requires immense quantities of timber, bamboos, and firewood, goes without saying. Among other articles of forest produce, cattle fodder is an important item. In the drier portions of the country the supply of grass, particularly during seasons of drought, is more plentiful under the shelter of trees than out in the open. In times of scarcity, grain can easily be carried long distances to provide food for the people, while cattle fodder cannot be so easily carried. As a matter of fact, where forests have been formed and protected in the drier parts of India, they have proved a great help in enabling the people to maintain their cattle in times of drought and scarcity.

In India the duty of taking action necessarily devolved upon the State. The result has been the formation of extensive forest estates, called reserved forests, which at present, the author states, aggregate 33,000,000 acres, or three times the area of State forests in the German Empire. If forest matters in India continue to be properly managed, these estates will not only secure the well-being of the people, but will be an important source of strength to the Government, financially and otherwise. As yet, the revenue which they yield is insignificant in relation to their extent. But it is growing steadily. Dr. Schlich shows that during the three years 1864-67 the average annual net revenue from the Government forests amounted to £106,615, and during the five years 1882-87 to £384,752; and he states it as his opinion that, twenty-five years hence, the net surplus will be four times the present amount. More important, however, than the annual revenue is the steadily increasing capital value of these Government forest estates.

In Great Britain the aspect of affairs is different. The small area of the Crown forests, burdened as they are with prescriptive rights, cannot reasonably be expected materially to help the development of systematic forest management. But there are over 2,500,000 acres of woods and forests in the hands of private proprietors, and there are 26,000,000 acres of barren mountain land and waste, a portion of which might be planted up. Proprietors, as a rule, desire to augment their income and to increase the capital value of their estates. In many cases this might be effected by a more systematic management of their woodlands, and by the planting up of waste lands. The chief obstacle to progress in this direction is the low

price of timber and the high rent at present obtained by the letting of grouse moors and deer forests.

Upon data which cannot be gainsaid, Dr. Schlich has based important calculations, which will be found on pp. 17–19. Space forbids the discussion of details, but the result is that Scotch pine forests cannot be expected to yield more than 2½ per cent. on the capital invested (the value of the land and of the growing crop).

"All land, therefore, which can be let for the raising of field crops, for shooting, or other purposes, at a rental equal to, or upwards of, 2½ per cent. of the capital value of the land, had better be so let. On the other hand, land which would realize a rental of less than 2½ per cent. of its value, may with advantage be planted with Scotch pine or other similarly remunerative trees."

These conclusions are based upon circumstances as they exist at the present time. But a change of circumstances is not impossible. The author points out that 6,000,000 loads of timber are imported annually into the United Kingdom from Europe and North America, and that only a small portion of the forests which furnish this large supply are under systematic management and control. It may be regarded as certain that the supply from Sweden and Norway and from North America, amounting at present to nearly 4,000,000 loads a year, will continue to diminish, and, under the circumstances of the case, the necessary result of such diminution will eventually be a rise in the price of timber. Again, if proprietors of woodlands in England and Scotland were in a position to offer large quantities of home-grown timber of good quality for sale, regularly at stated seasons, timber traders would make their arrangements accordingly, and in many cases better prices would be obtained. Firewood is at present almost unsaleable in the United Kingdom, but if—and this may happen—the price of coal should rise considerably, firewood would in some districts become an article of general consumption, as it was 150 years ago, and to some extent this would improve the money yield of woodlands.

It is not too much to say that the publication of Dr. Schlich's manual will give a powerful impetus to systematic forest management in the United Kingdom, in India, and in the vast colonies of the British Empire—in fact, wherever the English language is spoken.

D. BRANDIS.

FERREL'S THEORY OF THE WINDS.

A Popular Treatise on the Winds. Comprising the General Motions of the Atmosphere, Monsoons, Cyclones, Tornadoes, Waterspouts, Hailstorms, &c. By William Ferrel, M.A., Ph.D., &c. (New York: John Wiley and Sons. London: Macmillan and Co. 1889.)

NUMEROUS as are the popular treatises on various branches of phenomenal meteorology that have appeared during the last quarter of a century, English literature has hitherto been singularly deficient in elementary works treating of the physical and mechanical processes of the atmosphere from a theoretical point of view, and suited to the capacity of the average student. Those versed in the higher mathematics may indeed find all they require in such modern works as Sprung's "Lehrbuch der Meteorologie," and Ferrel's "Recent Advances in Meteorology," the high merit and originality of which last are somewhat veiled under its more obtrusive title—"Part 2 of the Report of the Chief Signal Officer of the [U.S.] Army for 1885." But these works are hardly suited for popular instruction; and for that large class of students whose mathematical acquirements are more limited, but who nevertheless desire to understand the movements and internal changes of the atmosphere, and to interpret them rationally in accordance with mechanical and physical laws, there has hitherto been little guidance, save such as they may obtain from casual references to them in works devoted to the general teaching of these sciences. It is perhaps in consequence of this divorce of the deductive from the inductive treatment of meteorological subjects that the contributions of English observers to the science of meteorology bear but an insignificant proportion to the labour expended on observational work, and that so much of this work is abortive, and practically of little value, owing to the absence of guiding and suggestive theoretical knowledge.

It is, then, with no ordinary degree of satisfaction that we hail the publication of Prof. Ferrel's treatise, the title of which heads this notice. As the originator and discoverer of many of the most important problems dealt with in these pages, no one could be better fitted to explain them in terms suited to general comprehension, and this task he has performed with a completeness and lucidity which leave but little to be desired. The work is, as it professes to be, a "popular" treatise, but popular only in the higher sense of the word. A system of movements so complex as those of the earth's atmosphere cannot be made clear to anyone who is not capable of following a chain of close reasoning, or who is not prepared to bring to the study that concentrated attention that is requisite to master any problem in deductive science. But, these being granted, no further demand is made on the student than some familiarity with the elements of algebra, and the simplest conceptions of plane trigonometry and kinetics. The action of the mechanical and physical forces that determine and regulate the wind system of the globe is clearly explained in the first two chapters of the work.

The most important and original portion of the book is that which deals with the general circulation of the atmosphere, in relation to which the cyclones and anticyclones that cause the vicissitudes of local weather are but matters of subordinate detail. The magnitude of the work achieved by Prof. Ferrel in this field has hitherto been recognized only by the few. It is not too much to say that he has done for the theory of atmospheric circulation that which Young and Fresnel did for the theory of light; and that the influence of his work is not more generally reflected in the literature of the day, must be attributed to the want of some popular exposition of the theory.

Starting with the fundamental conditions of a great temperature difference between equatorial and polar regions and a rotating globe, and postulating in the first instance a uniform land or water surface, it is shown how the convective interchange of air set up by the former must result in producing two zones of maximum

pressure in about lat. 30° in both hemispheres, two principal minima at the poles, and a minor depression on the equator, together with strong west winds in middle and high latitudes, and an excess of easterly winds in equatorial regions. The two tropical zones of high pressure determine the polar limits of the trade winds, and the whole system oscillates in latitude with the changing declination of the sun. Further, as a consequence of the fact that the great mass of the land is restricted to the northern hemisphere, whereas the southern hemisphere presents a comparatively uninterrupted sea surface, on which the retarding friction is less than in the northern hemisphere, the west winds of middle and high latitudes are much stronger in the latter than in the former, and by their lateral pressure cause a slight displacement of the tropical zones of high pressure and the equatorial zone of low pressure to the north of their normal positions on a hypothetical uniform terrestrial surface.

The great modification and extension of Hadley's theory thus introduced by Prof. Ferrel depends mainly on two points of the first importance. By all previous writers it was assumed that a mass of air at rest relatively to the earth's surface on the equator, if suddenly transferred to some higher latitude—say, *e.g.*, 60°—would have a relative easterly movement in that latitude equal to the difference of rotary velocities on the equator and on the 60th parallel, or about 500 miles an hour, the difference being proportional to that of the cosines of the latitudes. This, however, would be true only in the case of rectilinear motion. In reality, as Prof. Ferrel was the first to demonstrate, the mass of air would obey the law of the preservation of areas, like all bodies revolving under the influence of a central force, and its relative eastward velocity in latitude 60° would be 1500 miles an hour, being as the difference of the squares of the cosines. If, on the other hand, any mass of air at rest in latitude 60° were suddenly transferred to the equator, it would have a relative westerly movement of 750 miles an hour, and any mass of matter whatever moving along a meridian is either deflected—or if, like a railway train or a river between high banks, it is not free to yield to the deflecting force, presses—to the right of its path in the northern, and to the left in the southern, hemisphere.

The second point first established by Prof. Ferrel is that, in virtue of centrifugal force, this deflection or pressure to the right in the northern, and to the left in the southern, hemisphere is suffered in exactly the same degree by bodies moving due east and due west, or along a parallel of latitude, and therefore also in all intermediate azimuths.

From the first of these principles it will be readily seen why the west winds of middle latitudes are so much stronger than the easterly winds of the equatorial zone; and from the second, how these opposite winds, by their mutual pressure, produce the tropical zones of high barometer and the polar and equatorial regions of low barometer.

In subsequent chapters are discussed the modes in which the general circulation of the globe affects the climates of different latitudes by determining the distribution of rainfall in wet and dry zones, and inequalities of temperature through the agency of marine currents. Also the causes that modify and disturb the regularity of the ideal system, the chief of which is the mutual interaction of expanses of land and sea. The general excellence of these demonstrations is indisputable, but we have marked one or two passages which appear to us to be of doubtful validity, and which we recommend to the reconsideration of the author when the time comes, as we doubt not it will ere long, for the issue of a second edition of his work.

The first point to which we would take exception is what seems to us the too great influence ascribed to mountain-chains in deflecting the great atmospheric currents. That they deflect the surface winds, like other irregularities of the surface, and in proportion to their magnitude, is, of course, a matter of universal experience; but, in the absence of other causes operating to produce a diversion of the greater currents, their action in this respect appears to us to be merely local. As an instance we will take the case of the Western Ghats of India, an escarpment from 3000 to 7000 feet in height, running athwart the direction of the summer monsoon of the Arabian Sea. The wind charts of the Arabian Sea, issued by the Indian Meteorological Office, show no appreciable deflection of the monsoon wind on the windward face of this range; and if the same cannot be asserted of the corresponding wind in the north of the Bay of Bengal, where it impinges on the coast range of Arakan, it is evident that the deflection of this current to north, and eventually to north-west, is caused by the indraught towards the heated plains of Northern India.

We believe that a similar explanation will be found to hold good in all the more conspicuous cases cited by Prof. Ferrel. Thus, at p. 183 he says:—

"The air of the lower strata of the atmosphere in the trade-wind zone of the North Atlantic, having a westerly motion, and impinging against the high table-lands and mountain-ranges of Mexico, is deflected around towards the north over the south-eastern States, and up the Mississippi valley into the higher latitudes, where it combines with the general easterly flow of these latitudes, and adds to its strength. This completely breaks up the continuity of the tropical calm belt and dry zone, so that, instead of a dry region with scanty rainfall, such as is found in North Africa, Arabia, Persia, Beloochistan, and Cabul, we have on the same parallels in the southern and eastern United States a region of abundant rainfall, and all the way up the Mississippi valley and in the interior of the continent there is much more rain than in the interior of Asia."

Taking this passage as it stands, or only together with the immediate context, it might be understood to imply that the author ascribes this great diversion of the winds of the Gulf of Mexico, together with all the rainfall they bring to the southern States of America, solely to the influence of the comparatively low mountain-chain of Central America. That such, however, is not his meaning is evident from his subsequent remarks on p. 215, where, in describing the monsoons of North America, after noticing the high temperature of the land area in summer, he says:—

"On the southern and south-eastern coast, in connection with the deflection referred to [in the passage quoted above], it causes the prevailing winds to be southerly and south-easterly, instead of north easterly, as they would otherwise be in these trade-wind latitudes."

In point of fact, as may be seen on Dr. Hann's charts for January and July, in the new edition of Berghaus's "Physical Atlas," the diversion of the trade-winds of the Gulf of Mexico, northward up the Mississippi valley takes place only in the summer, and is an effect of the same agency, viz. the heating of the northern continents, that breaks up the high-pressure zone of the northern tropic into two anticyclones, one in each of the great oceans, and it is the juxtaposition of the Atlantic anticyclone and the Mexican cyclonic depression that determines the course of the winds and the resulting rainfall. To judge from the case of the Western Ghats, we think it may be safely concluded that, if there were no mountain-chain to the west of the Gulf, the results would not be greatly different. All the other instances quoted, illustrative of the diversion of great currents by mountain-chains, except such as are purely local, appear to us to be really due to other and similar causes.

In treating of the monsoons, Prof. Ferrel points out with perfect justice that their strength depends on the form of the land, and that they blow strongly only where the interior of the country is high and mountainous. But when he adduces Persia as an illustration of the negative case, we are unable to admit its relevancy. At p. 199 he observes:—

"In accordance with the preceding view of the principal cause of monsoons and land and sea breezes, it is seen from observation that all the great monsoons and the strongest land and sea breezes are found—the former in countries and on oceans adjacent to high mountain-ranges, and the latter along coasts with high mountains in the background. Neither the heated interior in summer of the Great Sahara of Northern Africa, nor of Arabia and Persia, which is considered the warmest region on the globe, causes, during this season of the year, any great indraught of air. It is true that at this season the north-westerly winds prevail a little more on the north-west coast of Africa and the ocean adjacent, due, no doubt, to the influence of the highly-heated desert of the Sahara; but over Arabia and Persia the north-west winds continue to blow almost incessantly, during June and July, away from the interior toward the Arabian Sea. . . . The monsoon influence, therefore, of countries mostly level, without an elevated interior, however highly they may become heated in summer or cooled in winter, is not very great."

But the interior of Persia is a part of the great tableland of Iran, and, to quote the description of Sir Oliver St. John, "its average height above the sea may be about 4000 feet, varying from 8000 or higher in certain of the outer valleys to not more than 500 in the most depressed portions of its centre." Its average elevation is therefore much greater than that of the interior of India, very much greater than that of the Indo-Gangetic plain, which is the goal of the Indian monsoon, and, as a glance at the map will show, it is not deficient in mountains. The explanation of the fact that, instead of attracting the monsoon from the Arabian Sea, it is itself swept by north-west and west winds—blowing, not, indeed, towards the Arabian Sea, but towards the lower Indus valley—must then be sought for elsewhere. The true explanation appears to us to lie in a combination of causes. Partly, perhaps, in the latitude, which brings it within the zone of the strong easterly current of extra-tropical regions, which, by its right-handed pressure, must resist any indraught from the Arabian Sea; but chiefly in the fact that any tendency that the heated highlands of Persia may have to create such an indraught is overborne by the stronger set towards India. For the latter country reaches far down into the tropics, and the centre towards which the monsoon blows must be determined by the resultant of all the temperature gradients of the whole heated region. An eastward direction having been given to the monsoon at the outset, its strength in that direction is greatly increased by the energy set free in the Indian monsoon rainfall.

This question is one of more than theoretical importance. These west winds of Persia and Afghanistan are the dry winds of Northern and Western India, and when they prevail beyond their normal limits, over the north of the Arabian Sea and a great part of India itself, to the exclusion of the rain-bearing current, they bring the drought and consequent dearth that have made India so disastrously notorious for its famines. Possibly, the explanation of their abnormal extension may be looked for in those oscillations of the great polar cyclonic systems to which Prof. Ferrel alludes at p. 339 of his work.

Cyclones and tornadoes are treated at great length, each of these subjects occupying more than one hundred pages of the book; and in connection with the latter is given the author's theory of the formation of hail, a subject which has hitherto been less understood than almost any other phenomenon of the atmosphere. It will be best given in the author's own words:—

"In the ascending current of a tornado, as in that of the equatorial calm belt, or of a cyclone, the rain-drops are formed down in the cloud region, and carried upward until they become too large to be supported by the current and so fall to the earth. . . . In a tornado, however, the ascending current is often so strong that the rain is supported until, by the blending of the small drops by coming in contact, very large drops are formed, and the strong ascending currents often extend so high that these large drops are carried away up into the region of freezing temperature. . . . There they are frozen, and after having been carried up and outward above to a distance from the centre, where the ascending current is not strong enough . . . to keep them up, they slowly descend, and receiving additions of ice as they fall, as long as their temperature remains below zero, . . . they finally fall to the earth as solid hailstones"

The concentric coatings so commonly observed in large hailstones are explained by these hailstones being carried again and again into the vortex by the strong indraught in the lower part of the storm-cloud, the theory being that every hail-cloud is a tornado, although it may not reach down to the lower atmosphere. The vapour being condensed as water in the lower part of the vortex, which is frozen at a higher level, and as snow in the upper part, each pair of coatings indicate an additional ascent through the storm-cloud. This view, which, even at first sight, seems far more reasonable than any previous theory, has received unexpected confirmation from the experience of more than one adventurous balloonist, more especially that of Mr. John Wise, whose fate it was to be drawn seven times successively into the vortex of a hail-cloud, and carried up repeatedly until the balloon was thrown out at the top. The account is, unfortunately, too long for extracting.

From what has been said, it will be apparent that Prof. Ferrel's book enters very fully into the many important topics enumerated in the title. Indeed, its subject-matter covers very much of the ground of which modern meteorology usually takes cognizance, and in the thoroughness of its treatment we know of no modern work in our language that can be brought into comparison with it.

H. F. B.

A NEW ATLAS OF ALGÆ.

Atlas deutscher Meeresalgen. Heft I. Von Dr. J. Reinke (Berlin: Paul Parey, 1889).

THE German Government, operating through the Kommission zur wissenschaftlichen Untersuchung der deutschen Meere, has undertaken to bear the cost of producing this sumptuous "Atlas" in the interests of fishery, and students of phycology have to thank an economic aspect of their study for a very remarkable addition to the literature of it. Similarly, we are indebted to the United States Fish Commission for the publication of Prof. Farlow's "New England Algæ."

It may be said at once that Dr. Reinke's "Atlas" is a success in every way, its level being that of Bornet and Thuret's "Études Phycologiques." From the point of view of *technique*, the plates are splendidly done, and the rest of the publication is worthy of them. This first part contains twenty-five quarto plates, and the text belonging to them consists of descriptions of the Algæ figured and special descriptions of the illustrations. Speaking not merely from an inspection of the book, but from a knowledge of the material of much of it communicated by Dr. Reinke to the British Museum, I do not hesitate to state that every one of these figures has great value to phycologists. They are not mere portraits of Algæ, taken from specimens more or less at haphazard, as is too much the fashion, but they represent faithfully characteristic stages in the development of the organisms in point. What is commonly termed "microscopical detail" fills the "Atlas," and one can hardly imagine it better done. In this portion the author (who has had the assistance of Dr. F. Schütt and P. Kuckuck) deals prominently with the Phæophyceæ, which, it is well known, are his particular study at present. Many of them are types of his own discovery, and generally unknown to workers in this field until this satisfactory introduction to them. Since they are of special importance to our native phycologists as Algæ of the North Sea and Baltic, a list is given of them :—

Halothrix lumbricalis, Kütz., *Symphoricoccus radians*, Rke., *Kjellmania sorifera*, Rke., *Asperococcus echinatus*, Mert., var. *filiformis*, Rke., *Ralfsia verrucosa*, Aresch., *R. clavata*, Carm, *Microspongium gelatinosum*, Rke., *Leptonema fasciculatum*, Rke., var. *uncinatum*, var. *majus*, var. *flagellare*, *Desmotrichum undulatum*, J. Ag., *D. balticum*, Kütz., *D. scopulorum*, Rke., *Scytosiphon pygmæus*, Rke., *Ascocyclus reptans*, Cr., *A. ocellatus*, Kütz., *A. balticus*, Rke., *A. fœcundus*, Strömf., var. *seriatus*, Rke., *A. globosus*, Rke., *Ectocarpus sphæricus*, Derb. et Sol., *E. Stilophoræ*, Cr., *E. repens*, Rke., *E. ovatus*, Kjellm., var. *arachnoideus*, Rke., *Rhodochorton chantransioides*, Rke., *Antithamnion boreale*, Gobi, var. *balticum*, Rke , *Blastophysa rhizopus*, Rke., *Epicladia Flustræ*, Rke., *Cladophora pygmæa*, Rke., *Pringsheimia scutata*, Rke.

It may be anticipated that a fair number of the novelties among these so-called "German Algæ" (the title reminds one of the "Protestant trout") may be found on our own coasts.

It should be mentioned that more systematic detail with reference to many of these is to be found in the author's "Algenflora des Westlichen Ostsee" (Berlin, 1889).

The author very properly calls attention to the fundamental importance of a thorough knowledge of marine Algæ to fishery, since the plant world prepares by its organs of assimilation the food of the animal world in the sea. The German Commission deserve the highest praise for the enlightened view of their functions embodied in this undertaking, and no biologist will grudge the warmest encouragement to Dr. Reinke in his work. It is anticipated that the book, when complete, will contain a hundred plates, with the accompanying text. In these days, when the most unmitigated rubbish frequently comes to us with highly pretentious illustrations, the student has learned to be on his guard against "prepossessing appearances." No *plate manufacture*, however, can produce the welcome impression of weight and importance stamped on this "Atlas," gained to a great extent by the fact that Dr. Schütt and Herr Kuckuck, who have drawn the plates, have given us the work of skilful botanists, and not that of draughtsmen only.

G. M.

OUR BOOK SHELF.

Die mikroskopische Beschaffenheit der Meteoriten erläutert durch photographische Abbildungen. Von G. Tschermak. (Stuttgart: E. Schweizerbart'sche Verlagshandlung [E. Koch], 1883–85.)

Die Structur und Zusammensetzung der Meteoreisen erläutert durch photographische Abbildungen geätzter Schnittflächen. Von A. Brezina und E. Cohen. (Stuttgart: E. Schweizerbart'sche Verlagshandlung [E. Koch], 1886–87.)

Die Meteoritensammlung des k. k. mineralog. Hofkabinetes in Wien. Von A. Brezina. (Wien: Alfred Hölder, 1885.)

THE above three works together provide for the student a rich treasury of information relative to the characters of meteorites. The first two illustrate, by the aid of photography, the structure and composition of the more typical meteoric stones and irons respectively. The work dealing with the meteoric stones is complete in three parts, including 25 large plates, and has been undertaken by Prof. Tschermak, who had charge of the Vienna Collection of Minerals from 1869 to 1877. Of that which relates to the meteoric irons only two parts have as yet appeared, but they comprise no fewer than 24 large plates: it is undertaken jointly by Dr. Brezina, who succeeded Prof. Tschermak in the keepership of the Vienna Collection, and by Prof. E. Cohen, of Greifswald, whose series of micro-photographs of sections of terrestrial minerals and rocks is so well known.

Photography has rarely been applied to a more satisfactory purpose than the multiplication of exact representations either of transparent meteoritic sections, or of etched meteoric irons as seen with the unassisted eye or when magnified by means of the microscope. Meteoritic falls are rarely so large that the market is flooded with

illustrative specimens; and, indeed, a good collection of typical meteorites is inaccessible to most students. But, further, meteoric irons are very prone to deteriorate, through oxidation, and the perpetuation of the characters of a freshly etched face is thus especially to be desired. The excellence of the photographs is beyond all praise. The details, whether of the chondritic structure or of the Widmanstätten figures, are most beautifully shown. A brief description of the salient features of the sections is furnished with each plate.

The third work is nominally a Catalogue of the Vienna Meteorites, but, by reason of the completeness of that collection, is virtually a survey of the petrographical characters of the meteorites of all the known falls. The classification adopted is in the main that suggested by Gustav Rose in 1864, and developed by Tschermak in 1872 and 1883. The detailed description and definition of the groups is preceded by a history of the Vienna Collection, and also by a sketch of the various theories which have been proposed relative to the mode of formation of meteorites. As a result of his microscopical researches, Dr. Brezina supports the view that the structural features of meteorites are due to hurried crystallization, and not to a slow agglomeration of fragmentary matter. Dr. Brezina adds a chronological list of the meteorites preserved in the known collections, and also a lengthy index of names, synonyms, and localities. The work extends over 126 pages, and is accompanied by four plates. L. F.

Introduction to Chemical Science. By R. P. Williams A.M., and B. P. Lascelles, M.A., F.C.S. (London: Ginn and Company, 1889.)

THERE could hardly be a more concise and well-digested summary of elementary chemical principles and applications than that contained in this work. It is a manual intermediate between the natural philosophy primer and the minute and detailed text-book, and fills the gap pointed out in the Report on Chemical Teaching of a British Association Committee in 1888. Hence, as an outline of chemical science to be filled up in greater detail from larger works, and as an introductory text-book, this volume will be found exceedingly useful. The experiments described are such as should be performed by everyone beginning the study of chemistry, and would also serve as an excellent introduction to a course of qualitative analysis. In addition to the treatment of metals and non-metals, the work includes chapters on organic chemistry, and others on photographic chemistry, the chemistry of rocks, and electro-chemistry. Indeed, Mr. Williams, the author of the American edition, and the reviser, Mr. Lascelles, may claim to have produced a most comprehensive little work, and one deserving considerable commendation. •

The Cradle of the Aryans. By Gerald H. Rendall, M.A. (London: Macmillan and Co., 1889.)

THE question as to the primitive home of the so-called Aryan race has lately excited so much interest that many students must have wished for a short and clear account of the controversies relating to the subject. This is exactly what Prof. Rendall supplies in the present essay, the substance of which was originally communicated to the members of the Liverpool Literary and Philosophical Society. Prof. Rendall accepts Penka's theory that the Aryans were a European people who, at the close of the glacial epoch, followed the ice northwards, and settled in Scandinavia; and that Scandinavia was the centre from which, at various subsequent periods, groups of the Aryan race were dispersed. All the arguments marshalled by the German writer in favour of this hypothesis are here briefly and effectively stated. The philological part of the case is presented in a more scholarlike spirit by Prof. Rendall than by Penka himself, whose rash philological conjectures have prevented a good many people from doing full justice to the weight of his anthropological and ethnological evidence.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Mr. Cope on the Causes of Variation.

MR. E. D. COPE'S letter in NATURE of November 28 (p. 79) is a fair sample of his writings on biological theory, in so far as I am acquainted with them.

Mr. Cope proposes to teach Mr. Wallace and others the first principles of both logic and biology. The tone of his letter encourages a similar frankness in reply. Mr. Cope must not take it amiss when he is charged with two of the gravest faults of which a critic can be guilty—namely, complete misapprehension of the matter which he is attempting to criticize, and no less complete ignorance of the recognized and elementary facts of the branch of science to which that particular matter relates. I do not hesitate to assert that Mr. Cope puts forward an argument which could not possibly be entertained by anyone who is acquainted with the most notorious and admitted facts of heredity and variation. I venture to express myself thus emphatically, because it is a matter for sincere regret that American biology should at this moment be identified with what is sometimes called "a school of philosophy" which owes its distinction to a deliberate ignoring of the writings of Mr. Darwin. By all means let us have discussion and criticism of Mr. Darwin's conclusions, but let it be understood that those who enter upon such discussion have at any rate an elementary acquaintance with the works of Mr. Darwin himself, if not with those of Weismann and Wallace; otherwise, much time and much of your valuable space will be wasted.

That Mr. Cope has not the necessary elementary acquaintance with the admitted facts of heredity and variation will appear from what follows. The discussion in which he has intervened is one as to whether certain structural peculiarities exhibited by flat-fish are due to the transmission to their offspring of a form and position of parts *acquired* by muscular efforts by the ancestors of flat-fish, or whether these given structural peculiarities suddenly appeared in the ancestors of flat-fish as a "congenital variation" having no adaptive relation to any efforts or experiences of a preceding generation, and were advantageous to their possessors, so that the individuals thus born were favoured in the struggle for existence, survived to maturity, and transmitted their peculiarity to some of their offspring with such intensification as is found experimentally to be the result of breeding from parents both of which possess a given congenital peculiarity.

The question raised is, in short, whether in this case Lamarck's *hypothesis* of the transmission of acquired characters is the necessary explanation, or whether the case can be explained by the action of the *known* causes (not hypothetical causes) on which Mr. Darwin founded his theory of the origin of species, viz. the occurrence of congenital variations unrelated to any like variations in parents or ancestors, and the selection and intensification of such variations in subsequent breeding. There has been here no ambiguity—such as unfortunately arises sometimes when like questions are discussed—as to the sense in which the term "acquired characters" is used. It is clear enough that by the "acquired characters" of a parent we do not mean characters congenital in the parent, but expressly exclude them; it is clear that we refer on the contrary (as did Lamarck) to new characters acquired by the parent as the direct consequence of the action of the environment upon the parental structure, and exhibited by that parent as definite measurable features.

Now let us consider Mr. Cope's contribution to the discussion. He accuses Mr. Wallace—who is one of those who refuse to adopt Lamarck's gratuitous hypothesis of the transmission of acquired characters—of being guilty of the sin of "non-sequitur" and "paralogism." He then proceeds to make a general statement, the truth of which neo-Darwinians (or post-Darwinians, or anti-Lamarckians), in common with all men, recognize,

although Mr. Cope offensively implies that they do not, viz. "Selection cannot be the cause of those conditions which are prior to selection: in other words, a selection cannot explain the *origin* of anything." How can Mr. Cope presume to tell us this? Who has ignored it? when? and where? Mr. Cope does not seem to be aware of the fact that the anti-Lamarckians attach great importance to the existence of congenital variation, that Darwin himself has written at length on the subject, and that Weismann has developed a most ingenious theory as to the relation of fertilization and its precedent phenomena to this all-important factor in evolution.

Mr. Cope puts aside all that has been done on that subject, or else is ignorant of it, and calmly lays down the following proposition: "If whatever is acquired by one generation were not transmitted to the next, no progress in the evolution of a character could possibly occur. Each generation would start exactly where the preceding one did." The full significance of this sentence can only be apprehended when it is understood that Mr. Cope believes that progress in the evolution of a character *does* occur. The statement therefore amounts to this: (1) that whatever is acquired by one generation is transmitted to the next; and (2) that the only possible explanation of the fact that a new generation does not exactly resemble its parents at a corresponding age is that the parental generation has transmitted to its offspring particular features acquired by it between birth and maturity.

I doubt whether Mr. Cope will find any other naturalist—even the most ardent Lamarckian—to join him in these assertions.

With regard to the first, it is hardly necessary to say that it has never yet been shown experimentally that *anything* acquired by one generation is transmitted to the next (putting aside parasitic diseases); and as to *everything* ("whatever") being so transmitted, every layman knows the contrary to be true. Children are not born with the acquired knowledge of their parents. If there were no other explanation offered of offspring varying from their parents at a like age than the hypothesis of transmission of characters acquired by the parents on their way through life by the action of the environment, this hypothetical explanation would still be quite insufficient to account for the fact that the individuals of one brood vary enormously as compared with one another, a fact which points to the individual germs (egg-cells and sperm-cells) as the seat of the processes which result in variation, and not to the parental body which is the common carrier of them all. Assuredly these broods demonstrate that *all* the acquired characters are not transmitted to *all* the offspring.

With regard to the second proposition which Mr. Cope's statement contains, experimental fact is directly opposed to its truth. As cited by Darwin on p. 8 of the first edition of the "Origin of Species," Geoffroy St. Hilaire showed that "unnatural treatment of the embryo causes monstrosities; and monstrosities cannot be separated by any clear line of distinction from mere variations." Mr. Darwin himself was "strongly inclined to suspect that the most frequent cause of variability may be attributed to the male and female reproductive elements having been affected prior to the act of conception." What he meant by "being affected" is explained at greater length in the "Animals and Plants under Domestication," where, in chap. xxii., there is a long discussion of the causes of variability, the conclusions of which are supported by an array of observed facts which Mr. Cope cannot be permitted to ignore at his pleasure. Mr. Darwin there gives solid reasons (as was his wont) for holding that variability results from the conditions to which the parents have been exposed: changes of any kind in the conditions of life, even extremely slight changes, often suffice to cause variability. But Mr. Darwin's examination of the facts did not lead him to conclude that the bodily characters acquired by the parents as the result of changes were those which manifested themselves as variations in the offspring. On the contrary he showed that the effect of changed conditions, of excess of nutriment, and of the crossing of distinct forms, is a "breaking down," as it were, of the hitherto fixed characters of the race, leading to the reappearance of long-lost characters and to the appearance of absolutely new characters, the new characters having no more (and perhaps not less) relation to the exciting cause which acted through the parent than has the newly-formed pattern in a kaleidoscope to the tap on the kaleidoscope tube which initiated the rearrangement.

For Mr. Cope to complain of the methods of reasoning of post-Darwinians, and at the same time without any reasoning at all to assert (as he does, not directly but by implication) that there is no such thing as "congenital variation" or "sporting," is not quite satisfactory. When it is asserted that every feature by which a young animal differs from the structure of its parents at a corresponding age must have been acquired by one or other of the parents as actual structural features, and so transmitted as an acquired character to the offspring, the whole world of fanciers, horticulturists, farmers, and breeders, is ready with its unanimous testimony to contradict the assertion.

Let me say, in conclusion, that, as Mr. Wallace has pointed out, Mr. Darwin did not consider that variability in a state of nature was either so general or so wide in its range as later observations and reflections lead us to believe it to be. Mr. Darwin studied those causes which are found by practical gardeners and breeders to be favourable to excessive variation in animals and plants under domestication. He showed clearly that the resulting variations had no adaptive relation to the exciting causes, and were manifested in the structure at birth of a new generation, and not in that of the generation subjected to the exciting cause. No one has yet been able to give an adequate account of the frequency and range of variation of any number of animals or plants in a state of nature, because natural conditions destroy, on the average, all individuals born of two parents—except two—before maturity is reached, and those two are naturally selected in consequence of their adhesion to the specific type.

There can be no doubt from a consideration of the facts cited by Darwin that, whilst variation often is reduced to a *minimum* in natural conditions which remain constant, natural variations of conditions can and do occur, which excite the germ-cell and sperm-cell, or their united product, to vary as in conditions of domestication. There can be no doubt that there was in Mr. Darwin's mind the conception of a definite relation between two effects arising from changed conditions: the one being the disturbance of the equilibrium of the organism and its consequent production of variations; the other being the new requirements for survival; in fact, there seems to be, as it were, at once a new deal and new rules of the game. It is not difficult to suggest possible ways in which the changed conditions shown to be important by Darwin could act through the parental body upon the nuclear matter of egg-cell and sperm-cell, with its immensely complex and therefore unstable molecular constitution, so as to bring about *variations* (arbitrary, kaleidoscopic variations) in the ultimate product of the union of the remnant of the twice-divided threads of the egg-nucleus with the nuclear head of a spermatozoon. The wonder is, not that variation occurs, but that it is not excessive and monstrous in every product of fertilization.. And yet Mr. Cope writes from the other side of the Atlantic to assert that there is no possible cause of departure from parental type in offspring, excepting that assumed in Lamarck's unproved, improbable speculation! E. Ray Lankester.

December 7.

Protective Coloration of Eggs.

Some years ago an idea similar to that of your correspondent, Mr. Grensted (November 21, p. 53), occurred to me, as regards the protective coloration of eggs; and, curiously enough, the red-backed shrike was one of the birds whose eggs I selected for special observation. My experience has been that the ground colour of these eggs is quite arbitrary. I fear that I cannot furnish data, as I ought; but I well remember that I found in Sussex a rather abnormally pale clutch of eggs in a very dark nest; and that I regarded this, at the time, as completely doing away with my hypothesis. The evidence that I got from other, less striking instances, told about equally for and against.

Another egg, whose variations I watched pretty closely, was that of the yellowhammer. Apart from differences of marking, the ground-colour of this egg varies from pure or pinkish-white, to a white rather deeply suffused with purplish-red or olive-brown. But in this case, again, the correspondence of colour between the egg and its surroundings could not be made out at all satisfactorily.

A pale and little-marked specimen of the egg of the spotted flycatcher, that was brought in to me one spring at Malvern, suggested to me that it would be worth while to observe the variations here also. But I again failed to arrive at any conclusion.

I am so strongly tempted unreservedly to accept the "protective" theory, that I perhaps lay too great stress on these negative instances. As a matter of fact, I suppose that the experience of a single individual is rarely large enough to justify any induction being made from it. I myself, for instance, have never come across the extreme variations of the cuckoo's egg, such as Seebohm figures. E. B. TITCHENER.

3 Museum Terrace, Oxford, December 3.

Is the Bulk of Ocean Water a Fixed Quantity?

MR. MELLARD READE's criticism is perfectly sound. If the bulk of the ocean water on the surface of the globe has always been the same, the oceans could not at any time have been shallower than at present without a decrease in the area of the land. Consequently, the supposition that in early geological times the area of the land was larger, and the depth of the oceans less, demands the further inference that the bulk of the ocean water was less then than it is now.

When writing on the physics of the sub-oceanic crust, I saw that this was a necessary consequence of the theory, but I was not then quite prepared to discuss it. I have since had some correspondence with Prof. A. H. Green and Mr. O. Fisher on the subject, and will briefly indicate the possibilities that have occurred to us.

The first suggestion made was that, if the solar radiation was greater in Palæozoic times, there would be greater evaporation, and as the temperature of the air would also be higher, the atmosphere could hold more aqueous vapour than it does now, so that we might suppose a part of the water which is now in the ocean to have been then permanently suspended above it. Mr. Fisher, in writing to me, admits this possibility, and even thinks it might be feasible to estimately roughly the amount of water so suspended if the mean temperature of the ocean at any period was known. But he says:—"I do not think you could get much diminution of the oceans in this way, for, suppose the present atmosphere to consist of nothing but aqueous vapour, then it would represent a layer of water about 30 feet thick evaporated from the earth's surface. Now, it seems hardly probable that at a former time there should have been an amount of aqueous vapour in the atmosphere so great that the mass of such additional vapour should equal that of all the oxygen and nitrogen and vapour now in the atmosphere; and even if there was this amount, it would take off only about 30 feet of water from the surface of the globe," or about 37 feet from the present surface of the oceans.

If, therefore, the bulk of the water on and above the surface of the earth has remained the same since the time when the crust was first formed, it seems difficult to find any means of sensibly diminishing the amount of water in the oceans. But need we make this preliminary assumption, and is it not really possible that there has been an increase in the bulk of surface-water, and not a decrease by absorption, as some theorists would have us think? May we not suppose, in fact, that water-substance has always existed in the interior of the earth, and may it not, by its constant and gradual escape, have always been adding to the bulk of the surface-waters?

This idea had occurred to Mr. Fisher so long ago as 1873, and the following passage occurs a paper then published (Trans. Camb Phil. Soc., vol. xii., Part 2 p. 431): "If such was the condition of the interior in the early stages of the cosmogony, a large portion of the oceans now above the crust may once have been beneath it"; and in the new edition of his "Physics of the Earth's Crust" he further discusses the manner in which this water-substance may be diffused through the magma of the liquid substratum beneath the crust.

As a matter of fact, it is well known that almost all volcanoes, when in eruption, emit large quantities of steam, and the presence of this steam has always been connected with the causes of volcanic activity. There are only two ways of accounting for the presence of this steam: (1) that water from the sea or from the rainfall gains access to the deep-seated foci of volcanic action; (2) that the water-substance is a primary constituent of the liquid magma below, and that when this material is forced up to the surface, the pressure which kept the water in solution or combination is removed, and it is blown off as steam.

As regards the first possibility, there are great difficulties in the way of supposing that surface-water can find its way to any region where the heat is sufficient to keep rock constantly in a liquid condition. It does seem possible that the access of water to the interior parts of a volcano *already established* may sometimes cause an eruption, and, under certain circumstances, an eruption of great violence; but the descent of water through the earth's crust to depths of 20 or 30 miles so as to be the initial cause of the establishment of volcanoes is not so easy to understand. The pressure of the superincumbent rocks at a depth of 2 or 3 miles must be so great that all cracks and interstitial spaces would be reduced to a minimum, and at the depth of 5 miles one would suppose that none such could exist. Several facts are known to geologists which show that all cracks diminish rapidly downwards. One such fact is that in many deep mines the throw of a *fault* diminishes with the depth to which it is followed. Another is the existence of such warm springs as those of Bath, the explanation of which is supposed to be that water percolating downward (say from the Mendips) reaches a depth at which there is less resistance to its travelling laterally than to its further descent, and that ultimately reaching a crack or fault, it is forced up this path of least resistance by the hydrostatic pressure of the descending stream.

It is true that a residuum of the water might continue its downward journey, being, as it were, slowly sucked downward as far as the minutest interstitial spaces extended; but what would happen when it reached the lower layers of the crust? Could it possibly reach and be absorbed by or dissolved in the semi-fused rock which must there exist? Captain C. E. Dutton has well expressed this difficulty. Referring to the high temperature which must exist at a depth of 5 or 6 miles, he says:—"At such a temperature the siliceous materials of which the rocks are composed are no longer hard and brittle as when they are cold, but viscous and plastic. . . . Now a crack or fissure might reach very far down into hard, cold, brittle rocks, but into soft semi-fused plastic rocks, never. Under a pressure of several miles of superincumbent strata, a crack, or even the minutest vesicle, would be tightly closed up as if its walls were wax or butter. A more perfect packing against ingress of water could not be conceived."[1]

Even capillary action could not come into play under such conditions as these.

Let us next consider the alternative theory suggested by Mr. Fisher. He claims that geologists furnish him with a certain amount of positive evidence for the idea that water is an essential constituent of the liquid magma from which the igneous rocks have been derived. Passing over the proofs of the existence of water in the crystals of volcanic rocks and in the materials of deep-seated dykes, let us come at once to granite, a rock which can only have been formed at great depths and under great pressures, and which often forms large tracts that are supposed to have been subterranean lakes or cisterns of liquid matter in direct communication with still deeper reservoirs. Now, all granites contain crystals of quartz, and these crystals include numerous minute cavities which contain water and other liquids; and the quartz of some granites is so full of water-vesicles that Mr. Clifton Ward has said: "A thousand millions might easily be contained within a cubic inch of quartz, and sometimes the contained water must make up at least 5 per cent. of the whole volume of the containing quartz." This amount only represents the water that has been, as it were, accidentally shut up in the granite, for some was doubtless given off in the form of steam which made its way through the surrounding rocks.

It is therefore generally conceded that granite has consolidated from a state of igneo-aqueous fusion, and that the liquid magma from which all granitic intrusions have proceeded contains water-substance. It is therefore only a step further to assume that this water-substance is an essential constituent of the liquid substratum, and to suppose that it has been there since the consolidation of the earth. That there is no inherent improbability in this supposition, and that it is not inconsistent with chemical views of cosmogony, Mr. Fisher has shown at the end of his chapter on the "Liquid Substratum."

I am only now concerned with it as an explanation of the secular increase in the bulk of the ocean waters which is demanded by my theory of the evolution of continents and oceans. We can prove from the geological records that volcanic action has always been in operation from the very earliest times in the world's history, and if it is true that such a reservoir of water-substance has always existed in the earth's interior, the continual volcanic eruptions must have constantly added water to the oceans on the earth's surface. Hence, as I stated in my

[1] "Volcanoes," by C. E. Dutton, in *Ordnance Notes*, No. 343, Washington, 1889.

first letter, we are at liberty to imagine a time when there was much more land than there is at present, and when all the oceans were comparatively shallow. A. J. JUKES-BROWNE.

Galls.

BEFORE rushing into arguments on this subject, it appears to me that more good might be done by entering into investigations of the physiological and morphological problems involved.

A gall-fly of a particular species inserts an egg in a certain position on a certain plant (oak, for instance). Another gall-fly of a different species inserts its egg almost in the same position on the same plant. But the results are totally dissimilar. An abnormal growth is set up, from irritation, in either case; but the nature of this growth is quite different. The initial irritation is set up by the presence of the egg, and in most gall-insects the egg *grows*—that is to say, it increases vastly in size before the larva is hatched. The irritation is continued by the larva, and the gall is produced, varying in form in accordance with the species of gall-fly that deposited the egg. But I want to know in what consists the difference in the active irritation that causes so great a divergence in the results? I am not aware that this has ever been answered. But I am quite sure it could be answered on purely physiological grounds if carefully studied. The answer would not in the least detract from the importance of the point as regards natural selection; but it might very materially modify speculative theories based on results only, without a precise knowledge of the agencies that produced those results. R. MCLACHLAN.

Lewisham, November 29.

ALTHOUGH I see no need of a better explanation than Prof. Romanes's (NATURE, November 28, p. 80) of the difficulty which galls seem at first sight to present for natural selection, yet I beg leave to say some words of further elucidation.

When it was said by Darwin ("Origin of Species," chap. vi.): "If it could be proved that any part of the structure of any one species had been formed for the exclusive good of another species, it would annihilate my theory, for such could not have been produced through natural selection," he evidently meant only species living without organic connection with each other, viz. his own example of the rattlesnake. The argument does by no means apply to organisms living in a relation of *symbiosis*, as is the case with gall-bearing plants and the larvæ inhabiting the galls.[1] Such associations form, as it were, one compound organism. Natural selection evidently may act in favour of each symbiont separately, provided only that the effect will not damage the other symbiont in such a degree as seriously to impair its existence. Some "disinterested" expenditure of energy and of organic substance is not excluded by natural selection, but may be promoted, if of advantage to the other partner. Thus the production of galls will scarcely do any serious injury to an oak, and even if such were sometimes the case, there would be no comparison to the damage worked, for instance, by Trichinæ, on the organism of man and animals, which hosts, nevertheless, in consequence of the stimulus caused by the parasite, afford the substance for capsules protecting the worms, just as plants produce manifold structures beneficial to the gall-insects. If Trichinæ would attack a species of mammals as frequently as, for instance, leaf-cutting ants attack some tropical plants, then those hosts would be forced either to develop, by survival of the fittest, some protection against their invasion, or they would succumb to the enemy and die out.

Analogous examples might be multiplied of both plants and animals, and it is especially to be remembered, as alluded to by Prof. Romanes, that the chemical activities of parasites, including the elaboration of ferments affecting the saps and tissues of the host, are as much under the guidance of natural selection as are their morphological variations. D. WETTERHAN.

Freiburg, Badenia, November 30.

WITH all due deference to your able correspondents Dr. St. George Mivart and Prof. G. J. Romanes, I cannot for the life of me understand how the theory of natural selection can be seriously assailed by investigations into the formation of galls by insects. Gall-formation has always appeared to me to be a pathological, that is a *perverted physiological* process, and to be due to the action of some animal irritant upon normal vegetable tissues during their period of active growth. These formations are therefore, to my mind, fairly on a par with the globular nests produced by the larvæ of the Œstrus, or bot-fly, in the hides of oxen; or to the inflammatory foci in the tissues of the kidneys, due to the translation of Bacilli, in the case of ulcerative endocarditis. Other examples bearing on the subject will doubtless occur to your readers. In all such instances we have certain changes in the cellular or protoplasmic tissue-elements of the host, brought about by the growth and development of a foreigner in their midst; and natural selection, in so far as it operates in such cases, seems to have sided mostly with the stranger, and to be to his advantage alone. That the host under these circumstances performs actions "which, if not self-sacrificing," are at least "disinterested," must be admitted; but it is the self-sacrifice of coercion and disinterestedness under compulsion. W. AINSLIE HOLLIS.

Brighton, December 1.

Luminous Night Clouds.

THE many inquiries and appeals regarding observations of luminous night clouds which have recently appeared in the columns of NATURE, and the growing importance of the subject, will justify me, perhaps, in sending to you, for publication in that journal, the following item, so long after the event it describes took place.

About the middle of November 1887, between eight and nine in the evening, as I was walking homewards from my day's work, I noticed what appeared to me to be the arch of a rainbow very low above the western horizon, and of a snow-white colour. A bank of clouds was rapidly approaching from the west, which, at the time of the first appearance of the arch, covered nearly half the sky, the eastern half being clear. The arch appeared to move eastwards, with and in the midst of the clouds, for it continually rose above the horizon, and, in the course of about half an hour, had approached the zenith.

At this time I called out several people to witness the phenomenon, which certainly presented a most extraordinary appearance. The arch appeared to be uniformly of about 3° or 4° in width, and extended north-north-east and south-south-west across the whole sky. The latter was about wholly overcast with the clouds at this time, except the arch, which presented a glaring brightness, and illuminated the earth with a weird splendour four or five times exceeding that of the brightest moonlight.

While at the zenith, the stars shone through the entire width of the arch with apparently more than ordinary brightness; but as the arch approached towards and receded from that point, the width of the transparency was observed to diminish rapidly with the distance, until at 10° or 15° on either side the stars were invisible through it.

The phenomenon appeared to be a division in the cloud stratum, the opposite walls of which were pretty clearly defined; and there appeared to be absolutely nothing between these opposite cloud walls but the purest air and the white light of the arch. I remember also that the wall or border of cloud on either side of the arch was slowly revolving upon an axis parallel with the arch; just as is often seen in the front bank of clouds of an approaching storm. But I do not remember the direction of the rotation, or whether both borders rotated in the same or in opposite directions.

The arch moved towards the east at about the same pace that it approached from the west, and with apparently the same width and direction of extension. There was no moonlight at the time, and only a gentle breeze was blowing. The weather preceding the phenomenon was fine for several weeks; but a few days afterwards, or on November 19, there was a sudden and extraordinary fall of the temperature, accompanied by some snow and very high wind.

I have thought that possibly this phenomenon might throw some light on the subject of luminous clouds, and that this tolerably accurate description of it may therefore be of interest to the students of that subject. I may add, however, that the luminosity of the arch did not appear to proceed directly from the clouds themselves, but from the clear space between the

[1] Darwin's thorough acquaintance with these important structures is shown by his elaborate discussion in "Animals and Plants under Domestication," chap. xxiii. (2nd ed. vol. ii. p. 272). It is particularly to be noted that Darwin insists on the accordance of galls, for instance, on roses, with structures arising through bud-variation.

clouds; although, according to the best of my recollection, luminous filaments seemed to extend from the clouds for a short distance into the span of the arch. EVAN MCLENNAN.

Brooklyn, Iowa, U.S.A., November 22.

Electrical Figures.

I RECENTLY noticed a pretty form of electrical discharge, which has probably been described before, but was new to me. Perhaps one of your readers will be able to refer us to an account of it.

The poles of a Voss machine are put very near together: a plate of ebonite $\frac{1}{16}$ inch thick is placed between them. As the machine works, a succession of delicate ramified discharges run over both surfaces of the plate: they are bright green, and each crooked line is discontinuous—a series of dashes, as if stitched out in silk, now above and now below the surface.

Winchester College, December 6. W. B. CROFT.

NEW DOUBLE STARS.

THE highest quality of seeing, as of acting or of thinking, needs initiative. A mental impulse is the spring of discovery, even by a purely visual process. The mind prompts the eye, interprets what it suggests, bodies out its semi-disclosures. So that to perceive what has never been perceived before is, in a sort of way, an act of *invention*. It thus happens that an accurate is not always an original observer. Novelties, as such, are almost inaccessible to many persons with exquisite powers of vision for whatever is already known to be within its range.

The late Baron Dembowski was an example of a first-rate observer but slightly endowed for detection; Mr. Burnham, on the other hand, is a born discoverer. The accidents of his career have turned his attention almost exclusively to double stars; and his glance seems to have a compulsive power of turning simple into compound objects by long and intent looking. His Chicago thousand of new pairs are famous; he bids fair to accumulate an equally imposing array at Lick. Nor does he neglect the old in the search for the new. The more exciting is not permitted to exclude what is in many respects the more useful occupation.

Progress in double-star astronomy is absolutely dependent upon remeasurements of the relative positions and distances of known pairs. We can otherwise learn nothing as to the nature of their connection. Inquiries about them can, by this means alone, be pushed through the three successive stages leading up towards complete knowledge. In the first place, it has to be decided whether the stars shift their places perceptibly with reference one to the other. If they are "fixed," but with a common proper motion, then they may safely be set down as physically coupled, although centuries may elapse before the character of their mutual revolutions becomes apparent. In the next place, the nature of relative motions, where they exist, has to be ascertained. Should they prove to be rectilinear, that fact alone overthrows the possibility of any real connection between the stars. Each pursues its way independently of the other. Finally, in the interesting cases in which curvilinear motion shows itself, persistent micrometrical measures are required to determine the shape and period of the orbit traced out.

Yet the majority of these objects receive little or no attention. This is in part due to their great numbers. About 12,000 double stars—using the term in the widest sense—are now known; nearly 5000 are in really close conjunction—so close, in some 1400 instances, as to render the chances of accidental juxtaposition all but evanescent. Only between fifty and sixty stellar orbits have, however, as yet been computed, and many of them from most inadequate data. The truth is, that this branch of work wants organizing. It is too vast and too important to be abandoned to the capricious incursions of irresponsible amateurs, whose industry is often wasted by being misapplied. There ought, nevertheless, to be little difficulty in distributing the observational resources available as advantageously as possible by the intervention of some recognized authority, a central repository being at the same time constituted whence computers could obtain on demand the materials needed for the investigation of particular systems. The tasks of stellar astronomy are so multitudinous as imperatively to demand combination for their effectual treatment.

Discovery, meanwhile, must advance as it can. It is far from desirable that it should remain stationary. Although our acquaintance among double stars is already embarrassingly large, we cannot refuse to extend it. Every addition to it, indeed, is, for a variety of reasons, to be welcomed.

Information on the general subject of stellar compositeness can only be gained by continually widening the area of research. The comparative frequency of its occurrence can thus only be estimated. Struve found one in forty of 120,000 stars examined by him down to 1827 to be compound; but the proportion was naturally higher for the brighter stars, as being in general much nearer the earth, and consequently of more facile optical separation. Every twenty-fifth star in Piazzi's Catalogue, every eleventh in Flamsteed's, proved accordingly to have a companion within less than 32″. But the process of dividing stars has since made such strides as to show that the real preponderance of single over double ones must be much smaller than these numbers indicate. Perhaps, indeed, no star can be called absolutely single. Between a small companion sun and a large planet in its self-luminous stage it is not easy to establish a distinction. The star we know best may not always have been, in its "surpassing glory," so undeniably solitary as it now is. Jupiter, if it ever shone with anything like stellar lustre, would have constituted with it a fine unequal pair such as are plentifully exemplified in our catalogues.

The distribution of double stars is characterized by a somewhat irregular condensation towards the Milky Way. They abound in Cygnus and Lyra, are scanty in Cassiopeia and Cepheus; while Struve met with rich regions where lucid stars are few, in Auriga, Telescopium, and Lynx. Burnham, however, could detect no marked local preferences among his numerous pairs. Sir John Herschel was struck with the paucity of close doubles in the southern hemisphere; but no searching scrutiny has yet been carried out there with modern instruments.

The curious tendency of stars already in close association to split up still further when sufficiently powerful means are brought to bear upon them, has been strongly accentuated by Mr. Burnham's investigations. Primaries with double satellites, such as Rigel, or satellites with double primaries, such as ξ and β Scorpii, swarm on his lists. A fresh instance of the former kind is ζ Piscium (Σ 100), registered by Struve as somewhat widely double, but found to be triple last autumn with the Lick twelve-inch achromatic. The satellite of Struve's companion, at an interval of less than one second from it, is of the eleventh magnitude. The bright stars are estimated by Burnham as of sixth and eighth, but were photometrically determined at Harvard as of 5·4 and 6·4 magnitudes; and Webb thought that the chief of the pair occasionally rose to the fourth rank of lustre. A presumption is thus afforded that both fluctuate in light. Their spectrum, like that of most variable double stars, is of the Sirian type; and their real fellowship is made manifest by a community of proper motion. We have here, then, a genuine ternary system.

Aldebaran is the centre of a mixed group. A small star at 30″ detected by Mr. Burnham at Chicago on October 31, 1877, was described by him as making with the ruddy bright star, a pair resembling Mars and his outer satellite (*Astr. Nach.*, No. 2189). A drift together through space

is probable, Mr. Burnham's remeasurements after eleven years indicating relative fixity, notwithstanding Aldebaran's appreciable advance in the meantime. A more remote companion, however, discovered by Herschel in 1781, is certainly optical, and has been shown at Lick to be double (*ibid.*, No. 2875). Most likely it forms part of the cluster of the Hyades, upon which Aldebaran is casually projected.

The division of the leading member of the group known as σ Orionis illustrates Struve's remark that multiple stars are intermediate between double stars and clusters. Herschel saw it as doubly triple, one set being much fainter than the other. Each proved, under Struve's and Barlow's scrutiny, quadruple, with two very small stars between; while the chief of the decuple assemblage has been resolved at Lick into an excessively close pair, recalling the case of Sir J. Herschel's quintuple star 45 Leporis, broken up into *nine* components by Burnham in 1874. No relative, and scarcely any absolute motion is perceptible among the constituents of σ Orionis; but one of them, called "ashen" by Struve, "grape-red" by Webb, is perhaps variable in colour.

The "Pointer" next the Pole, α Ursæ Majoris, has so far been seen as double only with the giant telescope of Mount Hamilton. The extreme difficulty of the pair arises from the disparity of light between its members, the eleventh magnitude satellite at 0"·83 being almost swallowed up in the glare of its brilliant primary. This disparity, too, throws some shadow of doubt on the reality of the connection, since the supply of small stars for the occupation of chance positions is of course vastly greater than of large. The similar, but more distant companion of γ Cassiopeiæ (at 2"·18) also recently discovered at Lick, is hence not unlikely to prove merely optical, the Milky Way, in which this pair occurs, being pre-eminently rich in such objects; and the presumption is still smaller that a fourteenth magnitude neighbour of θ Cygni owns a genuine allegiance. But here, as Mr. Burnham points out, the proper motion of the larger star will speedily decide (*Astr. Nach.*, No. 2912.) There can, on the other hand, be no hesitation in admitting that η Ophiuchi, resolved last spring by the same indefatigable observer into two nearly equal components, at 0"·35, constitutes a physical system, and one in which rapid movements may be looked for. The stars evidently travel together, else they should have been, through the effects of a proper motion of one second of arc in ten years, so far apart a little time back that they could not possibly have escaped separate discernment. Their relation to the Milky Way is picturesque, and has been thought to be significant. "Situated at the extreme northern and pointed extremity of a luminous elongated patch of milky light," Mr. Gore remarks, η Ophiuchi "looks as if it were drawing the nebulous matter after it like the tail of a comet" (*Journal Liverpool Astr. Society*, vol. vii. p. 178). But we may safely regard the appearance as illusory.

Some of Mr. Burnham's measures of known doubles also supply results of interest. Thus, the duplex, sea-green companion of γ Andromedæ can now barely be "elongated" with a magnifying power of 2700 on the great refractor. Yet, so lately as 1881, the two stars could be distinguished with eight inches of aperture. The unequal pair, 99 Herculis, discovered by Alvan Clark in 1859, is even more recalcitrant. No amount of optical constraint can now extract from it the slightest indication of duplicity. Since 1878, 85 Pegasi has traversed 213° of its orbit; and Mr. Schaeberle's new elements, embodying the Lick data, give it a period of $22\frac{1}{3}$ years, and oblige us (on the dubious assumption that Brünnow's small parallax can be depended upon) to ascribe a mass to the system eleven times the solar, the components revolving at nearly eighteen times the distance of the earth from the sun. The sun and Jupiter, if of equal *areal* lustre, would present, at half the supposed distance of 85 Pegasi, just its telescopic aspect.

Like 85 Pegasi, δ Equulei is optically triple, while physically double, the companionship of Struve's more distant attendant being in each case temporary and accidental. The bright star of δ Equulei was divided by O. Struve in 1852, and the pair soon proved to be in exceptionally rapid motion. They constitute, in fact, the swiftest binary system yet known. Glasenapp's period, nevertheless, of $11\frac{1}{2}$ years is evidently too short. The Lick measures show the star to be lagging slightly behind its predicted place.

The investigation of stellar orbits has scarcely yet emerged from a tentative stage. Its results are for the most part loose approximations, largely open to future correction. There are very few stars of which the period is known within a few years; there are perhaps two—42 Comæ and ξ Ursæ—of which it is known within a few months. This is due to no lack of skill or diligence in the computers, but solely to the deficiencies, both in quality and quantity, of the materials at their command. Very small errors become enormous when they affect the relative situations of objects divided by a mere *hair-breadth* of sky; and there is no branch of astronomy in which "personality" has played a more conspicuous or a more vexatious part than in double-star measurements. This at least is abolished by photography; which has, however, as yet proved applicable only to a limited class of coupled stars. With the extension of its powers to all, a new era in the knowledge of stellar revolutions may be expected to open.

A. M. CLERKE.

GEOLOGICAL EXCURSION TO THE ACTIVE AND EXTINCT VOLCANOES OF SOUTHERN ITALY.

THE excursion of geologists to the volcanic regions of South Italy came to a very satisfactory conclusion. We have already referred to the first part of the excursion to the Lipari Islands, and the interesting state of activity in which the volcanoes of Vulcano and Stromboli were found to be in. On leaving those islands the party proceeded to examine the Val di Bove, the Cyclopean Islands, the slopes of Etna with its numerous parasitic cones and lava streams, and the central crater itself. The Italian Minister of Public Instruction allowed the party to sleep in the observatory near the mountain summit, and although the weather was rough and misty, about half the party were able to get a good view of the crater, which is now in a solfataric condition. The geologists had also the advantage of becoming acquainted with the mud volcanoes of Paterno. In this part of the excursion the party had the valuble help of Prof. O. Silvestri, to whom Dr. Johnston-Lavis handed over the direction at Etna, although still acting as general director and interpreting Prof. Silvestri's demonstrations. All along the journey the party were *fêted* by the prefect of the province and the mayors of the different communes, and found invaluable hospitality in the splendid villa of the Marquis Favara at Biancavilla. The second fortnight of the excursion was spent at Naples and its vicinity, under the direction of Dr. Johnston-Lavis, aided for the sedimentary rocks by Prof. Bassani of the University of Naples. Although the weather was not so favourable as in Sicily, the delay only amounted to two days. Many thanks are due to the mayor of Naples for his hosp'tality in providing for the party a splendid steam yacht for their visit to Capri and Ischia, so affording very greatly increased facilities for their excursions. The members gave a day to the examination of the reservoirs and other works connected with the new and most perfect and purest town water supply in Europe, as well as the new drainage works and destruction of the old town of Naples. Although the visit to the crater of Vesuvius had to be delayed for upwards of ten days for suitable weather.

the party had the good fortune to see the volcano in great perfection. There existed at the time of the visit four concentric crater rings and two main vents ejecting red-hot lava cakes, which the geologists were able to approach within ten yards, after which they descended some distance on the slopes of the great cone to a small lava stream issuing from its sides, at which various experiments were performed. The director, who has visited the crater over sixty times, remarked that he had never but once seen it to greater perfection.

The numerous volcanoes of the Phlegrean fields were examined, and most of those present expressed their satisfaction at the many important lessons to be learnt from them. At Pompeii the members had the valuable direction of Dr. A. Sambon for the archæological part, whilst Dr. Johnston-Lavis devoted himself only to explaining the phenomena and materials associated with the destruction of the buried cities.

After Naples the party examined on their way northwards the volcano of Roccamonfina, under the direction of Dr. Johnston-Lavis, and Monte Cassino under that of Prof. Bassani of Naples. The Lyceum at Sessa Aurunca was kindly lent by the commune to accommodate the members during their night's stay on their way over the mountain, a sumptuous dinner being provided by the municipality. The carriages the next day were offered by the province of Terra di Lavoro, and after the ascent had been made of the central cone (Mount Santa Croce) a lunch not less sumptuous than the dinner of the preceding evening was given by the town of Roccamonfina.

The next day was devoted to Monte Cassino, its manuscript and art treasures, as well as the Cretaceous limestones constituting the mountain upon which it is built. Prof. Bassani acted as geological director.

At Rome the party examined the concentric craters, parasitic cones, crater lakes, lava streams of the Alban volcano, also the fossiliferous Pliocene beds capped by volcanic deposits close to the Eternal City. The lower Mesozoic limestones, the travertine, the sulphur springs, and all the other points of geological interest of the Campagna Romana were visited.

As directors of the excursions around Rome may be mentioned Profs. Mele, Portis, and Strüver. Signor Zezi (secretary of the Italian Geological Survey), Signors Demarchi, Clerici, Tellini, and Prof. Lanciani kindly undertook the archæological demonstrations which acted as dessert to the rich geological repast.

The official excursions terminated on October 28, with the trip to Tivoli, although a number of geologists remained to visit the sights of Rome. In the evening a dinner was offered to Dr. Johnston-Lavis, Mr. L. Sambon, and the Roman directors. The thanks of the party were offered to the Minister of Public Instruction, Prefects and Mayors, and private individuals, who had done so much to facilitate the progress, through often almost inaccessible districts, for a large party.

Special votes of thanks were proposed to the different Italian geologists who had kindly offered their services in directing the party through their districts, and lastly to Dr. Johnston-Lavis for originating this new departure in scientific excursions, as well as acting not only as director in his own districts, but interpreting and organizing during the whole excursion, and to Mr. L. Sambon for his administrative skill, his attainments in different branches of science, which added so much to the success and comfort of over forty English geologists, not to speak of the numerous Italians who from time to time joined.

REMARKABLE HAILSTONES.

On p. 43 of the present volume of NATURE the following extract is given from a paper by Prof. Houston in the Journal of the Franklin Institute:—"On some of the hailstones, though not on the majority of them, well-marked crystals of clear transparent ice projected from their outer surfaces for distances ranging from $\frac{1}{8}$ to $\frac{1}{4}$ of an inch. These crystals, as well as I could observe from

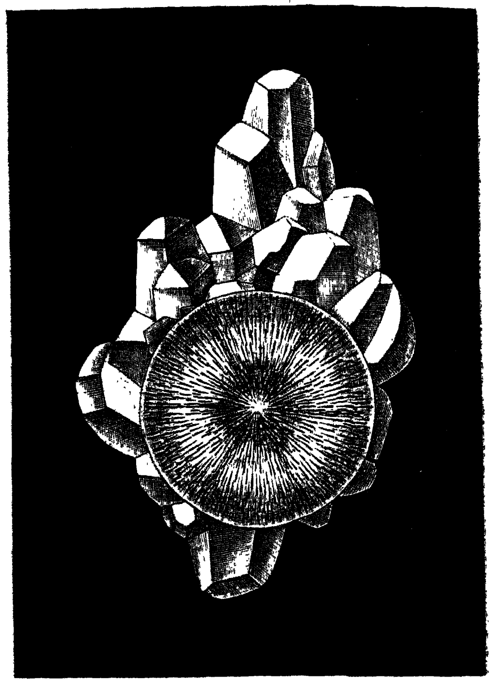


FIG. 1.

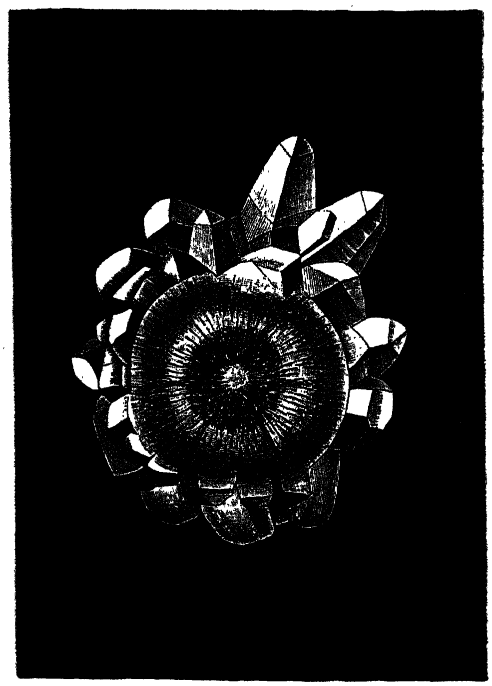


FIG. 2.

the evanescent nature of the material, were hexagonal prisms with clearly cut terminal facets. They resembled the projecting crystals that form so common a lining in

geodic masses, in which they have formed by gradual crystallization from the mother-liquor. They differed, however, of course, in being on the outer surface of the spherules."

It is evident from Prof. Houston's paper that this peculiar form of hail was unknown to him, and, as it must also have been unknown to many who have propounded theories as to the formation of hail which will not account for it, I think that a service may be rendered to meteorology by the reproduction of three of the exquisite lithographs of this form of hail given in Prof. Abich's paper, "Ueber krystallinischen Hagel im Thrialethischen gebirge," published at Tiflis in 1871. The hailstones represented in Figs. 1–3 all fell on June 9 (21), 1867, at Bjeloi Kliutsch, a village about twenty miles south-west of Tiflis,

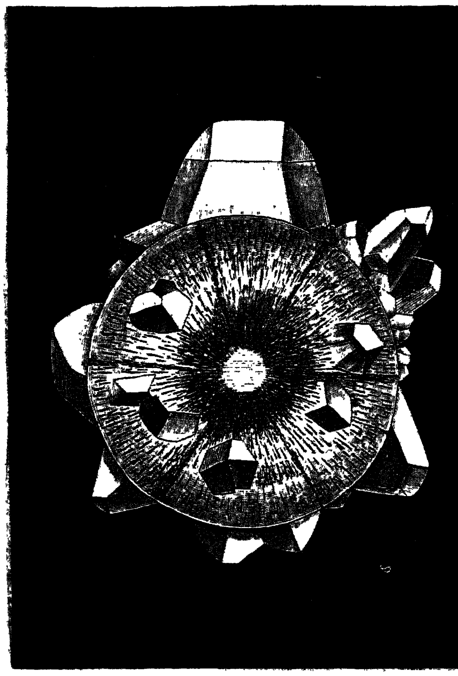


FIG. 3.

and 12,425 feet above sea-level (lat. 41° 33′ N., long. 44° 30′ E.).

Theories of the formation of hail are almost innumerable. I was reading a pamphlet not long since which contained summaries of, I think, twenty-three theories. Some—like Prof. Schwedoff's, that hailstones come from interplanetary space (Brit. Ass. Report, Southampton, 1882, p. 458)—are very droll; but the subject is a very difficult one, and one upon which I do not know of a single good treatise in our language. Possibly, the reproduction of these figures may induce someone to prepare an exhaustive memoir. I could place a large amount of historical and theoretical material at the disposal of any competent person who would undertake the preparation of such a work, it being quite impossible for me to do it myself.

G. J. SYMONS.

NOTES.

AT a largely attended meeting in Edinburgh on Tuesday, Dec. 3, Sir Douglas Maclagan in the chair, it was resolved that Mr. Geo. Reid, R.S.A., should be commissioned to paint a portrait of Prof. P. G. Tait, to be placed permanently in the rooms of the Royal Society of Edinburgh. A committee was appointed to carry out the resolution, including, among others, Mr. John Murray (*Challenger* Expedition), Convener; Mr. Gillies Smith, Hon. Treasurer; Lord President Inglis, Lord Kingsburgh, Lord Maclaren, Sir William Thomson, Sir Arthur Mitchell, Prof. Robertson Smith, Prof. Chiene, Dr. Alexander Buchan, Mr. Robert Cox, and Mr. William Peddie. It was proposed that an etched engraving of the portrait be prepared for distribution among the subscribers, the plate to be destroyed after the required number of copies have been thrown off. It was further resolved that all the Fellows of the Royal Society of Edinburgh, the Professor's old pupils, and others, be afforded an opportunity of taking part in this public recognitition of Prof. Tait's eminent services to science.

ITALY, France, and the United States of America were represented in the elections to foreign membership of the Royal Society on Thursday last. Prof. Stanislao Cannizzaro, of Rome, was elected on the ground of his researches on molecular and atomic weights; Prof. Chauveau, of Paris, for his researches on the mechanism of the circulation, animal heat, nutrition, and the pathology of infectious diseases; and Prof. Rowland, of Baltimore, for his determination in absolute measure of the magnetic susceptibilities of iron, nickel, and cobalt; for his accurate measurements of fundamental physical constants; for the experimental proof of the electro-magnetic effect of electric convection; for the theory and construction of curved diffraction-gratings of very great dispersive power; and for the effectual aid which he has given to the progress of physics in America and other countries.

ADMIRAL MOUCHEZ and MM. Janssen and Perrotin, head astronomers of the Observatories of Paris, Meudon, and Nice, were raised, in November, to the grade of Officer of the Order of the Rose of Brazil, and MM. Frassenet, Paul, and Prosper Henry, admitted to knighthood in the same order. The Paris Correspondent of the *Daily News* says that the diplomas securing to them these distinctions were the last official documents signed by Dom Pedro. He asked his secretary to add a personal compliment to each of the astronomers with whom he was personally acquainted.

SOME time ago we announced that a Physical Society was about to be formed in Liverpool. This has now been done, and we are glad to learn that the new Society begins its career under most favourable conditions. The meeting at which it was constituted was well attended, and displayed much interest in the scheme. Nearly ninety names were at once handed in to the secretary, Mr. T. Tarleton, for membership. Prof. Oliver Lodge, F.R.S., was appointed President. The next meeting will be held in the Physics Theatre, University College, Liverpool, on Monday, the 16th inst., at 8 o'clock, when the President will deliver his inaugural address.

DR. JOHN G. MCKENDRICK, F.R.S., Professor of Physiology in the University of Glasgow, has been elected President of the Philosophical Society of Glasgow.

PROF. LESQUEREUX, the eminent American bryologist and palæontologist, died in his house at Columbus, Ohio, on October 25, at the age of nearly eighty-nine years.

WE regret to learn from a memoir that has been sent to us by Prof. Barboza du Bocage, that Señor José Augusto de Souza died recently at Lisbon, where he was Curator of the Zoological Department in the Museum. He was the author of some useful memoirs on African birds, and is best known for his Catalogue of the *Accipitres*, *Columbæ*, and *Gallinæ* in the Lisbon Museum.

THE fifth of the series of "One Man" Photographic Exhibitions at the Camera Club will be open for private and press

view on Monday, December 16, at 8 p.m., and on and after Tuesday, December 17, it will be open to visitors on presentation of card. The Exhibition will consist of pictures by the late Mr. O. G. Rejlander, and a selection from over 200 of his famous figure and *genre* studies will be shown. The pictures will be on view for about six weeks.

On November 21 the American Philosophical Society, Philadelphia, celebrated the hundredth anniversary of its first occupation of its present hall. The banquet was a great success. The following were the toasts:—"The language of Science and Philosophy is universal, but adopts various dialectic forms to diffuse knowledge," proposed by Prof. John W. Mallet, representative from the Royal Society of London; "Our kindred Societies in every clime," proposed by Prof. Joseph Lovering, President of the American Academy of Arts and Sciences; "All research into the Book of Nature has not discovered an erratum," proposed by Sir Daniel Wilson, President of the University of Toronto; "The successful pursuit of Science expunges error—it never antagonizes truth," proposed by the Hon. Lyon G. Tyler, President of William and Mary College; "Mental Analysis is the efficient solvent of many difficulties in Science and Philosophy," proposed by the Rev. Dr. Charles W. Shields, Princeton College; and "The labours and achievements of great teachers in Science and Philosophy live after them—these are their monuments," proposed by the Right Rev. Dr. John J. Keane, President of the Catholic University of America.

Dr. Pax, of Breslau, has been appointed Curator of the Botanic Garden in Berlin; Mr. D. G. Fairchild, Assistant in the section of Vegetable Pathology in the United States Department of Agriculture; Dr. H. Dingler, Professor of Botany in the Forest Academy of Aschaffenburg; Dr. F. Noll, Professor of Botany in the University of Bonn; and Dr. N. Wille, of Stockholm, Lecturer on Botany at the Royal Agricultural Institution at Aas, near Christiania.

Prof. Bornmüller, Director of the Botanic Garden at Belgrade, has started on a twelve months' botanical tour through Asia Minor. Beginning at Amasia, he will travel through the country between the courses of the Kisil-Irmak and Euphrates, southward to the completely unexplored mountains of Ak-dagh. The *Botanical Gazette* says that this country has only once been explored, thirty-five years ago, by the Russian botanist Wiedemann. According to the same authority, Prof. Bornmüller is a young and very successful explorer, with a great deal of experience, especially from his long journey in 1886, through Dalmatia, Monte Negro, Greece, Turkey, East Bulgaria, and Asia Minor. His original collection will be transferred to Weimar, where it will be carefully gone through by Prof. Hausknecht.

The "mountain laurel," or *Kalmia*, and the Indian corn, are suggested in American papers as national flowers for the United States.

In the December number of the *Kew Bulletin* Mr. Thiselton Dyer explains that for some years, when it has been necessary to find space in the Palm House at Kew for the development of new and interesting species of palms, he has not hesitated to transfer to the Temperate House plants which he thought would probably endure a lower temperature. The experiment has been most successful, many of the plants luxuriating in the change. Anxious to obtain further information as to cool cultivation of tropical and sub-tropical plants, Mr. Dyer lately applied for leave to send Mr. Watson, assistant curator at Kew, to the south of France to report on what he might be able to observe. Permission was given; and Mr. Dyer's statement is followed by a series of valuable and interesting notes in which Mr. Watson sums up the results of his mission. His journey took place in the latter part of October. He had a fortnight at his disposal, and during that time he visited as many gardens as possible between Hyères and Mentone. One of the most interesting of the gardens visited was a branch establishment, at Hyères, of the Société d'Acclimatation, Paris. Here a good deal of experimental gardening is practised, plants of all kinds being planted and tested as to their hardiness, &c. Mr. Watson says that while he was inspecting these gardens the idea was suggested "that a well-managed botanical station, devoted chiefly to experimental testing, proving, and breeding operations amongst plants, would, if established in some such favoured locality as Hyères, be capable of much valuable work."

The following are the lecture arrangements at the Royal Institution, so far as they relate to science, before Easter:—Prof. A. W. Rücker, six Christmas lectures to juveniles on electricity; Prof. G. J. Romanes, ten lectures on the post-Darwinian period; Mr. Frederick Niecks, four lectures on the early developments of the forms of instrumental music (with musical illustrations); Prof. Flower, three lectures on the natural history of the horse and of its extinct and existing allies; the Right Hon. Lord Rayleigh, seven lectures on electricity and magnetism. The Friday evening meetings will begin on January 24, when a discourse will be given by Prof. Dewar on the scientific work of Joule. Succeeding discourses will probably be given by Sir Frederick Abel, Mr. Henry B. Wheatley, Prof. J. A. Fleming, Mr. Shelford Bidwell, Prof. C. Hubert H. Parry, Mr. Francis Gotch, Prof. T. E. Thorpe, Prof. G. F. Fitzgerald, the Right Hon. Lord Rayleigh, and other gentlemen.

Messrs. Macmillan and Co. will shortly publish the first part of Prof. Eimer's work on "Organic Evolution as the Result of the Inheritance of Acquired Characters according to the Laws of Organic Growth," translated by J. T. Cunningham, M.A., F.R.S.E., late Fellow of University College, Oxford.

Messrs. Blackwood and Sons have just published "The Construction of the Wonderful Canon of Logarithms," a translation of "Mirifici Logarithmorum Canonis Constructio," by John Napier, of Merchiston. The work was published in 1619, but is so rare as to be very little known, being only once reprinted in 1620, and never translated. The present translation is by William Rae Macdonald, who also contributes notes and a catalogue of Napier's works.

Slight shocks of earthquake, lasting from five to ten seconds, were felt on Sunday, at Taranto, Foggia, Chieti, Montesaraceno, Agnone, Ancona, and Urbino. At Torremileto, in the province of Foggia, a strong shock is said to have been felt; and a slight shock, followed by a somewhat stronger one, occurred at Naples soon after 6 a.m. On Monday there were seismic disturbances in Dalmatia, Bosnia, and Herzegovina. According to a telegram, through Reuter's Agency, from Vienna, a somewhat severe shock was felt on Monday, at 6.30 a.m., at Knin, Dernis, Sebenico, Trau, Scardona, and Spalato, the direction of the movement being from north-east to south-west. A violent shock, lasting five seconds, occurred at 6.40 at Serajevo, being felt three minutes later at Novi and Krupa also.

At the ordinary meeting of the Council of the Sanitary Assurance Association, on Monday last, arrangements were completed for a series of lectures during January and February 1890, in the theatre of the College of State Medicine, Great Russell Street. The series will include the following:—Mr. H. Rutherfurd, barrister-at-law, on "House Sanitation from a Householder's Point of View," Sir Joseph Fayrer, F.R.S., in the chair; Prof. T. Roger Smith, on "Household Warming and Ventilation," Sir Douglas Galton, F.R.S., in the chair; Mr. Mark H. Judge, on "The Sanitary Registration of Buildings Bill," Lord Henry Bruce, M.P., in the chair. The object of

the Association being to promote good sanitary arrangements in the houses of all classes of the community, both men and women are invited to these lectures. Discussion is invited.

THE "Fauna of British India," of which we noticed the first volume of fishes last week, is making steady progress. Mr. Eugene Oates will produce the first volume of the birds of India during the present month. The work will be principally founded on the great Hume Collection in the British Museum, and the author of the "Hand-book of the Birds of British Burmah," may be trusted to give a thoroughly good account of the birds of India. Side by side with his three volumes on Indian ornithology, Mr. Oates will also publish a new edition of Mr. A. O. Hume's "Nests and Eggs of Indian Birds," which has long been out of print. For this purpose Mr. Hume has intrusted to Mr. Oates the whole of the material collected by him for a second edition, and there is no doubt that the work will be warmly welcomed by naturalists. Portraits of some of the leading men who have contributed to the history of Indian ornithology will be given in this new edition, and will form an interesting feature of the work.

MR. FRANCIS NICHOLSON, a well-known Manchester ornithologist, is about to issue an English translation of Sunderall's "Tentamen," with a memoir and portrait. This work will be welcome at the present time, when increased attention is being paid to the classification of birds.

MR. SEEBOHM will, we understand, propound his system of arrangement of the class Aves in the January number of the *Ibis*, and the memoir will doubtless be a valuable one, as the author is known to have devoted close study to the subject during the past two years.

MR. A. P. GOODWIN, who was with Sir William McGregor on his recent exploration of Mount Owen Stanley, is about to start on a lecturing tour in America. He was successful in taking several interesting photographs of the country visited by the Expedition, and he paid especial attention to the habits of the Birds of Paradise and the Bower-birds. He has some remarkable sketches of the playing-grounds of some of the latter, notably of *Amblyornis subalaris*, of Sharpe, which rivals in decorative faculty the Gardener Bower-bird (*Amblyornis inornata*) of North-Western New Guinea.

PROF. GIARD has recently discovered a micro-organism which possesses the power of conferring luminosity or phosphorescence upon different crustaceans. This microbe was found in the tissues of *Talitrus*, and is easily cultivated in appropriate media. It soon kills *Talitrus*.

M. LOUBAT, member of the New York Historical Society, has presented the French Academy of Inscriptions with a sum producing 1000 francs per annum; his intention being that a prize of 3000 francs shall be offered every three years for the best printed work concerning the history, geography, archæology, ethnography, linguistics, and numismatics of North America. The first prize will be granted in 1892, and the Academy has decided that the works submitted for consideration shall not relate to matters referring to an earlier date than 1776. The competition will be open to the author of any work on the subject published after July 1, 1889, in any of the following languages: Latin, French, English, Spanish, and Italian. Two copies must be sent to the Secretary of the French Institute before December 31, 1891.

IN the Pacific Coast region there are now four flourishing colonies of introduced pheasants. Dr. C. Hart Meriam, who refers to the subject in his last Report to the American Agricultural Department, says that the most northerly of these colonies is at the south end of Vancouver Island, near Victoria; the second in Protection Island, in Puget Sound; the third at the junction of the Willamette River with the Columbia; and the fourth in the middle portion of the Willamette Valley. The two latter colonies are now separated by so narrow a strip of territory that they will doubtless become united during the next few years. All the pheasants of the three colonies last mentioned appear to have been imported from China by Judge O. N. Denny.

THE American Agricultural Department has been making careful inquiry as to the food of crows; and the result, as set forth in a Report by Mr. Walter B. Barrows, is likely to surprise those who have always contended that these birds do very much more good than harm. It is not disputed that they destroy injurious insects, that they are enemies of mice and other rodents, and that they are occasionally valuable as scavengers; but these services are slight in comparison with the mischief for which they are responsible. The injury done by them to Indian corn, wheat, rye, oats, and other cereals is enormous. According to one observer, the crow eats corn "from ten minutes after planting until the blades are three inches high;" and more than a score of other observers testify that he not only pulls up the young plants, but digs up the newly sown seed. His depredations extend to potatoes, sweet potatoes, beans, pea-nuts, cherries, strawberries, raspberries, and blackberries; and he widely distributes certain poisonous plants, the seeds of which are improved rather than impaired by passage through his digestive organs. As if all this were not enough, it is shown that the crow eats beneficial insects, and that he makes himself a most formidable nuisance by destroying the eggs and young both of domesticated fowls and wild birds.

TWO new seismoscopes, made by Brassart Brothers, of Rome, and adopted at the Italian meteorological stations, are described in the *Rivista Scientifico-Industriale* of October 15. They are of a very simple nature, the one consisting merely of an iron rod, about 5 inches long, leaning slightly against an adjustable screw support near its middle, and with its lower pointed end in a cup. When a shock or tremor occurs, the rod falls away from its support and is caught by a fixed metallic ring making electric contact and ringing a bell. In the other instrument, the ring is connected with a hinged lever arrangement, which stops the mechanism of a timepiece, showing when the shock occurred.

THE National Association for the Promotion of Technical and Secondary Education has issued an excellent Report on the existing facilities for technical and scientific instruction in England and Wales. As Mr. Acland and Mr. Llewellyn Smith explain in a prefatory note, the Report is not intended so much for experts as for those who wish to obtain, without consulting many Blue-books and other official documents, some trustworthy information as to what is being done. The facts have been arranged with the utmost care, and the work ought to be of considerable service in helping to show "what are the gaps in our educational system that must be filled, and how great is the need for the re-organization and improvement of existing agencies."

THE Annual Report of the Manchester Literary and Philosophical Society, published in vol. ii., 4th series, of the Proceedings, shows a marked improvement in the financial condition of the Society, the membership being only one less than at the corresponding period last year. The volume contains many papers and abstracts of papers of varying interest. There is a long paper on "*Hymenoptera Orientalis*" by Mr. Cameron, giving descriptions of the various species, their habits and localities, and references to the literature of the subject. Dr. A. Hodgkinson communicates an interesting paper on the "Physical Cause of Colour in Natural and Artificial Bodies," recording experiments which tend to show whether the colour is produced by a

structure of thin plates, or one of fine lines. There are two papers on leaves from the cutting of the Ship Canal, one giving a general description, and the other, by Dr. Schunck, F.R.S. showing that the green colouring-matter, which has proved to be so permanent, is due to a modified form of chlorophyll; spectroscopic examination of the colouring-matter showed it to be identical with that produced by the action of dilute hydrochloric acid on ordinary chlorophyll.

THE Middlesex Natural History and Science Society has issued a volume containing its Transactions during the session 1888-89. The volume opens with an interesting Presidential address by Prof. Flower, on the Natural History Museum, Cromwell Road, and some recent additions thereto. Mr. E. M. Nelson has an illustrated paper on diatom structure; and Mr. J. A. Brown contributes a paper, also illustrated, on working sites and inhabited land surfaces of the Palæolithic period in the Thames Valley.

THE fourth volume of "Blackie's Modern Cyclopædia" has been issued. It begins with the word "fire" and ends with "Ilorin." The work, as we have said on former occasions, is admirably edited by Dr. C. Annandale. The articles are necessarily brief; but, so far as we have been able to test them, they are clear and accurate. There is no falling off in the present volume.

MESSRS. WARD, LOCK, AND CO., have added to their "Minerva Library of Famous Books" a reprint of Dr. A. R. Wallace's fascinating "Narrative of Travels on the Amazon and Rio Negro." A biographical sketch of the author is contributed by Mr. G. T. Bettany, the editor of the series; and the volume includes a portrait of Dr. Wallace, a map, and full-page illustrations.

HAZELL'S Annual for 1890—the fifth issue—has been published. It is edited by Mr. E. D. Price. An immense quantity of information, alphabetically arranged, has been packed into this useful volume. Many articles which the editor describes as "new and important" have been inserted in the present issue.

A SCIENCE CLUB has been formed among the students of the University of St. Andrews for the purpose of developing the interest already taken in scientific pursuits. Prof. W. C. McIntosh, F.R.S., has been elected Hon. President for the session 1889-90.

ANOTHER important paper by M. Henri Moissan upon the perfected mode of preparation and upon the density of fluorine, is contributed to the current number of the *Comptes rendus*. Since the appearance of his paper of two years ago, M. Moissan has employed an electrolysis apparatus of much larger size, and has added to it an accessory apparatus by means of which the gas may be obtained quite free from vapour of hydrofluoric acid, which, as described in NATURE last week, is the cause of the destructive action upon platinum. The platinum U-tube of the new apparatus has a capacity of 160 c.c., and contains during the electrolysis 100 c.c. of hydrofluoric acid. The exit tube at the positive side, from which the fluorine is liberated, is continued into a small platinum spiral condenser immersed in a bath of methyl chloride at − 50° C., where all but the last trace of hydrofluoric acid is retained. From this the gas is led through two platinum tubes filled with fragments of sodium fluoride, a salt which combines with hydrofluoric acid with great energy, forming hydrogen sodium fluoride. By these means the fluorine is obtained perfectly pure, and is quite invisible in dry air, no trace of fuming being apparent, as is the case before purification. In order to determine the density of the gas, a couple of ingeniously constructed platinum flasks have been employed. Each of these flasks is closed by a cylindrical stopper also of platinum; to the side of the neck a side tube is attached on a level with the centre of the stopper. Through the stopper an aperture is bored in such a manner that, when the stopper is rotated into a certain position, connection is established between the interior of the flask and the side tube. A vertical tube also passes through the stopper and penetrates to near the bottom of the flask; this tube is also closed at its upper end by means of a platinum stopper. The stoppers are finely polished and adjusted with great care. Each flask weighs about 70 grams and has a capacity of about 100 c.c. In the density determinations the two flasks were counterpoised on the two pans of the balance. One of them was then filled with pure dry nitrogen gas, which was subsequently displaced by the pure fluorine, the electrolysis apparatus being connected with the upper end of the vertical tube of the density flask by means of flexible platinum tubing. The fluorine was allowed to pass through the apparatus for five minutes after cold silicon was readily ignited by the gas issuing from the side exit tube. The stopper of the flask was then rotated through half a revolution, so as to completely shut off the exit tube, and the stopper of the vertical tube replaced. The flask was again weighed against the other flask containing air, and the difference of weight noted. The amount of residual nitrogen was estimated by opening the stopper of the vertical tube under water, when the fluorine instantly decomposed an equivalent of water, liberating oxygen and forming hydrofluoric acid. The mixture of oxygen and the residual nitrogen was then collected, and the oxygen absorbed by pyrogallic acid and potash. Three determinations yielded, for the density of fluorine compared with that of hydrogen, 18·27, 18·26, and 18·33. These values appear to indicate that the number 19, usually taken as representing the atomic weight of fluorine, is slightly too high, and this view is confirmed by the low numbers obtained in former determinations of the density of phosphorus trifluoride.

THE additions to the Zoological Society's Gardens during the past week include a Malayan Bear (*Ursus malayanus* ♀) from Malacca, a Gold Pheasant (*Thaumalea picta* ♀) from China, presented by Captain Bason; a Common Squirrel (*Sciurus vulgaris*), British, presented by Mr. W. Aubrey Chandler; a Mexican Deer (*Cariacus mexicanus* ♂) from Peru, a Grey-breasted Parrakeet (*Bolborhynchus monachus*) from Monte Video, deposited; an American Bison (*Bison americanus* ♂ born in the Gardens.

OUR ASTRONOMICAL COLUMN.

OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at Greenwich at 10 p.m., December 12 = 3h. 27m. 9s.

Name.	Mag	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G. C. 768	—	—	3 39 29	+23 14
(2) D.M. + 71° 201 ...	7	Reddish-yellow.	3 18 56	+71 29
(3) ε Eridani	3	Yellow.	3 27 5	− 9 51
(4) ζ Eridani	4	White.	3 10 5	− 9 14
(5) 27a Schj.	7	Red-yellow.	3 32 21	+62 18
(6) R Lacertæ	Var.	Orange.	22 38 23	+41 48

Remarks.

(1) The General Catalogue description of this nebula is as follows:—!!! Bright; very large, irregular figure. According to Tempel, this is a variable nebula, and its spectrum, which has not yet been recorded, will therefore have a special interest. Continued observations may, very probably, give a clue to the origin of the variability.

(2) Duner classes this with stars of Group II., but states that the spectrum is only feebly developed. Further observations are necessary before it can be placed in position on the "tem-

perature curve." As I have previously pointed out, the "feebly developed" stars of the group are probably either early or late species, as the bands would be weak in either case. If it be an early star, the bands in the blue will be most strongly developed; while, if it be a late star of the group, the bands in the red will be strongest. In the latter case, lines would probably also be seen.

(3) Konkoly classes this with stars of the solar type. As in former stars of this class which have appeared in these columns, observations are required to decide whether the star belongs to Group III. or to Group V. (For criteria, see p. 20.)

(4) This is a star of Group IV., of which observations of the relative intensities of the hydrogen and metallic lines are required, so that the star may be arranged in a line of temperature with others.

(5) This is a star of Group VI., which Dunér describes as having a spectrum consisting of three zones, band 2 being probably also present. Particular attention should be given to the intensity of the band 6 as compared with the others. Other subsidiary bands should also be looked for, as they are seen in several stars of lower magnitude, and it is important that we should know whether their presence is dependent solely upon the brightness of the star, or really indicates a difference in the condition of the star itself. (For notation of bands, see p. 112.)

(6) The maximum of this variable will occur on December 27. The period is 315 days, and the magnitude varies from $< 13 \cdot 5$ at minimum to $8 \cdot 6$ at maximum. The spectrum has not yet been recorded.

Note.—Some of the comets of which ephemerides have recently appeared in NATURE may possibly be bright enough for spectroscopic examination. It is not likely that, at their present perihelion distances, their temperatures will be very high, so suggestions for comparison spectra may be confined to those suitable for low-temperature comets. The probable sequence of spectra as a comet leaves aphelion is as follows:—(1) The spectrum of a planetary nebula, as in the comets of 1866-67, observed by Dr. Huggins. This consists of a single line in the position of the chief nebula line near λ 500. (2) The low-temperature spectrum of carbon, consisting chiefly of three flutings near λ 483, 519, and 561. (3) The high-temperature spectrum of carbon, consisting mainly of flutings near λ 564, 517, and a group of five flutings extending from 468 to 474. The most convenient comparison to begin with will be the flame of a spirit-lamp, which will give the hot carbon spectrum. If this does not show coincidences with the cometary bands, a comparison with the bright fluting in the spectrum of burning magnesium should be made. This will determine the presence or absence of the chief nebula line. If neither shows coincidences, the positions of the bands relatively to the hot carbon flutings may roughly indicate the presence or absence of cool carbon. As the two less refrangible flutings of cool carbon fall very near to two of hot carbon, the best criterion for cool carbon is the fluting at λ 483, which is about one-third of the distance from the fluting commencing at 474 towards that commencing near 517. Any variation of the form of the least refrangible cometary band from the corresponding carbon fluting should be noted, as this varies with the temperature (see Roy. Soc. Proc., vol. xlv. p. 168).

A. FOWLER.

PHOTOMETRIC INTENSITY OF CORONAL LIGHT.—The observations made by Prof. Thorpe during the solar eclipse of 1886 (Phil. Trans., vol. clxxx., p. 363, 1889) show that the diminution in intensity of coronal light at different distances from the sun's limb does not vary according to the law of inverse squares. The following measurements make this apparent:—

Distance in Solar Semi-diameters.	Photometric Intensity. Observed.	Photometric Intensity. Law of Inverse Squares.
1·6	0·066	0·066
2·0	0·053	0·042
2·4	0·043	0·029
2·8	0·034	0·022
3·2	0·026	0·016
3·6	0·021	0·013

The brightness of the brightest measured part of the corona (1·55 solar semi-diameters) was 200 times less bright than that of the surface of the moon, or about 0·06 candle, whilst the furthest spot at 3·66 solar semi-diameters was only 1/800 of the brightness, or 0·015 candle. The results obtained will be useful in comparing the brightness of the corona on this occasion with that of other eclipses, and determining what connection the sun-spot periods have with the coronal phenomena.

CORONA OF JANUARY 1, 1889.—Prof. Tacchini, in the *Atti della R. Accademia dei Lincei* (p. 472), gives a note on the corona as shown in a positive copy, on glass, of one of Mr. Barnard's negatives taken during this eclipse. The corona extends, according to Prof. Tacchini, from $+64°$ to $-68°$ on the west limb of the sun, and from $+53°$ to $-68°$ on the east limb, these being about the limits of the zone of the maximum frequency of protuberances derived from his own observations. Two of the protuberances on the photograph were observed at Rome and at Palermo.

MINOR PLANET (12), VICTORIA.—Dr. Gill has issued the ephemeris of this planet for the opposition of 1889, computed from elements which have been corrected from the observations of 1888.

Observatories co-operating in the meridian observations of Victoria should compare their results with this ephemeris, employing $8'' \cdot 80$ for the solar parallax.

Dr. Auwers has undertaken the discussion of the meridian observations, so the detailed results should be forwarded to him as soon as possible.

COMET SWIFT (*f* 1889, NOVEMBER 17).—The following ephemeris is given by Dr. R. Schorr (*Astr. Nachr.*, No. 2937):—

1889.	R.A. h. m. s.	Decl. ° ′	1889.	R.A. h. m. s.	Decl. ° ′
Dec. 12...	23 47 28 ...	+19 6·7	Dec. 22...	0 19 7 ...	+21 49·4
13...	50 31 ...	19 23·6	23...	22 24 ...	22 4·8
14...	53 36 ...	19 40·4	24...	25 43 ...	22 20·1
15...	56 42 ...	19 57·1	25...	29 2 ...	22 35·2
16...	59 50 ...	20 13·6	26...	32 23 ...	22 50·1
17...	0 2 59 ...	20 29·9	27...	35 44 ...	23 4·8
18...	6 10 ...	20 46·1	28...	39 6 ...	23 19·3
19...	9 22 ...	21 2·2	29...	42 30 ...	23 33·6
20...	12 35 ...	21 18·1	30...	45 54 ...	23 47·7
21...	15 50 ...	21 33·8	31...	49 18 ...	24 1·5

The brightness of the comet $= 0 \cdot 81$ (December 12) and $0 \cdot 57$ (December 31), that at discovery being taken as unity.

Comptes rendus, No. 23 (December 2, 1889), contains observations of this comet extending from November 20 to November 27. It is noted that the comet is very feeble and diffuse.

PERIODIC COMETS.—Several short-period comets return to the sun in 1890, and their ephemerides will be furnished as soon as issued. The perihelion passage of Brorsen's comet will occur about February 25, Denning's comet may be expected to return to perihelion in May, and D'Arrest's comet about the third week in September. The orbit of Barnard's comet has not yet been sufficiently defined to enable the date of perihelion passage to be stated.

THE ECLIPSE PARTIES.—The following telegram relating to the eclipse parties has been received:—"*Loanda, December 7.*—The United States corvette *Pensacola*, Captain Arthur R. Yates, with the Solar Eclipse Expedition on board, arrived at St. Paul de Loanda to-day. The voyage down was very smooth, with delightful sailing. The astronomers were at work on the instruments all the way, and are all ready for the eclipse. The time is now so short that it is inadvisable to attempt to take the party and all their instruments inland, so the Expedition will locate at Cape Ledo immediately, and send one or two branch parties inland, with such instruments as are not bulky or heavy, and can quickly be set up and adjusted. The European eclipse observers are beginning to arrive here. Mr. Taylor, of the Royal Astronomical Society, London, has already arrived with a small outfit of apparatus. None of the French or German astronomers are yet here. Cape Ledo turns out to be in every way the most favourable point for locating the American Expedition. Not only are the meteorological conditions likely to be better, but the party can live for the most part on the *Pensacola*, as she will lie at a safe anchorage near the shore. The health of the members of the party is thus insured. The eclipse is several seconds longer there than at Muxima, and chances for clear afternoon skies appear to be rather better. If nothing is heard from the Expedition for the next few days, it may either be taken that the Eclipse Station is finally located at Cape Ledo, or that the semi-cannibal Quissamas have cleared out the whole Expedition."

RECENT INDIAN SURVEYS.

THE "Statement exhibiting the Moral and Material Progress and Condition of India," recently issued, devotes, as usual, a section to the survey work of the past year, of

which the following is a summary. The work of the Survey of India is divided under five heads, namely:—(1) Trigonometrical Survey, (2) Topographical Survey, (3) Cadastral Survey, (4) Special Surveys and Explorations, (5) Map Production.

Trigonometrical.—Out of twenty-six survey parties employed during the year, only one was engaged on trigonometrical work. It carried secondary triangulation for 370 miles along the Coromandel coast as far as the Tanjore District; the work is intended as a basis for marine survey operations. Some triangulation in extension of the great Indian triangles had to be undertaken in Baluchistan as a basis for topographical maps there.

Topographical.—The number of parties engaged in this work was reduced from eight to six, and 15,673 square miles of topographical survey were accomplished, which included 934 square miles of survey in the Southern Mahratta country, the same party doing a quantity of detached forest survey in the valuable teak forests of Kanara; 1085 square miles of topographical work in Guzerat, besides 285 square miles of detailed forest survey in the jungles of Thana and Nasik. Parties 15 and 16 continued the Baluchistan survey, accomplishing in all 11,977 square miles. The cold and snow in winter, as well as the difficulty in getting supplies, were extremely trying to the parties. 977 square miles were surveyed in the Himalayan districts of Kangra, Simla, and the native States pertaining to those districts; 4535 square miles of triangulation and 1284 square miles of topographical survey in the Madura district and the States of Travancore and Cochin of South India. The cost of the Himalayan work and of the Baluchistan surveys was considerably cheaper per square mile than in the previous year.

Forest Surveys.—Two half-parties of the Topographical Survey did fresh work, as above stated, in Bombay. Ground was broken in the forests near Hoskungabad of the Central Provinces; but in the first year, on account of climatic difficulties and the ruggedness of the country, the out-turn of work was small. 343 square miles of forest survey were effected in the forests of the Prome and Thayetmyo districts of Lower Burmah. In Gorakpur of the North-West Provinces, and in Orissa, surveys of certain forest reserves were made by cadastral parties working in the neighbourhood. The whole area of forest surveys accomplished by all these parties during the year was 893 square miles.

Geodetic.—Telegraphic longitude operations were resumed, and seven arcs of longitude were measured between trigonometrical stations in Southern India. The season's observations tend strongly to confirm previous evidence that on the coast of India there is a perceptible deviation of the plum-line towards the ocean.

Tidal and Levelling Operations.—The recording of tidal curves by self-registering tide-gauges, their reduction, and the publication of tide-tables, were continued at eighteen stations, of which seven are permanent, and eleven are temporary for five years. The registrations of tides were satisfactory, and there were few failures. So far as predictions of high water were concerned, 98 per cent. of the entries in the tables were correct within 8 inches of actual heights at open coast stations, and 69 per cent. at riverain stations, while as to time of high water, 56 and 71 per cent. respectively of the entries were correct within fifteen minutes. Levelling operations were prosecuted from Madras to Vizagapatam, at False Point, to connect the Marine Survey beach marks with the main line of level, and from Chinsurah to Nuddea, along the right bank of the Hooghly. There were 597 miles of double levelling accomplished. In Upper Burmah, survey parties or surveyors accompanied the columns which marched through the northern Shan States, the southern Shan States, and the columns that operated in the Yaw country, the Chindwan Valley, and the Mogoung district. Triangulation was carried over 23,274 square miles, and 20,780 square miles of hitherto unknown country were mapped on a scale of four miles to the inch, of which 7605 belonged to the Shan States. North-east from Mandalay, the survey was carried as far as the Kanlow ferry, on the Salween River, a place on the old caravan road between Burmah and China. A large scale map was made of the Ruby Mines tract, showing the sites of all ruby workings. Surveyors accompanied an exploring expedition from the Assam Valley, across the Patkoi ranges, into the Hukong Valley of Upper Burmah, and surveyed two practical passes through the Patkoi hills. A good map of the Black Mountain country was prepared on observations and surveys taken by officers deputed with the Hazara field force. The hill country of Western Nepal has been observed and mapped, and a compilation of recent observations by explorers in Tibet and Bhutan will shortly be published.

Marine Survey.—The survey-vessel *Investigator* and two boat parties were employed on marine surveys throughout the open season, the staff being employed in the chart office during the monsoon months. The *Investigator* accomplished 4630 miles, and the boat parties 1542 miles of soundings. Among the results of the year's work were soundings round the approaches to Madras, whereby it was shown that there were 1700 fathoms of water on a spot hitherto marked on the charts as "5 fathoms doubtful." Surveys were made round the Laccadive and the Andaman Islands, at the Palk Straits, the Western Coral Banks, on the Malabar coast near Cannanore and Tellicherry, and off Parbandar. Interesting marine organisms, some of them quite new, were brought up by the trawler, especially from a depth of 250 fathoms off the Andamans. The observations for temperature have enabled the survey to construct a temperature curve which is fairly constant for all parts of those seas.

Geological Survey.—Among the investigations by the Geological Survey during the year 1888 may be mentioned the examination of the auriferous rocks known as the Dharwar rocks, bands of which occur in the gneiss mountains, from the edge of the Deccan trap in the meridian of Kaladgi, across the upper basins of the Kistna, Tangabhadra, Penner, and Cauvery Rivers. At many places in these bands of Dharwar rock, the geological officers discovered traces of extensive gold workings, the existence of which was hardly known to the present inhabitants. The investigators consider that in many places, especially in the Kolar and Maski bands, gold will be found in quantities that will repay working. The workers of past centuries used to crush the ore in saucer-like hollows in the solid, tough, trappoid rocks, with rounded granite crushers, weighing about a ton each. The supposed diamond sources in the Anantapur district of Madras were examined, but with only negative results. The coal-field of Singareni, in the Nizam's dominions, was examined; it was estimated that 17,000,000 tons of coal were available in the field. The geologists reported that the cost of raising coal into waggons at the pit's mouth ought not eventually to exceed 2 rupees a ton. Further examinations were made of the coal-bearing rocks of Western Chota Nagpore and of Rajmehal; the latter coal source cannot be thoroughly tested until bore holes are put down. The seams of coal at Kohst, in Baluchistan, were found to contain $1\frac{1}{2}$ to 2 feet of good coal at times; coal from surface workings is now chiefly used in locomotives; but the best plan for permanent workings has not yet been settled. The petroleum sources at Khatun, in Baluchistan, and in the Rawal Pindi district of the Punjab, were visited by officers of the Survey; the Khatun oil is too thick to flow down a pipe for forty miles to the railway, where it has made excellent fuel. The Cashmere coal-field, in the upper valley of the Chenab, was also examined.

The report of the Cadastral Surveys and Settlements is devoid of scientific interest.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—In the course of the term which has just come to an end, Mr. J. B. Farmer, B.A., has been elected to a Fellowship at Magdalen, after an examination in botany—a subject to which no Fellowship has been allotted for many years; and the Burdett-Coutts Scholarship in Geology has been awarded to Mr. F. Pullinger, Corpus.

Mr. Hatchett Jackson will continue to act as Deputy Professor of Comparative Anatomy for the next two terms at least.

The recently founded Readership in Geography seems to have proved a success this term, as Mr. Mackinder had a class of fifty in regular attendance.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, November 21.—"On the Tubercles on the Roots of Leguminous Plants, with special reference to the Pea and the Bean." By H. Marshall Ward, M.A., F.R.S., F.L.S., late Fellow of Christ's College, Cambridge, Professor of Botany

in the Forestry School, Royal Indian Engineering College, Cooper's Hill.

In the Philosophical Transactions for 1887 (vol. clxxviii. B, pp. 539–562, Pls. 32 and 33) the author published the results of some investigations into the structure and nature of the tubercular swellings on the roots of *Vicia Faba* and other Leguminous plants.

The chief facts established in that paper were as follows:—That the tubercles occur in all places and at all times on the roots of Papilionaceous plants growing in the open land, but that in sterilized media and in properly conducted water-cultures they are not developed, unless the root is previously infected by contact with the contents of other tubercles. In other words, the tubercles can be produced at will by artificial infection. The author also showed that the act of infection is a perfectly definite one, and is due to the entrance into the root-hair of a hypha-like infecting tube or filament, which starts from a mere brilliant dot at the side or apex of the root-hair, passes down the cavity of the latter, traverses the cortex of the root from cell to cell, until its tip reaches the innermost cells of the cortex, where it branches and stimulates these cells to divide and form the young tubercle.

These facts of the infection were entirely new, as were the methods, and the author showed actual preparations of the infecting filaments passing down the root-hairs, at the time (June 1887).

In this paper the author described and explained the trumpet-shaped enlargements of the filaments, and the bacterium-like contents of the cells (bacteroids—gemmules), and showed that the latter arise from the former. He also pointed out that the root-hairs are distorted at the point of infection, and that the infecting filament originates there from a brilliant granule, presumably one of the bacteroids. Another important observation was that the protoplasm of the cells of the tubercle is stimulated by the activity of the bacteroids in it, and behaves like a plasmodium.

The author now draws attention to some results of his further researches into this confessedly difficult subject.

After numerous culture experiments and observations made last year (1888), it was decided to abandon the broad-bean as the subject for histological analysis, chiefly because it takes so long to exhaust its stores of reserve materials; it was better for the cultures to be made with the pea, the cotyledons of which are so much smaller, and the plant of which is more easily managed in every way in water and pot cultures, while the tubercles and their contents present no essential features of difference.

But more conclusive evidence than the above is offered for the identity of the bacteroids in the two cases. In some of the cultures made in the summer of 1888 the roots of the pea were successfully infected with bacteroids taken from the tubercles of the bean, and this is a point of importance, in view of the belief that each species of Leguminosæ may have its own species of bacteroid.

It is especially the very young root-hairs, with extremely delicate cell-walls, that are infected, and the first sign is the appearance of a very brilliant colourless spot *in* the substance of the cell-wall: sometimes it is common to two cell-walls of root-hairs in contact, and not unfrequently one finds several root-hairs all fastened together at the common point of infection. This highly refringent spot is obviously the "bright spot" referred to in the author's previous paper as the point of infection from which the infecting filament takes origin It soon grows larger, and develops a long tubular process, which grows down inside the root-hair, and invades the cortex, passing across from cell to cell, as described in 1887.

As a matter of fact, then, the "bright spot" is the point of origin of the infecting filament; and, as a matter of inference from the experiments, it cannot but be developed from one of the "bacteroids" or "gemmules" of the tubercles. This attaches itself to the root-hair, fuses with and pierces the delicate cellulose wall, and grows out into a hypha-like filament at the expense of the cell contents. The further progress of this filament has already been described in the author's memoir in the Philosophical Transactions for 1887.

Researches were made during 1888 and 1889 with the object of learning more about the conditions which rule the development of the tubercles, and the relations of the organism to them. The experiments seem to prove conclusively that the well-being of the organism of the tubercle and that of the pea or bean go hand in hand. This of course is only so much evidence in favour of the view that we have here a case of symbiosis of the closest kind, as expressed in the previous memoir.

During the spring and summer of 1888 numerous experiments were made with water-cultures with beans, allowed to germinate in soil so as to be infected by the "germs" therein, as demonstrated previously. Several dozens of such cultures were made, and some of them placed in the dark, others in the ordinary light of the laboratory, and some in a well-lighted greenhouse. Tables were prepared showing the number of leaves, living and dead, the condition of the roots, the height of the stem, and so forth, as recorded every week or so (or at shorter intervals) when the plants were examined. It resulted that, when the beans are in any way so interfered with that they do not assimilate more material than is necessary for the growth and immediate requirements of the plant, the infecting organism either gains no hold at all on the roots, or it forms only small tubercles which are found to be very poor in "bacteroids": in some cases the starving plants began to develop tubercles, which never became larger, and in which the infecting organism seemed to be in abeyance. Whether this is due to the bacteroids being developed in small quantities, or to their absorption into the plant, is still a question.

In these tubercles the chief difference was the paucity in bacteroids, and the prominence of the branched filaments in the cells.

In the spring of this year (1889) the author started a series of water-cultures of beans, infected artificially by placing the contents of tubercles on their root-hairs, and kept the roots oxygenated by passing a stream of air through the culture liquid for twenty-four hours at intervals of a few days: here again the increased growth of the plants—not compensated by increased assimilation—seemed to cause the suppression of the tubercles, or the formation of very poor ones only. These and similar experiments lead to the conclusion that the organism which induces the development of the tubercles is so closely adapted to its conditions that comparatively slight disturbances of the conditions of symbiosis affect its well-being: it is so dependent on the roots of the Leguminosæ, that anything which affects their well-being affects it also.

Some experiments with peas, which are now being tabulated, may throw some light on the wider question which has been raised of late, as to the alleged connection between the development of these tubercles and the increase of nitrogen in Leguminous plants. Thirty-two peas were sown in separate pots of silver-sand, or soil, in five batches of six each, and one of two, and treated in various ways.

The tubercles were developed on all but one of the plants, except those in the completely sterilized media. The evidence at present goes to show that *the Leguminous plant gains nitrogen by absorbing the nitrogenous substance of the bacteroids from the tubercles*; that nitrogenous substances are thus brought by the "bacteroids" ("gemmules") of the infecting organism of the plant; and that, finally, no satisfactory explanation seems forthcoming as to how the organism obtains this nitrogen in certain cases where no compounds of nitrogen have been added. At any rate, if we regard the pot of sand and its pea as one system, there is in some cases *a distinct gain of nitrogen in the crop, and in the sand at its roots.*

The author then refers to the literature since 1887, and reviews two papers by Prażmowski which bear directly on these researches.

"To sum up, Prażmowski's account of the whole matter confirms that given to the Royal Society by the author in 1887, excepting that he interprets the origin and nature of the bacteroids differently; he regards them as produced from the *contents* of the filaments—as germ-like bodies developed in the interior of the filaments, and not budded off from them. This is hypothesis only, however, for the author expressly states (p. 253), 'Direct habe ich ihre Theilungen nicht gesehen, obgleich ich mir die Mühe gab, sie in den verschiedensten Nährmedien und unter den verschiedensten äusseren Bedingungen zu züchten.' He concludes they can only multiply in the still *living* protoplasm.

"As to the shapes of the bacteroids and tubercles, Prażmowski's statements agree with those of previous observers, and he also remarks the plasmodium-like appearance of the cell protoplasm at certain stages, as noticed by myself. Some observations on a possible spore-formation need not be dwelt upon, as he recognized his mistake in a subsequent paper in 1889.

"He leaves the question as to the origin of the bacteroids by budding or otherwise quite undecided, having failed to satisfy himself whether my suggestion is right or not; at the same time, he fully agrees with me and others in believing that these tiny bodies must be the infecting agents, easily and abundantly distributed as they are in the soil, water, &c."

The author concludes by saying:—

"I think it will be admitted by all who study the literature of this subject, that the only real point at issue between Prażmowski and myself is the nature of the bacteroids and their origin from the filaments. I interpreted them as extremely minute budding 'gemmules,' and not bacteria; Prażmowski, with Beyerinck, regards them as true Schizomycetes. We have all alike failed to actually see the process of budding or fission, a fact which will surprise no one who has examined these extremely minute bodies, which are, as Beyerinck rightly puts it, among the smallest of living beings.

"The fact of infection, and the mode of infection, by means of a hypha-like filament passing down the root-hair were definitely established by myself in 1887, and it is satisfactory to find it confirmed in every essential detail by Prazmowski. Our views as to the symbiosis, the struggle between the protoplasm and the 'gemmules' (or 'bacteroids') are the same; though Prażmowski and Beyerinck carry the matter a step further in definitely inferring the absorption of the conquered bodies of the latter, a point in part supported by some of my experiments.

"As to the occurence, origin, and structure of the tubercles, Prażmowski's account is simply in accordance with my own; and it is interesting to note how many points of detail—the distortions of the root-hairs, the relations of the branching filaments to the nuclei and cell-contents, and those of the incipient tubercle to the end of the filament, for example—are confirmed by him."

Chemical Society, November 7.—Dr. W. J. Russell, F.R.S., President, in the chair.—The following papers were read:—Isolation of a tetrahydrate of sulphuric acid existing in solution, by Mr. S. U. Pickering. The freezing-points of mixtures of sulphuric acid and water form three distinct curves representing the crystallization of water, of the hydrate, $H_2SO_4 + H_2O$, and of sulphuric acid, and the highest point of each of these curves is in exact correspondence with the composition of the substance which crystallizes out. Solutions containing between 40 and 75 per cent. of sulphuric acid had not hitherto been frozen; but it appeared to the author that if his former deductions from the irregularities in the curves representing the densities and other properties of the solutions of the acid were correct, an independent curve representing the crystallization of a new hydrate should occupy this interval, and that this new hydrate should have the composition $H_2SO_4 + 5\frac{1}{2}H_2O$, or $H_2SO_4 + 4H_2O$. Experiment has proved it to be the latter. The two branches of the new curve rise from about $-80°$, and meet in a sharply marked angle at a point corresponding with the composition of the tetrahydrate, the temperature at which this point is reached being $-25°$. The tetrahydrate forms large, well-defined, hard crystals. The author regards the isolation of this hydrate as affording fresh confirmatory evidence of the hydrate theory of solution.—Additional observations on the magnetic rotation of nitric acid, and of hydrogen and ammonium chlorides, bromides, and iodides in solution, by Dr. W. H. Perkin, F.R.S. In his previous experiments, the author has limited his observations on nitric acid to the pure acid HNO_3; he has now examined a somewhat diluted acid, and the results indicate that HNO_3 unites with water, forming an acid analogous to orthophosphoric acid, viz. $(OH)_3NO$. The experiments on hydrogen chloride, bromide, and iodide were originally made on single samples in a very concentrated solution of each. These gave abnormally high results—rather more than twice the values calculated for the pure compounds—but on examination of solutions of different strengths, it was found that the rotation increases up to a dilution equivalent to about six or seven molecular proportions of water, to one molecular proportion of hydride, the value then remaining practically stationary. To see whether the solvent had any influence, a solution of hydrogen chloride in isoamyl oxide was examined, and was found to give values nearly identical with those calculated from the chlorine derivatives of the paraffins; and there can be little doubt that, if the other hydrides could be examined in a similar way, analogous results would be obtained. As union with water should reduce the rotations, the results are at present inexplicable. The compounds with ammonia and the compound ammonias have also been further examined; the results are remarkable when considered in relation to those afforded by the hydrides, as the rotations found, instead of being those calculated from the results obtained in the case of the paraffin derivatives, or those found in the case of hydrogen chloride dissolved in isoamyl oxide, nearly correspond with those required on the assumption that the hydrides are present in aqueous solution together with ammonia. The rotations, however, do not vary with the strength of the saline solutions. The author's explanation of this is that when the salts are dissolved in water, they dissociate almost entirely into the hydride and the amine, the hydride undergoing an increased rotation on account of its being in aqueous solution. In the case of triethylamine hydrochloride the numbers are lower, and there is evidently less dissociation; and in the case of tetrethylammonium chloride little or no dissociation appears to take place. Solutions of ammonium iodide and diethylamine hydrochloride in absolute alcohol gave somewhat lower numbers than aqueous solutions, indicating somewhat smaller, although still large, amount of dissociation. Ammonium nitrate and acid ammonium sulphate in aqueous solution give numbers agreeing closely with the calculated values, and apparently do not dissociate to any appreciable extent. In the discussion which followed the reading of this paper, Dr. Gladstone, F.R.S., stated that, on examining Dr. Perkin's solution of hydrogen chloride in isoamyl oxide, he found that the refraction and dispersion values deduced for the chloride are very much smaller than those afforded by aqueous solutions.—Phosphoryl trifluoride, by Prof. T. E. Thorpe, F.R.S., and Mr. F. J. Hambly. Phosphorus oxyfluoride, POF_3, may be easily and conveniently made by heating a mixture of cryolite and phosphoric oxide, and collecting the products at the mercurial trough.—Acetylation of cellulose, by Messrs. C. F. Cross and E. J. Bevan. On heating cotton cellulose with acetic anhydride and zinc chloride, a product is obtained which appears to be a pentacetyl derivative of cellulose. The compound is very stable, and on alkaline hydrolysis yields a substance having the properties of a normal cellulose. It would therefore appear that all the oxygen of the cellulose molecule acts as hydroxylic oxygen, and, in view of this result, a reconsideration of the present ideas as to the constitution of cellulose is rendered necessary.—Action of light on moist oxygen, by Dr. A. Richardson. The presence of liquid water very much facilitates the oxidation of many substances under the combined influence of sunlight and oxygen, but if the water is present as aqueous vapour, the decomposition is exceedingly slow, and in some cases is entirely arrested. The author finds that peroxide of hydrogen is formed when water containing pure ether, or pure water acidified with pure sulphuric acid, is exposed to light in an atmosphere of oxygen, and draws the conclusion that the oxidation of substances under the influence of light involves in many cases initially an oxidation of water to hydrogen peroxide, and that the oxidation of the compound is the result of a secondary interaction between it and the hydrogen peroxide. In the discussion which followed the reading of the paper, Prof. Armstrong pointed out that, whilst Dr. Richardson assumed that water was directly oxidized when mixed with ether and exposed to oxidation, Mr. Kingzett had argued—and in the case of turpentine had adduced weighty experimental evidence—that the hydrogen peroxide was a secondary product formed by the action of water on an organic peroxide. The use of ether or sulphuric acid, which Dr. Richardson had added with the object of protecting the peroxide, was to be deprecated, since hydrogen peroxide in weak solutions was comparatively stable; no satisfactory evidence had been adduced that the peroxide is formed in the absence of a third substance when water and oxygen are exposed to light. Prof. Dunstan remarked that he had found that hydrogen peroxide was not formed when pure ether was used, although a substance was obtained which was capable of liberating iodine from potassium iodide. The President said that in experiments which he and Captain Abney had made together on the fading of water-colours, the action of aqueous vapour had been most strikingly apparent; colours were found to be stable on exposure to light in dry air, which were considerably affected when aqueous vapour was present.—α-β-dibenzoylstyrolene and the constitution of Zinin's lepiden derivatives, by Prof. F. R. Japp, F.R.S., and Dr. F. Klingemann. The authors have continued their investigation of the interactions of dibenzoylstyrolene (anhydracetophenonebenzil), and find that there is an almost perfect parallelism in behaviour between it and one of the three isomeric oxy epidens prepared by Zinin, viz. the "acicular oxylepiden" melting at 220°. The various compounds obtained by them stand to the corre-

sponding compounds of the lepiden series in the relation of triphenyl derivatives of furfuran to tetraphenyl derivatives, a relation which is exhibited in the first place by dibenzoylstyrolene and oxylepiden themselves. Like "acicular oxylepiden," dibenzoylstyrolene yields two isomeric derivatives on heating; the isomeride formed in larger quantity in each case is almost certainly a derivative of crotolactone, whilst the isomeride formed in smaller quantity is probably a stereometric isomeride of "acicular lepiden" and dibenzoylstyrolene respectively.—Ethylic $\alpha\alpha_1$-diacetyladipate, by Prof. W. H. Perkin.—(1 : 2) methylethylpentamethylene, by Dr. T. R. Marshall and Prof. W. H. Perkin.—Action of reducing agents on α-ω-diacetylpentane; formation of (1 : 2) methylethylhexamethylene, by Dr. F. S. Kipping and Prof. W. H. Perkin.—Action of reducing agents on α-ω-diacetylpentane; formation of (1 : 2) dimethylheptamethylene, by the same.—Oxyamidosulphonates and their conversion into hyponitrites, by Dr. E. Divers, F.R.S., and Mr. T. Haga. The oxyamidosulphonates are the sulphazidates of Fremy, which Claus and Raschig have shown to be monosulphonic derivatives of hydroxylamine. The authors find that these compounds on treatment with alkali, instead of yielding hydroxylamine and the alkaline sulphate as asserted by Claus and Raschig, and as it is admitted they do when hydrolyzed by an acid, are converted exclusively into sulphite and hyponitrite, thus, $2HO . NH . SO_3K + 4KHO = (KON)_2 + 2K_2SO_3 + 4H_2O$. The reducing action of the oxyamidosulphonates has been examined, and it is found that the generally accepted view that it is due to the supposed conversion of these salts into sulphate and hydroxylamine, the latter then acting upon the copper hydroxide in the usual way, is untenable.—The alloys of lead, tin, zinc, and cadmium, by Mr. A. P. Laurie. In extension of his previous observations (Trans. Chem. Soc., 1888, 88), the author has made voltaic cells with the various alloys, and has thus compared their behaviour with that of the single metal by means of an electrometer. He concludes that the metals now examined do not combine together, thus confirming Matthiessen's conclusions.

November 21.—Dr. W. J. Russell, F.R.S., President, in the chair.—The following papers were read:—The law of the freezing-points of solutions, by Mr. S. U. Pickering. —The constituents of flax, by Messrs. C. F. Cross and E. J. Bevan. As a result of their examination of the cuticular constituents of the fibre, the authors have isolated ceryl alcohol, two fatty acids, of which one appears to be cerotic acid, an oily ketone, and a residue of complex, ill-defined, inert compounds yielding "ketones" on hydrolysis. These "ketones" have the characteristic odour of raw flax and flax goods, and from their property of emulsifying with water undoubtedly exercise an important influence on the wet processes of fine spinning of flax. The pectic group of constituents associated with the cellulose in the fibre proper is found to yield mucic acid on oxidation with dilute nitric acid, and flax cellulose when oxidized with potassium permanganate yields, in addition to oxycellulose and oxalic acid, acid substances from which furfural is obtained on acid hydrolysis.—Acetylcarbinol (acetol), by Prof. W. H. Perkin and Dr. J. B. Tingle. The authors announce the preparation of anhydrous acetylcarbinol.

Zoological Society, November 19.—Prof. W. H. Flower, F.R.S., President, in the chair.—The Secretary read a report on the additions that had been made to the Society's Menagerie during the month of October 1889, and called special attention to the arrival of a young male Gaur (*Bibos gaurus*) from Pahang, one of the native States in the Malay Peninsula, presented to the Society by Sir Cecil C. Smith, the Governor of the Straits Settlement.—The President exhibited and made remarks on a head of an African Rhinceros (*Rhinoceros bicornis*) with a third posterior horn partially developed. The animal from which it was taken had been shot by Sir John Willoughbey, in Eastern Africa.—The Secretary exhibited a skin of an albino variety of the Cape Mole-Rat (*Georychus capensis*), forwarded to the Society by the Rev. G. H. R. Fisk, of Cape Town.—Mr. A. Smith-Woodward exhibited and made remarks on a portion of the rostrum of an extinct Saw-fish (*Sclerorhynchus*) from the chalk of Mount Lebanon.—Mr. Goodwin exhibited and made remarks on specimens of some rare Paradise Birds obtained by him on Mount Owen Stanley, New Guinea, when in company with Sir William Macgregor's recent expedition; also some photographs taken on the same occasion.—A communication was read from the Rev. Thomas R. R. Stebbing and Mr. David Robertson containing the descriptions of four new British Amphipodous Crustaceans. These were named *Sophrosyne robertsoni*, *Syrrhoe fimbriata*, *Podoceropsis palmatus*, and *Podocerus cumbrensis*. Of these, *Sophrosyne robertsoni* belonged to a genus first observed at Kerguelen Island.—Mr. G. W. Butler read a paper on the subdivison of the body-cavity in Lizards, Crocodiles, and Birds, in which an attempt was made to analyze the complex conditions of the membranes observable in the last two groups, and to express them in terms of the simpler structures found in the Lizards.—Mr. J. H. Leech read the third part of his paper on the Lepidoptera of Japan and Corea, comprising an account of the *Noctuæ* and *Deltoidæ*; in all upwards of 475 species. Of these forty-six were now described as new to science, and two others were considered to be varietal forms. —Mr. R. Lydekker read a paper on the remains of a Theriodont Reptile from the Karoo System of the Orange Free State. The remains described were an associated series of vertebræ and limb-bones of a comparatively large Theriodont, which was probably different from any described form. The humerus was of the normal Theriodont type, and quite distinct from the one on which the genus *Propappus* had been fonnded, which the author considered to belong to a form closely allied to, if not generically identical with, *Pariasaurus*.—Mr. G. B. Sowerby read the descriptions of thirteen new or rare species of Land-Shells from various localities.—A communication was read from Mr. Edward A. Minchin containing an account of the mode of attachment of the embryos to the oral arms of *Aurelia aurita*. It was shown that the embryos of *Aurelia aurita* are developed on the arms, in broad capsules formed as evaginations of the walls of the oral groove, and that the capsules increase in size with the addition of more embryos.

Linnean Society, November 21.—Mr. W. Carruthers, F.R.S., President, in the chair.—Prof. Duncan exhibited and made remarks on a stem of *Hyalonema Sieboldii*, dredged between Aden and Bombay, a remarkable position, inasmuch as this Glass Sponge had not previously been met with in any waters west of the Indian Peninsula. Prof. Stewart criticised the occurrence, and referred to a parasite on the Sponge which had been found to be identical with one from the Japanese seas. —Mr. James Groves exhibited and gave some account of a new British Chara, *Nitella batrachiosperma*, which had been collected in the Island of Harris.—Mr. Thomas Christy exhibited some bark of *Quillaia saponaria* from Chili, which has the property of producing a great lather, and is extensively used for washing silk and wool. It is now found to solidify hydrocarbon oils and benzoline, and thereby to insure their safe transport on long voyages; a small infusion of citric acid rendering them again liquid.—Dr. F. Walker exhibited and made remarks on some plants collected by him in Ireland.—Mr. W. Hachett Jackson gave an abstract of an elaborate paper on the external anatomical characters distinctive of sex in the chrysalis, and on the development of the azygos evident in *Vanessa Io*.—Mr. E. B. Poulton followed by giving a *résumé* of his researches on the external morphology of the Lepidopterous pupa.—Mr. J. H. Leech gave an account of some new Lepidoptera from Central China.

PARIS.

Academy of Sciences, December 2.—M. Hermite in the chair.—On the fermentation of stable manure, by M. Th. Schlœsing. A series of experiments has been carried out by the author for the purpose of ascertaining whether, during fermentation under cover from the air, the manure of farmyards liberates nitrogen, as it is known to liberate a mixture of carbonic acid and methane. He finds that at the temperature of 52° C. no gaseous nitrogen is generated from the decomposition of nitric compounds; nor is any nitric combination formed by oxidation of ammonia in presence of organic substances. The organic matter loses more carbon than oxygen, the proportion of hydrogen remaining about the same. The reading of the paper was followed by some remarks by M. Berthelot on the same subject.—Remarks on the diastases secreted by *Bacillus heminecrobiophilus*, by M. Arloing. These researches show that under artificial cultivation this organism secretes several soluble ferments, enabling it to prepare for assimilation all the organic substances needed for the nutrition and development of a living being; and that amongst these ferments, or associated with them, there is one that transforms the organic matter, while liberating gases—that is, exercises a function hitherto attributed to the micro-organisms themselves, and not to their secretions.—Verbal report on the work of E. D. Suess, entitled "Das Antlitz der Erde," vols. i. and ii., 1885 and 1888, by M. Daubrée. This fundamental treatise on the constitution of the earth is here

described as a summary of the facts already established regarding the geology of the various parts of the globe, the essential features of its present mountain ranges and depressions, and the successive movements of the terrestrial crust of which these are the outcome. The work marks a new departure in the progress of physical geography.—Observations of Swift's new comet made with the Brunner equatorial at the Observatory of Toulouse, by M. B. Baillaud; and with the large equatorial at the Observatory of Bordeaux, by MM. G. Rayet and Picart. All these observations, extending from November 21 to November 27, give the same results: comet very faint and greatly diffused, making observations very difficult. Tables are also given of observations made at Algiers by MM. Trépied, Ramband, Sy, and Renaux, during the same period.—Mechanical realization of thermodynamic phenomena, by M. Chaperon. Purely mechanical systems may be conceived, which present a striking analogy to heat-engines in respect of their influence on finite movements. The author here describes one of these systems, which is distinguished by its extreme simplicity.—On the correspondence between the characteristic equations of gases, by M. Ladislas Natanson. The author here shows that Wroblewski's posthumous memoir, published by the Vienna Academy in November 1888, forms a natural complement to Van der Waal's law that at absolute, that is, *corresponding* temperatures proportional to the critical temperatures of the different bodies, the pressures, P, of their saturated vapours are proportional to the respective critical pressures.—Method of measuring the spheric and chromatic aberrations of the objectives of the microscope, by M. C. J. A. Leroy. Finding in an artificial eye certain effects connected with the aberrations of sphericity and refrangibility, the author has applied the method known as "Cuignet's keratoscopy" to the study of the aberrations of the eye, and of the objectives of the microscope. His present observations are confined to the objectives alone.—On the electric conductivity of the Eiffel Tower and its conductors, by M. A. Terquem. It is shown that the tower with its complete system of lightning conductors, constructed under the direction of MM. Becquerel, Berger and Mascart, is calculated to afford perfect security for a considerable space round about.—Fresh researches on the preparation and density of fluorine, by M. Henri Moissan.—Papers were submitted by M. Daniel Berthelot, on the electric conductivities and multiple affinities of aspartic acid; by MM. E. Jungfleisch and L. Grimbert, on some facts relative to the analysis of sugars; by M. G. Colin, on the varying effects of virulent substances used for inoculating animals; by M. P. Fliche, on the silicified woods of Algeria; by M. Stanislas Meunier, on the Phu-Hong meteorite, with remarks on the limerick type; and by M. Léon Teisserenc de Bort, on the distribution of atmospheric pressure over the surface of the globe.

DIARY OF SOCIETIES.

LONDON.

THURSDAY, DECEMBER 12.

ROYAL SOCIETY, at 4.30.—The Relation of Physiological Action to Atomic Weight: Miss H. J. Johnstone and Prof. T. Carnelley.—An Experimental Investigation into the Arrangement of the Excitable Fibres of the Internal Capsule of the Bonnet Monkey (Macacus sinicus): Dr. Beevor and Prof. V. Horsley, F.R.S.—On the Effect of the Spectrum on the Haloid Salts of Silver: Capt. Abney, F.R.S., and G. S. Edwards.—Magnetic Properties of Alloys of Nickel and Iron: Dr. Hopkinson, F.R.S.

MATHEMATICAL SOCIETY, at 8.—On the Radial Vibrations of a Cylindrical Shell: A. B. Basset, F.R.S.—Note on 51840-Group: G. G. Morrice.—On the Flexure of an Elastic Plate: Prof. H. Lamb, F.R.S.—Notes on a Plane Cubic and a Conic: R. A. Roberts.—Complex Multiplication Moduli of Elliptic Functions for the Determinants − 53 and − 61: Prof. G. B. Mathews.

INSTITUTION OF ELECTRICAL ENGINEERS, at 8.—Annual General Meeting.—Election of Council and Officers for 1890.—Electrical Engineering in America: G. L. Addenbrooke. (Discussion.)

FRIDAY, DECEMBER 13.

ROYAL ASTRONOMICAL SOCIETY, at 8.

QUEKETT MICROSCOPICAL CLUB, at 8.

INSTITUTION OF CIVIL ENGINEERS, at 7.30.—Hydraulic Station and Machinery of the North London Railway, Poplar: John Hale.

SATURDAY, DECEMBER 14.

ROYAL BOTANIC SOCIETY, at 3.45.

SUNDAY, DECEMBER 15.

SUNDAY LECTURE SOCIETY, at 4.—The Geology of London (with Oxy-hydrogen Lantern Illustration): Rev. J. F. Blake.

MONDAY, DECEMBER 16.

SOCIETY OF ARTS, at 8.—Modern Developments of Bread-making: William Jago.

ARISTOTELIAN SOCIETY, at 8.—Symposium—Is there Evidence of Design in Nature?: S. Alexander, Dr. Gildea, Miss Naden, G. J. Romanes.

TUESDAY, DECEMBER 17.

ROYAL STATISTICAL SOCIETY, at 7.45.—Accumulations of Capital in the United Kingdom in 1875–85 (with reference to a Paper read in 1878): Dr. Robert Giffen.

INSTITUTION OF CIVIL ENGINEERS, at 8.—On the Triple-Expansion Engines and Engine Trials at the Whitworth Engineering Laboratory, Owens College, Manchester: Prof. Osborne Reynolds, F.R.S. (Discussion.)

UNIVERSITY COLLEGE BIOLOGICAL SOCIETY, at 5.15.—Amphioxus: C. E. Franck.

WEDNESDAY, DECEMBER 18.

SOCIETY OF ARTS, at 8.—London Sewage: Sir Robert Rawlinson, K.C.B.

GEOLOGICAL SOCIETY, at 8.—On the Occurrence of the Genus Girvanella, and Remarks on Oolitic Structure: E. Wethered.—On the Position of the Westleton Beds or "Pebbly Sands" of Suffolk to those of Norfolk, and on their Extension Inland, with some Observations on the Period of the Final Elevation and Denudation of the Weald and of the Thames Valley, Part 2: Prof. Joseph Prestwich, F.R.S.

ROYAL METEOROLOGICAL SOCIETY, at 7.—Report of the Wind Force Committee on the Factor of the Kew Pattern Robinson Anemometer: drawn up by W. H. Dines.—On Testing Anemometers: W. H. Dines.—On the Rainfall of the Riviera: G. J. Symons, F.R.S.—Report on the Phenological Observations for 1889: Edward Mawley.

UNIVERSITY COLLEGE CHEMICAL AND PHYSICAL SOCIETY, at 4.30.—The Magnetization of Iron and Nickel: J. J. Stewart.

THURSDAY, DECEMBER 19.

ROYAL SOCIETY, at 4.30.

LINNEAN SOCIETY, at 8.—Intensive Segregation and Divergent Evolution in Land Mollusca of Oahu: Rev. John T. Gulick.—Dictopteris; with Remarks on the Systematic Position of the Dictyotaceæ: T. Johnson.

CHEMICAL SOCIETY, at 8.—On Frangulin: Prof. Thorpe, F.R.S., and H. H. Robinson.

ZOOLOGICAL SOCIETY, at 4.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Australia Twice Traversed, 2 vols: E. Giles (Low).—Physiology of Bodily Exercise: Dr. E. Lagrange (Kegan Paul).—Linear Differential Equations, vol. i.: Dr. T. Craig (Trübner).—Philosophy of the Steam-Engine: R. H. Thurston (Trübner).—The British Journal Photographic Almanac, 1890 (Greenwood).—Absolute Measurements in Electricity and Magnetism, 2nd edition: A. Gray (Macmillan).—Occasional Thoughts of an Astronomer on Nature and Revelation: Rev. Dr. Pritchard (Murray).—Star-Land: Sir R. S. Ball (Cassell).—The Story of Chemistry: H. W. Picton (Isbister).—A Text-book of Assaying: C. Beringer and J. J. Beringer (Griffin).—History and Pathology of Vaccination, 2 vols.: Prof. E. M. Crookshank (Lewis).

CONTENTS.

THURSDAY, DECEMBER 19, 1889.

THE EPIDEMIC OF INFLUENZA.

FOR the first time after an immunity of nearly half a century, our country is again threatened with an epidemic of influenza. The accounts we receive of epidemic illness in Russia, in Germany, and last of all in Paris, seem to make its irruption here every week more imminent. The question will, however, naturally be asked by the public, whether there is any real ground, in the history and in what is known of the nature of the disease, for such an apprehension? Is it a disease really brought from a distance? Is it anything more than the general prevalence of catarrhal affections, of colds and coughs, which the time of year, and the remarkably unsettled weather we have lately experienced, make readily explicable without any foreign importation? Indeed, is influenza, after all, anything more than a severe form of the fashionable complaint of the season?

To answer the last question first, and so to put it by, there can be little doubt that influenza is a distinct, specific affection, and not a mere modification of the common cold. The grounds for this belief cannot be fully stated here, but may be gathered by reference to the descriptions of the disease as seen in former outbreaks by physicians of the older generation; for instance, by Sir Thomas Watson in his classical "Principles of Physic," or the late Dr. Peacock in his article in Quain's "Dictionary of Medicine."

These symptoms, the history of the disease, and its distribution, all justify us in treating it as a distinct and specific disease, which when it is prevalent will rarely be mistaken, though, with regard to isolated and sporadic cases, difficulties of diagnosis may arise. About its nature, or its affinities with other diseases, it is unnecessary to speculate. It will be sufficient to inquire what its recorded history in the past justifies us in expecting as to its behaviour in the future. There are few cases in which history proves so important an element in the scientific conception of a disease as it does in that of influenza. For hardly any disease shows a more marked tendency to occur in epidemics—that is, in outbreaks strictly limited in point of time. After long intervals of inaction or apparent death, it springs up again. Its chronology is very remarkable. Though probably occurring in Europe from very early times, it first emerged as a definitely known historical epidemic in the year 1510. Since then, more than 100 general European epidemics have been recorded, besides nearly as many more limited to certain localities. Many of them have in their origin and progress exhibited the type to which that of the present year seems to conform. We need not go further back than the great epidemic of 1782, first traceable in Russia, though there believed to have been derived from Asia. In St. Petersburg, on January 2, coincidently with a remarkable rise of temperature from 35° F. below freezing to 5° above, 40,000 persons are said to have been simultaneously taken ill. Thence the disease spread over the Continent, where one-half of the inhabitants were supposed to have been affected, and reached England in May. It was a remarkable feature in this epidemic that two fleets which left Portsmouth about the same time were attacked by influenza at sea about the same day, though they had no communication with each other or with the shore.

There were many epidemics in the first half of this century; and the most important of them showed a similar course and geographical distribution. In 1830 started a formidable epidemic, the origin of which is referred to China, but which at all events by the end of the year had invaded Russia, and broke out in Petersburg in January 1831. Germany and France were overrun in the spring, and by June it had reached England. Again, two years later, in January 1833, there was an outbreak in Russia, which spread to Germany and France successively, and on April 3, the first cases of influenza were seen in our metropolis; "all London," in Watson's words, "being smitten with it on that and the following day." On this same fateful day Watson records that a ship approaching the Devonshire coast was suddenly smitten with influenza, and within half an hour forty men were ill. In 1836 another epidemic appeared in Russia; and in January 1837, Berlin and London were almost simultaneously attacked. Ten years later, in 1847, the last great epidemic raged in our own country, and was very severe in November, having been observed in Petersburg in March, and having prevailed very generally all over Europe.

Some of these epidemics are believed to have travelled still further westward, to America; but the evidence on this point seems less conclusive. Without entering on further historical details, and without speculating on the nature of the disease, we may conclude that these broad facts are enough to show that a more or less rapid extension from east to west has been the rule in most of the great European epidemics of influenza; and that therefore its successive appearance in Russia, Germany, and France, makes its extension to our own country in the highest degree probable.

There are, it is true, certain facts on the other side, but they appear much less cogent. Since our last great visitation, certain epidemics of influenza have been recorded on the Continent which have not reached our shores. One was that of Paris in 1866–67; another at Berlin in 1874–75, of a disease described by the German doctors as influenza, and of great severity, affecting all classes of society. But in all epidemic and even contagious diseases there are outbreaks which seem to be self-limited from the first, showing no tendency to spread. This has been notably the case with plague and cholera. On the other hand, when an epidemic shows an expansive and progressive character, it is impossible to predict the extent to which it may spread. And the present epidemic, it must be confessed, appears to have this expansive character.

Many interesting points are suggested by this historical retrospect. What is the meaning of the westward spread of influenza, of cholera, and other diseases? Is it a universal law? To this it must be said, that it is by no means the universal law even with influenza, which has spread through other parts of the world in every kind of direction, but it does seem to hold good for Europe, at least in the northern parts. The significance of this law,

as of the intermittent appearances of influenza, probably is that this is in Europe not an indigenous disease, but one imported from Asia. Possibly we may some day track it to its original home in the East, as the old plague and the modern cholera have been traced.

As regards, however, the European distribution of influenza, it has often been thought to depend upon the prevalence of easterly and north-easterly winds. There are many reasons for thinking that the contagium of this disease is borne through the air by winds rather than by human intercourse. One reason for thinking so is that it does not appear to travel along the lines of human communications, and, as is seen in the infection of ships at sea, is capable of making considerable leaps. The mode of transmission, too, would explain the remarkable facts noticed above of the sudden outbreak of the disease in certain places, and its attacking so many people simultaneously, which could hardly be the case if the infection had to be transmitted from one person to another.

Another important question, and one certain to be often asked, is suggested by the last—namely, whether influenza is contagious. During former epidemics great care was taken to collect the experience of the profession on this point, and its difficulty is shown by the fact that opinions were much divided. Some thought the disease could be transmitted by direct contagion, while others doubted it. But there was and is a general agreement that this is not the chief way in which the disease spreads, either in a single town, or from place to place.

We must avoid the fascinating topic of the cause of influenza, or our limits would be speedily outrun. But one simple lesson may be drawn from the facts already mentioned—namely, that the disease is not produced by any kind of weather, though that, of all possible causes of disease, is the one most often incriminated in this country. It is true that some of our worst epidemics have occurred in winter, but several have happened in summer; and the disease has been known in all parts of the world, in every variety of climate and atmospheric condition; so that it is certainly not due to a little more or less of heat or cold, moisture or dryness. Its constancy of type, the mode of its transmission, its independence of climatic and seasonal conditions, all suggest that its cause is "specific,"—that is, having the properties of growth and multiplication which belong to a living thing.

Whether the disease affects the lower animals is not absolutely certain, but the human epidemic has often been preceded or accompanied by an epidemic among horses of a very similar disease. It is pretty well known that such a disease is now very prevalent among horses in London. Nearly three weeks ago, one of the railway companies in London had 120 horses on the sick list, and the epidemic is still by no means extinguished. To a certain extent this must be taken as prognostic of human influenza.

It may be asked, if the influenza is really to come, can we form any notion how soon it is likely to appear? On such a point little beyond speculation is possible, for the rate at which the disease travels is extremely variable. Generally, it has taken some weeks, or even months, to traverse Europe, but occasionally much less, as, for instance, in 1833, when it appeared to travel from Berlin to Paris in two days. It is now barely a month since the epidemic became noticeable in Petersburg, where, according to a correspondent of the *British Medical Journal*, it began on November 15 or 17, though sporadic cases had undoubtedly occurred earlier. In the beginning of December it was already widely spread throughout Russsia, and, as it would seem from the published accounts, must have been in Berlin about the same time. In Paris the first admitted and recorded cases occurred about December 10, though doubtless there were cases before that date. Both public and private accounts report it exceedingly prevalent there now. In London, notwithstanding the abundance of colds and coughs, and the mysterious rumours which have been afloat, it appears to the present writer doubtful whether any cases of true influenza have yet occurred. But according to its apparent rate of progress, it might, if coming from Paris, have already arrived here; and it may be breaking out even while these lines are going through the press. But, on the whole, one would be disposed to give the epidemic another week or two. If its distribution depends, as it seems to do, on the winds, it is impossible to prophesy with much plausibility. A steady breeze setting in from one of the affected places might bring us an invasion in a very short time; but the current of air would have to be continuous over the whole district. Light local winds, whatever their direction, would, if the hypothesis be correct, have little effect. On the other hand, a steady frost, with an "anticyclone" period, might effectually keep off the disease. If, then, there is anything in the views above stated, prophecy belongs rather to the province of the weather-doctors than of the medical doctors.

Should the prospect seem a grave one, it may be some consolation to remember that an epidemic of influenza rarely lasts more than a few weeks—three to six—in one place; that it is rarely a fatal disease, though affecting large numbers of people; and that the present epidemic seems to have displayed on the Continent a decidedly mild type, which, according to the general rule, it is likely to retain. J. F. P.

THE HORNY SPONGES.

A Monograph of the Horny Sponges. By Robert von Lendenfeld. (London: Published for the Royal Society by Trübner and Co., Ludgate Hill, 1889.)

WITHIN the last few years, and as a direct result of the famous Expedition of the *Challenger*, three most important monographs of the sponges belonging to the groups of the Hexactinellida, Monaxonida, and the Tetractinellida have been published, nor must the valuable contributions by Poléjaeff to the history of the remaining groups, Calcarea and Keratosa, be overlooked. The Calcarea had the advantage of having been already monographed by Haeckel, and so there only remained the Horny Sponges to be fully described, in order that the natural history of the sponges should be up to date.

Such a work has now been accomplished—thanks to the liberality of the Royal Society—by the labour and scientific skill of Dr. Robert von Lendenfeld. This monograph forms a fine quarto volume of over 900 pages, with an atlas of fifty lithographed plates.

While a student at the University of Graz, Lendenfeld

tells us, his time was chiefly spent in the zoological laboratory of Prof. F. E. Schulze, then engaged on those researches on the natural history of sponges with which his name will ever be associated. This led him to take a special intest in the group, and to work out its history, first in the Mediterranean, and then at Melbourne and other places on the southern coast of Australia—a coast exceedingly rich in organisms of this class. From Melbourne, New Zealand was visited, and the Christchurch and Dunedin collections were examined. Next, that apparent El Dorado of the spongologist, Sydney, was explored, and, thanks to the splendid liberality of Sir William Macleay, Lendenfeld was enabled to establish a laboratory at the water-edge, and to study in a very thorough manner the sponges of this district.

With such abundant material, and with such ready help, nothing was wanting to work out the structural history of the species of the group. But to describe and name them, reference to type specimens was, above all things, necessary, and these latter were to be found most conveniently in the British Museum; thither, therefore, Lendenfeld came, early in 1886, at first resolved to write an account of the Australian Horny Sponges; but fortunately finding, during the progress of this work, that so great a proportion of the known forms were Australian, he determined to make a complete monograph of the group, and hence the volume which we proceed to notice.

This monograph of the Horny Sponges is divided into three parts: (1) an introduction, containing a brief historical summary and a detailed list of publications relating to sponges; (2) an analytical portion, devoted to the systematic description of all the known Horny Sponges; and (3) a synthetical part, in which the anatomy and physiology of sponges, especially of Horny Sponges, are treated, and their phylogeny, systematic position, and classification discussed.

Of the very extensive and scattered literature relating to the sponges, a most excellent bibliography is given; the papers are arranged alphabetically under their authors' names, but the publications of each author are given chronologically; the number of pages in each memoir is given, but, unfortunately, no reference is made to illustrations; abstracts and translations of papers are always quoted.

Considering the genus as "the important unit," the analytical part consists essentially of a series of monographs of the genera of Horny Sponges, but "species" as such are described; and the author has "done his best to make the different species equivalent," though this has been difficult of achievement. In those cases where he has felt compelled to establish varieties, he has followed the plan of E. Haeckel and F. E. Schulze, and has divided the whole species into "the requisite number of equivalent varieties." The total number of the species and varieties described amounts to 348, of which no less than 258 have been found in the Australian area.

It would not be possible, within any reasonable space, to give any satisfactory details of the analytical portion of this monograph. The descriptions of each genus are grouped into—an historical introduction; a sketch of the shape, size, colour, surface, and rigidity characteristic of the group; an account of the canal system, skeleton, with notes on the histology and physiology; the affinities of the genus; statistics of the species, with a key thereto, and details of distribution. Doubts must of necessity arise as to the exact limits that each author would ascribe to the species described by him, and in doubtful cases of this sort Dr. Lendenfeld has adopted the plan of placing no authors' names after them, but gives a full list of synonyms; we think it a pity that in these lists the memoirs, instead of being quoted, are simply referred to by numbers, for the explanation of which one must refer to the bibliographical list.

It is in the synthetical part, in which the general results are discussed, that the chief interest of this work lies, at least for the general reader. Here we have the questions of the general structure and evolution of sponges as a group considered, and their classification and systematic position discussed; and finally, as the fashion of some authors is, "an ancestral tree of the families" is given. Starting with the story of the metamorphic development of sponges, we find the primitive sponge defined as consisting of a simple ento- and ectoderm, and a thin mesoglœa—a very primitive mesoderm—between the two. Dr. Lendenfeld thinks that it is now generally acknowledged that the Physemaria, which Haeckel considered as "Gastreaden der Gegenwart," are not sponges at all, but Protozoa, so that they need not here be taken into account. Of course, it is evident that the views about these Physemarias, held at present by Haeckel, were, at the time of his thus writing, unknown to Dr. Lendenfeld. The modified Gastræa is traced onwards in its development, and the morphology of the adult structures are passed under review; their want of symmetry—and the exceptions are but few—is noted. None of the Horny Sponges are green; blue is never observed in the group, the range of colour being from light yellow to dark brown, light to dark red, and light to a dark, almost black, violet; the colour is lost in all, with a few exceptions, such as in *Aplysilla violacea*, when the sponge is preserved. The Horny Sponges would seem never to imitate their surroundings in colour, but it is suggested that in some cases the intense vivid colours may have the effect of frightening their enemies.

An attempt is made to account for the shape of the sponge conuli as the result of two pressure forces and to express this by formula. The biological student will scarcely be grateful for this, and is likely to be bewildered when he reads that "the conuli are hyperbolic rotatory bodies, formed by the rotating of the hyperbola,

$$y = (p \cdot x)/(t + t \cdot x),$$

round an axis parallel to the direction of pressure through the summit of the conulus." The canal system is described in some detail, the author not confining himself to the Horny Sponges. In contrasting this system in the Hexactinellida and the Hexaceratina, there seems some little confusion as to the comparative "tenderness" of the structures. The absence of spicules (siliceous) in the fibres is considered as the characteristic feature of the Horny Sponges, which distinguishes them from their siliceous ancestors; but in the superficial fibres of Aulena, echinating proper spicules occur; in the ground substance of several genera of Spongeliadæ, microsclera are

to be found, while in Darwinella, triaxon horny spicules abound.

Very interesting accounts are given of the connective tissue, muscle cells, and nervous system. Stewart's account of the "palpocils" is accepted; and, although Prof. Stewart's specimens are the only ones which show these organs properly, yet Lendenfeld thinks that, when groups of converging sense-cells are observed (in sections) below the continuous surface, these may be regarded as the cells of a "retracted" palpocil.

The researches of the author have thrown but little fresh light on the subject of the occurrence of the strange "filaments" in the species of the genus Hircinia; these filaments are generally more abundant in the superficial layer than in the interior of the sponge. They may be isolated, or arranged in bundles of varying thickness, in which they are parallel. Such bundles are particularly conspicuous in *H. gigantea*, where they form a pretty uniform network which pervades the whole of the sponge. The filaments are never straight: they may be continuously and simply curved, or they are undulating. The latter form of curvature is particularly frequently observed in the filaments which are joined to form large bundles. While their abundance is subject to variation, no case of a sponge with but a few isolated filaments is on record. No apparent young stages of these filaments have been seen. Schulze's researches enabled him to make no positive statement concerning them, but they at the same time demonstrated that "no cellulose is contained in them, that they have no trace of true cellular structure, and that they contain a great deal of nitrogen (9·2 per cent. of their substance), and that they are not Algæ. The resistance of the filaments in boiling alkali is against their being ordinary Fungi, while their general chemical composition indicates no relationship to the ordinary sponge skeleton." As to the very minute dumb-bell shaped structures observed by Poléjaeff, and considered by him to be young stages of the filaments, Lendenfeld thinks that this is extremely doubtful, "particularly as nobody besides Poléjaeff has seen them in *H. friabilis* or any other sponge." But is this so? for in another paragraph we read:—

"The spherical bodies which Schmidt and Poléjaeff consider as young stages of these filaments—in fact, as terminal knots, either dropped off, or on the way to produce a filament—have also been observed and carefully studied by Schulze, who considers them as monocellular Algæ, which have nothing whatever to do with the filaments."

Lendenfeld says that "no trace of filaments or 'spores' can be detected in the young embryos which are often found in specimens of Hircinia."

On the physiology of the group, this monograph throws but little light:—

"Our knowledge of the vital functions of sponges is at present exceedingly unsatisfactory. We do not even know which parts of the sponge absorb nourishment, or, in fact, what kind of food the sponges take in. We are equally ignorant concerning their respiration and secretion."

There being then no facts to serve us as guides to knowledge, the next "best thing" is to have recourse to imaginations, and our author "thinks" that "it is by no means unlikely that the sponges may exclusively absorb liquid food—that is to say, organic substances dissolved in the water which is continuously passing through their canal system. All the other organisms in which arrangements are made to insure a continuous water current—I refer to the higher plants—absorb exclusively nourishing material in solution (the absorption of gaseous food by plants does not concern us here). The existence of a traversing canal system and a continuous water current seems to me to point to the nourishing material of sponges being in solution in the sea-water. The numerous fine sieves and filter arrangements generally, and the mere fact that the water always enters through the smaller holes and is expelled through the larger, clearly shows that the sponges are not desirous that large food-particles should enter their canal system."

Even granting that the word "exclusively" should be after the word "material," we do not quite understand the comparison of the well-known facts of plant physiology as they are presented to us in the above extract, nor see how it helps us to an understanding of how the sponge adds to its protoplasm; the undoubted power possessed by some of the sponge-cells to lay down silica, lime, &c., is quite different functionally from the phenomena attending growth and development, using these terms in Herbert Spencer's sense; but once set a thinking, our author proceeds, and telling us that a "tape-worm is an animal which takes up liquid food, and which has no special digestive apparatus, and that it evidently takes up a great quantity of material from the surrounding chyle through the apparently indifferent cylindrical ectodermal epithelium cells; that the excess material and waste products are got rid of by the nephrydia," he goes on to say that he is inclined "to think that in sponges we may have a similar mode of absorption of nourishment"; but then, where are the nephrydia or their analogues? and he thinks again "that it is not impossible that the ciliated chambers may be partly analogous to the nephrydia of the Cœlomata, and that the collar-cells may, besides performing other functions, also secrete the urine." However uncertain, he adds, this hypothesis may appear, "I think there can be no doubt that there is more probability in it than in the view, held by Carter and others of the older authors, that the ciliated chambers are merely digestive apparatus." This seems a rather dreamy hypothesis, with no facts for its foundation; but it is but fair to remark that it comes at the very end of a volume which is a record of numerous and important observations.

Under the headings variability, parasitism, and symbiosis, many interesting details are given. The author thinks that certain forms of Aulena and Chalinopsilla imitate "certain siliciferous Cornacuspongiæ. These sponges have descended from those which they imitate; and, whilst they have lost the spicules in the fibres, they have retained the outer appearance of their better protected ancestors in a most striking manner." Apparently, "the primordial sponge ancestors were free-swimming, and had no skeleton. Some produced a calcareous, others a siliceous skeleton; in both the subsequent development, the formation of ciliated chambers, which the ancestors did not possess, and the fixing of the axis and rays of the spicules, were the same. The primordial Silicea had indifferent irregular spicules, from which the

triaxon and the tetraxon spicules were developed by an adaptation of the divergent development of the canal system. The primordial forms of both lived in water rich in silica, and certain forms of both lost their spicules in consequence perhaps, of rising from deeper to shallower water, where silica is more scarce. In both, some forms have lost the skeleton altogether, while others have replaced it gradually by spongin."

While acknowledging that some authors whose opinions must carry great weight, such as Balfour, Bütschli, and Sollas, consider the sponges as a separate group, equal in value to the groups Protozoa and Metazoa, Lendenfeld cannot but conclude that the sponges are, without doubt, Metazoa, and certainly Cœlentera, in the sense of being provided with a simple body cavity.

The last twenty pages of the work are devoted to a synopsis of all the known sponges, giving the classes, families, orders, and genera. In this extremely useful list there is a short analysis of the families and orders, which is based on the labours of Vosmaer, Ridley, Dendy, Sollas, Schulze, added to those of the author's own. The author ends his treatise with the statement that "Now that all the groups of sponges have been thoroughly investigated, we may consider our knowledge of their phylogenetic affinities established on a satisfactory footing" (p. 909); but it seems well to call to mind the statement with which he closes his short preface, and with which we feel the more inclined to agree, "our present knowledge of the group . . . has only just arrived at a stage corresponding to the knowledge of the higher animals of half a century ago" (p. 5).

In concluding our only too brief notice of this important work, for which all workers on the group must thank Dr. Lendenfeld, we may mention that the sponge portraits are for the most part photo-lithographs taken from the original types; though in a few cases, where no good specimens were available, the lithographic illustrations are from drawings.

THE FLORA OF SUFFOLK.

The Flora of Suffolk. By W. M. Hind, LL.D., Rector of Honington, assisted by the late Churchill Babington, D.D., F.L.S. With a Chapter on the Geology, Climate, and Meteorology of Suffolk, by Wheelton Hind, M.D., F.R.C.S. Pp. 508, with a Map. (London: Gilbert and Jackson, 1889.)

SUFFOLK is a characteristic lowland maritime English county, the flora of which, at the present day, contains absolutely no infusion of the boreal element. Its area is about 1500 square miles. The whole surface is flat, without any prominent rocks. It is underlain by chalk, which, in the north and west, lies immediately below the subsoil, but, in the south and east, is covered by Tertiary and Glacial deposits, which at Harwich have been found to reach a thickness of 1000 feet before the chalk is reached. In White's history of the county, its soils are classified into three groups: heavy lands, in which clay predominates; mixed land, common mixed soil, rich deep moulds, fen-lands, and rich marshes; and light lands, consisting of sand over chalk. To the first set belong the soils of the western two-thirds of the county, except in the extreme north and near the coast. The mixed lands are found—one portion east of the heavy lands between the Orwell and the Stour; a second in the north, between Halesworth and Yarmouth; and a third west of the heavy lands between Holston and Newmarket. The sandy, or light, soils are in the extreme north-west, in what is called the "Breck district," between Thetford and Mildenhall, where are found the rarest plants of the county, such as *Veronica hybrida*, *V. triphyllos*, *V. verna*, and *Apera interrupta*. The coast is remarkable for the extent of its tidal estuaries and bays, creeks and havens. There are no cliffs of any considerable height, but a great extent of sand and shingle. The beach at Orford, where grows the great mass of *Lathyrus maritimus*, the seeds of which saved the life of many poor people in a famine in the middle of the sixteenth century, is said to have the greatest breadth of sand anywhere on the English coast. The rivers are shallow streams with slow currents. In the north-east there are several lakes of brackish water, not so well known as the Norfolk Broads, of which Braydon Water covers 1200, and Thorpe Mere 1000, acres. The fresh-water lakes of the county are few and small. There is a considerable area of fen- and marsh-land, both in the north-west and east, so that we get in the county all the conditions that produce a rich low-country flora, and, superadded to the common lowland plants, rarities characteristic of chalk country, the seashore, and fen-land ditches and marshes.

The country is so easy of access from the centres where have lived many of the best botanists of bygone time, such as London, Cambridge, Yarmouth, Norwich, and Saffron Walden, that the principal features of its botany have long been known, and many excellent botanists, from the time of Buddle down to the present day, have resided within its compass. The father of Suffolk botany was Sir John Cullum, F.R.S., who lived near Bury St. Edmunds, and kept a diary between 1772 and 1785, in which he has recorded the occurrence of upwards of 500 plants. To his son, Sir Thomas Cullum, F.R.S., who was also an enthusiastic botanist, Sir J. E. Smith dedicated his "English Flora." In the present work there is not only a full general history of the progress of Suffolk botany, but, under each plant, the name of its first known collector is registered. The first "Flora" of the county was published in 1860. It was carried out mainly by the exertions of the late Mr. E. Skepper, working under the superintendence of Prof. Henslow. After it was published, Mr. Skepper made a great many notes for a new edition, but he died in 1867. For several years the Rev. Churchill Babington, who settled in the county in 1866, paid attention to the subject. In 1875, the Rev. W. M. Hind, a very competent botanist, well known by his "Flora of Harrow," settled in the county, and Dr. Babington sought and obtained his assistance to carry on the work. Dr. Babington died early in the present year.

The bulk of the book is, of course, occupied by the enumeration of the species and an account of the distribution and special localities of the varieties. The county is divided into five districts, and the distribution of the plants is traced through them. Only the Phanerogamia and Vascular Cryptogamia are dealt with, but the mosses of the county have also been well worked.

There is also a detailed tabular comparison of the plants of Suffolk with those of Norfolk, Cambridgeshire, and Essex, and a short chapter on the characteristic plants of the different soils of the county, which will be found very interesting to students of plant-dispersion. The chapters contributed by Dr. Wheeler Hind, the son of the editor, on the geology, physical geography, and meteorology of the county are very full, clear, and add greatly to the interest of the book.

One of the most interesting circumstances in the county flora is the occurrence of several maritime plants far inland. In the Breck country, between Thetford and Mildenhall, grow *Vicia lutea*, *Erythræa littoralis*, *Rumex maritimus*, *Carex arenaria*, *Phleum arenarium*, and *Corynephorus canescens*. These are all seaside plants, and their occurrence fifty miles inland is accounted for by Prof. Newton and the editor by supposing that an arm of the sea has penetrated here southward from the Wash at a comparatively recent period.

It is in Norfolk and Suffolk that the most valuable observations have been made, by Mr. Clement Reid and his fellow-workers, in illustration of the time of origin of our present British flora. The Cromer plant-bed extends into Suffolk, past Pakefield, to Southwold and Dunwich. This is pre-glacial, and yet, out of upwards of forty plants found in it that have been clearly identified, there are only two that are not British now—the spruce fir and *Trapa natans*. At Hoxne, near Diss, lacustrine deposits have been found resting on a bed of boulder clay, but beneath beds which contain bones of the elephant. In these are contained *Salix polaris*, *S. Myrsinites*, *Betula nana*, *Hypnum sarmentosum*, and a *Pinus* which is probably *sylvestris*—all characteristic Arctic-Alpine types, associated with many lowland plants which grow unchanged in Suffolk at the present time. A chapter in the book contains a list of all these plants, but their geological position is not clearly explained.

It will be seen that this is a very interesting and complete county flora, and that it is worthy of being studied carefully by all who are interested in the distribution of our indigenous plants. J. G. B.

THE MANUFACTURE OF IRON AND STEEL.

Iron and Steel Manufacture. By Arthur H. Hiorns. (London: Macmillan and Co., 1889.)

THIS volume is meant as a text-book for beginners, and will very worthily occupy that position. It is full of information, and information of the very kind which the student should possess before entering upon the study of the greater works of Percy or Phillips. On the other hand, those already engaged in the metallurgy of iron and steel will find in these pages much that may be referred to.

The book begins with a brief history of the processes that have been employed down to our own time, the landmarks in which are Dud Dudley's successful attempts to smelt with coal at the beginning of the seventeenth century; Cort's introduction of the puddling process in 1784; Neilson's recommendation to use hot blast in 1828; the revolution produced in the iron trade by the invention of the Bessemer steel process in 1855, as supplemented by R. F. Mushet, of the Siemens furnace and steel process, and finally of Thomas and Gilchrist's basic process.

The chapter which deals with chemical principles and changes, inserted for the benefit of those having a limited knowledge of chemistry, is valuable on account of the simple manner in which it is written; this is particularly the case as regards oxidizing and reducing agents, the examples given of oxidation and reduction showing the reactions very clearly. A chapter is devoted to the definition of metallurgical terms, refractory materials and fuel, another to the ores and alloys of iron, and then a description of the various processes employed in the metallurgy of iron and steel is given, attention being pretty equally divided between the two metals.

The most ancient and most difficult method of extracting iron from the ore is what is known as the direct method, and the author explains clearly the two causes of its failure, whether in the case of the old Catalan or any of the modern processes, and the reason why the blast furnace, although an indirect, has proved so successful a method. These two causes are "the easy oxidation of iron by carbonic acid and water, at the temperature at which ferrous oxide is reduced to the metallic state by carbon, carbonic oxide, or hydrogen, and the facility with which iron at a red heat combines with carbon."

The preparation of the ores for reduction in the blast furnace and their treatment therein are next brought forward, the advantages and disadvantages of the hot blast, the utilization of waste gases, the dimensions and form of blast furnace and subsidiary subjects being treated of.

The metal being now in the state of pig-iron, the means of refining and puddling are described; the various arrangements are set forth by which attempts have been made to effect the work of the puddler by mechanical means, whether by automatic rabbles or rotatory furnaces, and their relative advantages and disadvantages. A chapter is devoted to the treatment of puddled iron under the hammer and in the rolling mill, and to the tinning and galvanizing of iron.

Leaving the subject of malleable iron, the author next considers the question of iron-founding. He describes the cupola furnace in which the pig metal is fused; and the various methods of moulding and casting, and the brands of pig-iron used for different purposes, are treated of.

About a third of the book is devoted to the consideration of steel; it is in this branch of the treatment of iron that the greatest development has occurred of late years, and the book under review treats of all the modern practice. It is pleasant to find, too, in the preparation of an elementary work, that constructive perspective has been employed. Modern processes are not brought into prominence simply because they are modern, and ancient methods are not thrown into the shade if still employed. Amongst the latter we find full attention given to the cementation process, and crucible steel; whilst a chapter is devoted to each of the processes of Bessemer and Siemens. The book finishes with a chapter on steel-casting and on testing.

The volume before us is intended to assist pupils preparing for the ordinary grade examinations of the City and Guilds of London Institute, and its author—the principal of the School of Metallurgy in connection with

the Birmingham and Midland Institute—is to be congratulated on the good work he has done in this connection. The book is illustrated with 72 figures, which agree with the simplicity and clearness of the diction, and questions are found at the end of each chapter, which have been well prepared to test the learner's apprehension of its contents. We are pleased to be able to recommend this little work, as a foundation for the study of the metallurgy of iron and steel.

OUR BOOK SHELF.

On the Creation and Physical Structure of the Earth. By J. T. Harrison, F.G.S., M.Inst.C.E. (London: Longmans, 1889.)

THIS book brings to mind one of the most winning of the vagaries of childhood. A bright child of an inquiring turn will sometimes sit with comical sedateness listening to the talk of its elders. It may afterwards be overheard repeating to one of its playmates, or to some lucky adult who has the knack of winning its confidence, such detached scraps of the conversation as have found a resting-place in its little brain; and, conscious even at its early age of the necessity of some continuity in a narrative, filling up the gaps with inventions or criticisms of its own, charming every way, but mainly on account of their utter want of connection with the subject of the conversation which it is attempting to report. So our author has listened to the teaching of many geologists, and has culled many detached passages from their writings: these he repeats to the world in a book, printing between them comments and lucubrations of his own, about as innocent and as little apposite as the child's prattle—hardly so amusing, however. The following passage is a fair sample of the writer's own share in the book. "The termination of the Secondary Period, which introduced these altered conditions of the surface of the northern hemisphere, was really the commencement of what is called the Glacial epoch in Europe. We have noted signs of glaciation during the deposition of the upper chalk in India and North America, but now the conditions which induced that glaciation are extended in such a manner as to unite these districts, and produce that enormous accumulation of snow and ice at the North Pole, the weight of which in the Miocene epoch depressed the crust in that region and upheaved the mighty mountain ranges to which I have just referred."

The book bristles with cataclysms and catastrophes. The shifting of a thin crust on an internal nucleus which it does not fit, and incessant protrusions of granite, are invoked to account for phenomena which every-day people still persist in thinking are satisfactorily explained by every-day causes. But the author is one born out of due time—two centuries too late. How he and Burnet would have enjoyed a crack together! But there is this to be said, the "Sacred Theory of the Earth" is Burnet's own: the staple of the present work consists of extracts from the works of others. The mottoes are verses from the first chapter of Genesis, but their relevancy to the subject-matter of the chapters which they head is not obvious. A. H. G.

Through Atolls and Islands in the Great South Sea. By F. J. Moss. (London: Sampson Low, 1889.)

MR. MOSS—a member of the House of Representatives, New Zealand—started from Auckland, in September 1886, in the schooner *Buster*, for a voyage among the islands and islets of "the outer lagoon world." He was absent seven months, and during that period he crossed the equator six times, and visited more than forty islands among the least frequented groups. In the present volume he sums up the impressions produced upon him by what he saw and heard in the course of his voyage. Mr. Moss, in dealing with matters which really interest him, shows that he is an accurate observer and a man of sound judgment. His style, although plain and unpretending, is well fitted for the task he has fulfilled. The best parts of the book are those in which he tries to convey some idea of the daily life led by those natives whose customs he had an opportunity of studying. He appreciates warmly some aspects of the various Polynesian types of character, but thinks that the people are likely to degenerate rapidly, unless they can be provided with a better class of native teachers than most of those to whom the duty of guiding them is now intrusted. What is needed, he thinks, is, that the islanders shall have in their work and in their amusements freer scope for the imaginative powers with which they are endowed, and the exercise of which is too often foolishly discouraged. Everything Mr. Moss has to say on this subject deserves the serious consideration of those to whom his warnings and counsels are either directly or indirectly addressed.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Who Discovered the Teeth in Ornithorhynchus?

IN NATURE of November 14 (p. 31), Profs. Flower and Latter criticise my note which appeared the week previous (November 7, p. 11), concerning the discovery of teeth in the young *Ornithorhynchus*. They promptly dismiss my claim that Sir Everard Home discovered the teeth of the young *Ornithorhynchus*, by stating that the structures described and figured by Sir Everard are the well-known cornules of the adult animal.

If they will take the trouble to turn to the plate cited by me—namely, Plate lix. of the second volume of Home's "Lectures," 1814—and will read the accompanying explanation, they will see that Home was familiar with the teeth of both the young and the old animal.

For the benefit of those who may not have access to Home's "Lectures," I here reproduce outline tracings of two of his figures. Plate lix. Fig. 2, shows the teeth of the *young Ornithorhynchus*—the "first set," as Home says, "to show that there are two grinding teeth on each side." The next figure is a similar tracing from the succeeding plate in Home's "Lectures" (Plate lx.), which represents, to again use Home's words, "the under jaw of the full-grown *Ornithorhynchus paradoxus*, to show that there is only one grinder on each side." Both of these figures are natural size.

In the face of these *facts*, further comment seems unnecessary. I admit, of course, that Home did not discover the chemical composition of the teeth of the young animal—this was Poulton's discovery. C. HART MERRIAM.

Washington, D.C., November 30.

[We do not reproduce the outlines sent, as anyone interested in the subject may see the originals, not only in Home's "Comparative Anatomy," but in the Philosophical Transactions, where they first appeared.—ED. NATURE.]

I SHOULD be very sorry to deny the credit of any discovery to Sir Everard Home, or anyone else, if any evidence could be shown of its having been made. Of the figures cited by Dr. Hart Merriam, that of the younger animal seems (as far as can be judged from the roughly executed engraving, with the assistance of the descriptive text) to represent the horny plates, showing the hollows from which the true teeth have recently fallen; that of the old specimen, the same plates after they are fully grown, and their surfaces worn down by attrition. This difference led Home to conjecture that these plates were changed during the growth of the animal—a view which was corrected by Owen ("Comp. Anat. of Vertebrates," vol. iii. p. 272), by the statement

that "each division or tubercle of the [horny] molar is separately developed, and they become confluent in the course of growth." By the way, no one can have been better acquainted with the work of Home than his successor in the Hunterian Chair, Sir Richard Owen; and yet, in his numerous references to this subject (Art. "Monotremata," "Cyclop. Anat. and Physiology"; "Odontography"; "Comp. Anat. of Vertebrates," &c.), no trace is shown of any knowledge of a discovery which could not have failed to have interested him, if it had been made before his time.

If a cursory perusal of Sir Everard Home's first account of the mouth of the Ornithorhynchus (in the Philosophical Transactions for 1800), or any interpretation placed upon his figures, might lead anyone to infer, with Dr. Merriam, that the real teeth of the young animal had been discovered at that time, the best possible authority may be conclusively cited against such an idea, no other than that of Home himself, who, in his later description of the same specimen ("Lectures on Comparative Anatomy," 1814), describes the organs in question as "the first set of *cuticular teeth*"—an expression quite incompatible with their being the teeth described by Mr. Poulton and Mr. Oldfield Thomas. It really seems superfluous to have to remind a zoologist of such high repute as Dr. Hart Merriam that the difference between teeth with the structure and mode of growth which characterize these organs in the Mammalia generally, and the horny epithelial plates of Ornithorhynchus, is not merely one of "chemical composition." W. H. FLOWER.

The Pigment of the Touraco and the Tree Porcupine.

ATTENTION has been lately again directed to the red pigment in the wing feathers of the touraco, which has been stated by several observers to be soluble in pure water. Prof. Church, who was the first to experiment upon this pigment (*The Student*, vol. i., 1868; *Phil. Trans.*, 1869), quotes Mr. Tegetmeier and others, to the effect that this pigment can be washed out of the feathers by water. Later, M. Verreaux (Proc. Zool. Soc., 1871) confirmed these statements from his own experiments while travelling in South Africa; attempting to catch one of these birds whose feathers were sodden with rain, he found that the colour stained his hands "blood-red." A few years ago Prof. Krukenberg ("Vergl. Phys. Studien") took up the study of turacin—as Prof. Church termed the pigment—and added some details of importance to Prof. Church's account; Krukenberg, however, contradicted certain of the statements quoted by Church with reference to the solubility of turacin in pure water, remarking that the pigment in the dead bird is insoluble in water. A writer in the *Standard* of October 17 is able "partially to confirm" the assertion that turacin is soluble in pure water. Seeing that there is some conflict of opinion with regard to this matter, I think it worth while to state that I found it quite easy to extract with tap water (warm) some of the pigment from a spirit-preserved specimen of the bird; only a very small amount could be extracted in this way, and the feathers were not perceptibly decolorized even after remaining in the water for a fortnight. I also experimented upon a feather just shed from one of the specimens now in the Zoological Society's Gardens; this was steeped in water for some time without any effect being visible, but after a period of two days the water became stained a very faint pink.

The touraco, however, is not a unique instance of a terrestrial animal with an external colouring matter soluble in water. I am not aware whether other cases have been recorded, but I find a pigment of a similar kind in a South American tree porcupine (*Sphingurus villosus*).

This porcupine has bright yellow spines which are for the most part concealed by abundant long hair. The spines themselves are parti-coloured, the greater part being tinged with a vivid yellow; the tip is blackish-brown. I was unable to extract this pigment with chloroform, or with absolute alcohol even when heated; like so many other colouring substances which are insoluble in these fluids, the pigment could be extracted by potash or ammonia; I found also that tap water, warm or cold, dissolved out the yellow colour; the action was slower than when the water was first rendered alkaline by the addition of ammonia, but, unlike the touraco, the pigment was nearly, if not quite, as completely dissolved. The skin, from which the spines were taken, was a dried skin of an animal recently living in the Zoological Society's Gardens; it had not been preserved in alcohol or treated in any way which might lead to the supposition that the pigment was chemically altered. There is, therefore, a considerable probability that in the living animal the pigment is also soluble in water. I believe that this yellow pigment is undescribed, but I have not yet completed my study of it; in any case, it is not zoofulvin or picifulvin, or any "lipochrome." FRANK E. BEDDARD.

Exact Thermometry.

IN the account which Prof. Mills has given (NATURE, December 5, p. 100) of M. Guillaume's "Traité pratique de la Thermométrie de précision," the permanent ascent of the zero-point of a mercurial thermometer, after prolonged heating to a high temperature, is stated to be due to compression of the bulb—rendered more plastic by the high temperature—by the external atmospheric pressure.

The constant slow rise of the zero-point of a thermometer at the ordinary temperature is mentioned by Prof. Mills; and the late Dr. Joule's observation of this change in a thermometer during twenty-seven years is specially alluded to. It may, I imagine, be taken for granted that after the lapse of a sufficient length of time—possibly many centuries—a final state of equilibrium would be attained; and it has always appeared to me that the effect of heating the thermometer to a high temperature is simply to increase the rate at which this final state is approached. It is my impression that, owing to the more rapid cooling of the outer parts of the bulb after it has been blown, the inner parts are in a state of tension, as, to a very exaggerated degree, in the Prince Rupert's drops; and that it is the gradual equalization of the tension throughout the glass that causes the contraction; in other words, that the process is one of slow annealing.

This explanation appears to be supported by the facts—(1) that when a thermometer is exposed for a long time to a high temperature, the zero-point rises rapidly at first, then more and more slowly, and finally becomes constant or nearly so; (2) that the higher the temperature the more rapidly is this state of equilibrium attained. I do not know of any experimental evidence that the rate of ascent is influenced by changes of external pressure, and it seemed to be desirable to test the point.

In order to do this I have exposed three thermometers, A, B, and C, constructed by the same maker and of the same kind of glass, to a temperature of about 280° for several days in the same vapour-bath, under the following conditions:—The thermometers were all placed in glass tubes closed at the bottom (C being suspended from above), and the tubes were heated by the vapour of boiling bromonaphthalene. One of the tubes—that containing thermometer C—was exhausted so as to reduce the external pressure on the bulb to zero; the others were open to the air. In thermometer A there was a vacuum over the mercury, but air was admitted into B and C to increase the internal pressure. Consequently, the bulb of A was exposed to a resultant external pressure equal to the difference between the barometric pressure and that of the column of mercury in the stem of the thermometer; the internal and external pressures on the bulb of B were approximately equal; lastly, the internal pressure on the bulb of C was the sum of the pressures of the column of mercury in the stem and of the air above it, while the external pressure was zero.

The following results were obtained:—

	A.	Rise.	B.	Rise.	C.	Rise.
Zero before heating	0·15		0·10		−0·10	
		0·35		0·25		0·40
After 2 hours' heating ...	0·50		0·35		0·30	
		0·80		0·75		0·80
After an additional 5½ hours' heating	1·30		1·10		1·10	
Total rise of zero-point...		1·15		1·00		1·20

The thermometers were heated until 5 p.m. each day, and the zero-points read on the following morning.

If the diminution of volume of the thermometer bulb, usually observed, were due to external pressure, the zero-point of A should have risen, that of B should have remained nearly stationary, while that of C should have fallen. Instead of this, however, the zero-points of all three thermometers rose at nearly the same rate; therefore the yielding of the bulbs to pressure, owing to the plasticity of the glass, if it occurred at all, had no sensible effect on the result. SYDNEY YOUNG.

University College, Bristol, December 12.

Locusts in the Red Sea.

A GREAT flight of locusts passed over the s.s. *Golconda* on November 25, 1889, when she was off the Great Hanish Islands in the Red Sea, in lat. 13°·56 N., and long. 42°·30 E.

The particulars of the flight may be worthy of record.

It was first seen crossing the sun's disk at about 11 a.m. as a dense white flocculent mass, travelling towards the north-east at about the rate of twelve miles an hour. It was observed at noon by the officer on watch as passing the sun in the same state of density and with equal speed, and so continued till after 2 p.m.

The flight took place at so high an altitude that it was only visible when the locusts were between the eye of the observer and the sun; but the flight must have continued a long time after 2 p.m., as numerous stragglers fell on board the ship as late as 6 p.m.

The course of flight was across the bow of the ship, which at the time was directed about 17° west of north, and the flight was evidently directed from the African to the Arabian shore of the Red Sea.

The steamship was travelling at the rate of thirteen miles an hour, and, supposing the host of insects to have taken only four hours in passing, it must have been about 2000 square miles in extent.

Some of us on board amused ourselves with the calculation that, if the length and breadth of the swarm were forty-eight miles, its thickness half a mile, its density 144 locusts to a cubic foot, and the weight of each locust $\frac{1}{16}$ of an ounce, then it would have covered an area of 2304 square miles; the number of insects would have been 24,420 billions; the weight of the mass 42,580 millions of tons; and our good ship of 6000 tons burden would have had to make 7,000,000 voyages to carry this great host of locusts, even if packed together 111 times more closely than they were flying.

Mr. J. Wilson, the chief officer of the *Golconda*, permits me to say that he quite agrees with me in the statement of the facts given above. He also states that on the following morning another flight was seen going in the same north-easterly direction from 4.15 a.m. to 5 a.m. It was apparently a stronger brood and more closely packed, and appeared like a heavy black cloud on the horizon.

The locusts were of a red colour, were about $2\frac{1}{2}$ inches long, and $\frac{1}{16}$ of an ounce in weight. G. T. CARRUTHERS.

A Marine Millipede.

IT may interest "D. W. T." (NATURE, December 5, p. 104) to know that *Geophilus maritimus* is found under stones and sea-weeds on the shore at or near Plymouth, and recorded in my "Fauna of Devon," Section "Myriopoda," &c., 1874, published in the Transactions of the Devonshire Association for the Advancement of Literature, Science, and Art, 1874. This species was not known to Mr. Newport when his monograph was written (Linn. Trans., vol. xix., 1845). Dr. Leach has given a very good figure of this species in the *Zoological Miscellany*, vol. iii. pl. 140, Figs. 1 and 2, and says: "Habitat in Britannia inter scopulos ad littora maris vulgatissime." But, so far as my observations go, I should say it is a rare species. See *Zoologist*, 1866, p. 7, for further observations on this animal. EDWARD PARFITT.

Exeter, December 9, 1889.

Proof of the Parallelogram of Forces.

THE objection to Duchayla's proof of the "parallelogram of forces" is, I suppose, admitted by all mathematicians. To base the fundamental principle of the equilibrium of a *particle* on the "transmissibility of force," and thus to introduce the conception of a *rigid body*, is certainly the reverse of logical procedure. The substitute for this proof which finds most favour with modern writers is, of course, that depending on the "parallelogram of accelerations." But this is open to almost as serious objections as the other. For it introduces kinetic ideas which are really nowhere again used in statics. I should therefore propose the following proof, which depends on very elementary geometrical propositions. The general order of argument resembles that of Laplace.

I adopt the "triangular" instead of the "parallelogrammic" form. Thus, if PQ, QR represent in length and direction any directed magnitudes whatever, and, *if these have a single equivalent*, that single equivalent will be represented by PR.

To prove that the equivalent of PQ, QR is PR.

(1) The equivalent of two perpendicular lengths is equal *in length* to their hypothenuse.

For, draw AD perpendicular to hypothenuse BC.

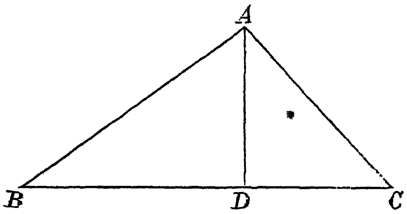


FIG. 1.

Then, let BD, DA = k . BA, making angle θ with BA towards BD.

Then, by similar triangles, AD, DC = k . AC, making angle θ with AC towards AD.

But these equivalents are at right angles, and proportional to BA and AC. Hence, their equivalent, by similar triangles, is k^2 . BC along BC.

But BD, DA, AD, DC = BC. $\therefore k^2 = 1$; $\therefore k = 1$.

(2) If theorem holds for right-angled triangle containing angle θ, it holds for right-angled triangle containing $\frac{1}{2}\theta$.

For, let ACD = θ, where D is 90°. Produce DC to B, such that CB = CA. Then ABD = $\frac{1}{2}\theta$.

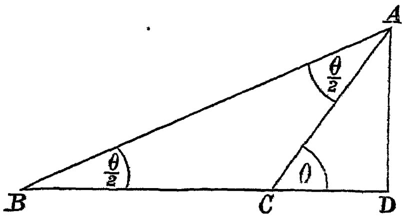


FIG. 2.

Then assume CD, DA = CA. Add BC. $\therefore$ BD, DA = BC, CA.

But BD, DA = BA in magnitude by (1); and BC, CA has its equivalent *along* BA, $\because$ BC = CA. $\therefore$ BD, DA = BA, both in magnitude and direction.

(3) If the theorem holds for θ and ϕ, it holds for $\theta + \phi$.

For make the well-known projection construction. Thus—

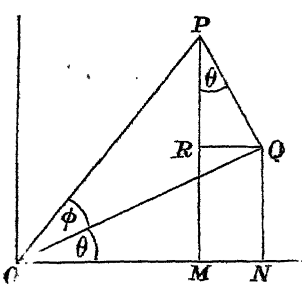


FIG. 3.

OP = OQ, QP = ON, NQ, QR, RP = OM, MP.

(4) Finally, by (1), theorem holds for isosceles right-angled triangle; $\therefore$ by (2) it holds for right-angled triangle containing angle $90° \div 2^n$; $\therefore$ by (3) it holds for right-angled triangle containing angle $m.\ 90° \div 2^n$: *i.e.* for *any* angle (as may be shown, if considered necessary, by the method for incommensurables in Duchayla's proof).

Hence, if AD be perpendicular on BC in *any* triangle,

BA, AC = BD, DA, AD, AC = BC. Q.E.D.

W. E. JOHNSON.

Llandaff House, Cambridge, November 12.

Glories.

Mr. James McConnel asks in Nature (vol. xl. p. 594) for acccounts of the colours and angular dimensions of glories. I saw a good instance of the phenomenon on Lake Superior, June 17, 1888, and, having had my attention called to the value of accurate descriptions in such cases by Mr. Henry Sharpe's "Brocken Spectres," I examined it carefully.

The shadow of my head on the mist was surrounded by a brilliant halo or glory, slaty-white around the head, followed by orange and red; then a circle of blue, green, and red, and the same colours repeated more faintly. The diameter of the innermost and brightest circle of red, as measured on the graduated semicircle of a clinometer, was $4\frac{1}{2}°$. There was also a very distinct, but nearly white, fog-bow outside, of 42° radius, as measured in the same way.

A. P. Coleman.

Faraday Hall, Victoria University, Cobourg, Ontario.

Fossil Rhizocarps.

Referring to Sir William Dawson's note on this subject in Nature of November 7 (p. 10), we regret that we have been unable to trace the original source from which the statement in our "Hand-book of Cryptogamic Botany" was derived, relative to the fructification of *Protosalvinia* or *Sporangites*. The sentence will therefore, with apologies to Sir W. Dawson, be removed from future editions of the work.

Alfred W. Bennett.

The Arc-Light.

Would you or any of your readers kindly tell me where I may find an account of any of the latest methods of determining the back E.M.F. of the arc-light?

Joseph McGrath.

Mount Sidney, Wellington Place, Dublin.

THE HYDERABAD CHLOROFORM COMMISSION.

The appointment of a Commission at the present time to investigate the action of chloroform as an anæsthetic might to many seem an anomaly. For the use of chloroform as an anæsthetic was introduced over forty years ago: it was in November, 1847, that Prof. Simpson, of Edinburgh, first brought this valuable agent before the medical profession. Since that time, the use of chloroform has enormously extended, especially in our country, and although there are other valuable agents of the same class—such as ether and nitrous-oxide gas—yet there is a universality of opinion that the employment of chloroform has in many cases a special advantage. Considering the extensive use of the agent, and the progress which has been made of late years in the study of the action of drugs in man, it certainly is surprising that the knowledge of the effect of chloroform on the different parts and organs of the body is not complete. This is not altogether from want of attention to the subject; because, previous to the Hyderabad Commission, at least two Commissions were appointed with the view of investigating the action of chloroform and its occasional serious effects. These Commissions were appointed by the Royal Medical and Chirurgical Society of London, and by the British Medical Association, and they were composed of men who, from their knowledge of experiment and acquaintance with practical medicine, were competent to discuss the question. The two Commissions arrived at the same conclusions as the distinguished French man of science, Claude Bernard, had published years before, and these conclusions tallied with the teaching of the great London medical schools.

Chloroform and other anæsthetic agents have a peculiar position: they are powerful drugs used, not for disease itself, but for the purpose of allowing an operation to be performed, preventing the pain which would otherwise be felt, and relaxing the contraction and spasms of the muscles, so that the surgeon can more readily and accurately operate. The administration of the anæsthetic is something, then, outside the diseased condition; so that its use ought theoretically to be perfectly harmless to the sick person. Unfortunately it is not always so, and deaths from chloroform are, although rare, by no means unknown. The administrator of chloroform is therefore a person of great responsibility: he has to watch carefully the effect of the agent on the patient, to notice any unfavourable change that occurs, and to adopt measures to counteract any bad effects which appear. The knowledge of the mode in which chloroform causes danger to the life of the patient is therefore of vast importance; for, if the administrator knows the signs of danger, there is more likelihood of counteracting a fatal result. These fatal results, which are among the saddest that occur in medical practice, ought, if possible, to be avoided.

What, then, is the danger to life of chloroform? Or, to speak more fully, what particular part of the body does chloroform injuriously affect when there is danger? This is just the point that the various Commissions have attempted to settle. In the Scotch schools, more especially that of Edinburgh, it has been taught that the great danger of chloroform was in failure of respiration; meaning by this that the danger-signal of chloroform was the stoppage or irregularity of the breathing. As a corollary to this belief, it was considered that the heart was only affected after the breathing had become interfered with; that, in fact, the respiration stopping, the blood was not oxygenated, so the heart stopped beating. This was the teaching of the great Edinburgh surgeon, Syme. The English (and especially the London) teaching was almost directly opposed to this. It was taught, and is still taught in the London schools, that the great danger from chloroform arose from its effect on the heart, which stopped beating before the respiration ceased. Which, then, of these two doctrines is true, or are both true?

The decision of this question is, as we have stated, one of vast importance; but it must be remembered that, whichever is right, the administrator of anæsthetics always pays attention to both the beating of the heart and the regularity of the respiration. Surgeon-Major Lawrie, one of the prominent members of the Hyderabad Chloroform Commission, says that "it is possible to avert all risk to the heart by devoting the entire attention to the respiration during chloroform administration." Medical opinion in England, both of that of experts (professional anæsthetists) and of the general profession, is distinctly opposed to this view; and the administrator who does not attend to the pulse, as well as to the breathing, is certainly neglecting one of the main paths by which Nature shows us what is going on inside the organism.

From the statement of Surgeon-Major Lawrie just quoted, it will be seen that the Hyderabad Chloroform Commission came to the conclusion that the danger from the administration arose, not from the heart, but from the respiration. This view was strongly combated in our contemporary, the *Lancet*. The importance of the question led the Nizam of Hyderabad to obtain the services of a scientific medical man from England to go out to India and attempt to settle the question. Dr. Lauder Brunton, F.R.S., consented to go; and, well known as he is for his life-long devotion to the experimental investigation of the action of remedies and their practical application, it was considered probable that his aid in the research would lead to interesting and important results. From the somewhat scanty news of the results which have been telegraphed to England, it seems likely that the investigation now progressing at Hyderabad will tend to revolutionize existing views as to the action of chloroform.

Dr. Brunton's views as regards the dangers of chloroform before he left England were clearly expressed in his well-known "Text-book of Pharmacology." In it he says that "the dangers resulting from the employment of

chloroform are death by stoppage of respiration and death by stoppage of the heart;" he lays as much stress on the effect on the heart as on the respiration, and he proceeds to affirm that too strong chloroform vapour may very quickly paralyze the heart. This view is, indeed, similar to the one we have already mentioned as taught in the London schools of medicine. It is also well known that death may occur soon after chloroform has begun to be administered, from the heart being affected. If the operation is begun too soon, fainting from pain may supervene, and a fatal result occur: this has always been strongly insisted upon by Dr. Brunton. Surgeon-Major Lawrie says that in such cases it is not the chloroform that acts on the heart, but simply that there is fatal syncope or fainting.

From the large number of experiments on animals which Dr. Brunton has performed in India, in conjunction with the Hyderabad Commission and a medical delegate of the Indian Government, it appears that the "danger from chloroform is asphyxia or an overdose;" there is none whatever from the heart direct. This statement is a distinct reversal of the view generally held in England. It means that chloroform causes a fatal result by affecting the respiration or by too much being taken into the system and affecting the brain; and that there is no direct paralysis of the heart from the chloroform. A perfectly impartial opinion cannot, however, be formed from the scanty records of the investigation which have been as yet received in England. We must wait for fuller details of the experiments before a final judgment can be passed.

It is well, however, to point out that the prevailing view in England has been founded, not only on experiments on the lower animals, but also on the extended clinical observation of two generations of medical men. Clinical observation is not so accurate or so lucid as that of direct experiment, but it has its value, and one by no means to be despised in a case where it is so extensive, and directed to a subject of such great importance, not only to the medical profession, but to the general public, as the question of the administration of chloroform.

ON THE CAVENDISH EXPERIMENT.

IN the last number of the Proceedings of the Royal Society (vol. xlvi. p. 253), I have given an account of the improvements that I have made in the apparatus of Cavendish for measuring the constant of gravitation. As the principles and some of the details there set out apply very generally to other experiments where extremely minute forces have to be measured, it is possible that an abstract of this paper may be of sufficient interest to find a place in the columns of NATURE.

In the original experiment of Cavendish (Phil. Trans., 1798, p. 469), as is well known, a pair of small masses, *m m* (Fig. 1), carried at the two ends of a very long but light torsion rod, are attracted towards a pair of large masses, M M, thus deflecting the arm until the torsion of the suspending wire gives rise to a moment equal to that due to the attraction. The large masses are then placed on the other side of the small ones, as shown by the dotted circles, and the new position of rest of the torsion arm is determined. Half the angle between the two positions of rest is the deflection produced by the attracting masses. The actual force which must be applied to the balls to produce this deflection, can be directly determined in dynamical units when the period of oscillation and the dimensions and masses of the moving parts are known. In the original experiment of Cavendish, the arm is 6 feet long, the little masses are balls of lead 2 inches in diameter, and large ones are lead balls 1 foot in diameter. Since the attraction of the whole earth on the smaller balls only produces their weight, *i.e.* the force with which they are attracted downwards, it is evident that the balls, M M, which are insignificant in comparison with the size of the earth, can only exert an extremely feeble attraction. So small is this that it can only be detected when the beam is entirely inclosed in a case to protect it from draughts; when, further, the whole apparatus is placed in a room into which no one must enter, because the heat of the body would warm the case unevenly, and so set up air currents which would have far more influence than the whole attraction to be measured; and when, finally, the period of oscillation is made very great, as, for instance, five to fifteen minutes. In order to realize how small must be the force that will only just produce an observable displacement of the balls, *m m*, it is sufficient to remember that the force which brings them back to their position of rest is the same as the corresponding force in the case of a pendulum which swings at the same rate. Now a pendulum that would swing backwards and forwards in five minutes would have to be about 20,000 metres long, so that in this case a deflection of one millimetre would be produced by a force equal to 1/20,000,000 of the weight of the bob. In the case of a pendulum swinging backwards and forwards once in fifteen minutes the corresponding force would be nine times as small, or 1/180,000,000 of the weight.

In spite of the very small value of the constant of gravitation, Cavendish was able, by making the apparatus on this enormous scale, to obtain a couple which

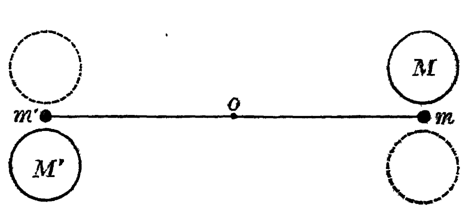


FIG. 1.

would produce a definite deflection against the torsion of his suspending wire.

These measures were repeated by Reich (*Comptes rendus*, 1837, p. 697), and then by Baily (*Phil. Mag.*, 1842, vol. xxi. p. 111), who did not in any important particular improve upon the apparatus of Cavendish, except in the use of a mirror for observing the movements of the beam.

Cornu and Baille (*Comptes rendus*, vol. lxxvi. p. 954, vol. lxxxvi. pp. 571, 699, 1001) have modified the apparatus with satisfactory results. In the first place they have reduced the dimensions of all the parts to about one-quarter of the original amount. Their beam, an aluminium tube, is only ½ metre long, and it carries at its ends masses of ¼ pound each, instead of about 2 pounds, as used by Cavendish. This reduction of the dimensions to about one-quarter of those used previously is considered by them to be one of the advantages of their apparatus, because, as they say, in apparatus geometrically similar, if the period of oscillation is unchanged, the sensibility is independent of the mass of the suspended balls, and *is inversely as the linear dimensions.* I do not quite follow this, because, as I shall show, if all the dimensions are increased or diminished together, the sensibility will be unchanged. If only the length of the beam is altered and the positions of the large attracting masses, so that they remain opposite to, and the same distance from, the ends of the beam, then the sensibility is inversely as the length. This mistake—for mistake it surely is—is repeated in Jamin's "Cours de Physique," tome iv. ed. iv. p. 18, where, moreover, it is emphasized by being printed in italics.

The other improvements introduced by Cornu and

Baille are the use of mercury for the attracting masses which can be drawn from one pair of vessels to the other by the observer without his coming near the apparatus, the use of a metal case connected with the earth to prevent electrical disturbances, and the electrical registration of the movements of the index on the scale, which they placed 560 centimetres from the mirror.

The great difficulty that has been met with has been the perpetual shifting of the position of rest, due partly to the imperfect elasticity or fatigue of the torsion wires, but chiefly, as Cavendish proved experimentally, to the enormous effects of air-currents set up by temperature differences in the box, which, with large apparatus, it is impossible to prevent. In every case the power of observing was in excess of the constancy of the effect actually produced. The observations of Cornu are the only ones which are comparable in accuracy with other physical measurements, and these, as far as the few figures given enable one to judge, show a very remarkable agreement between values obtained for the same quantity from time to time.

Soon after I had made quartz fibres, and found their value for producing a very small and constant torsion, I thought that it might be possible to apply them to the Cavendish apparatus with advantage. Prof. Tyndall, in a letter to a neighbour, expressed the conviction that it would be possible to make a much smaller apparatus in which the torsion should be produced by a quartz fibre. The result of an examination of the theory of the instrument shows that very small apparatus ought practically to work, but that in many particulars there is an advantage in departing from the arrangement which has always been employed, conclusions which experiment has fully confirmed.

As I have already stated, the sensibility of the apparatus is, if the period of oscillation is always the same, independent of its linear dimensions. Thus, if there are two instruments in which all the dimensions of one are n times the corresponding dimensions of the other, the moment of inertia of the beam and its appendages will be as $n^5 : 1$, and, therefore, the torsion also must be as $n^5 : 1$. The attracting masses, both fixed and movable, will be as $n^3 : 1$, and their distance apart as $n : 1$. Therefore, the attraction will be as n^6/n^2 or $n^4 : 1$, and this is acting on an arm n times as long in the large instrument as in the small; therefore the moment will be as $n^5 : 1$; that is, in the same proportion as the torsion, and so the angle of deflection is unchanged.

If, however, the length of the beam only is changed, and the attracting masses are moved until they are opposite to, and a fixed distance from, the ends of the beam, then the moment of inertia will be altered in the ratio $n^2 : 1$, while the corresponding moment will only change in the ratio of $n : 1$; and thus there is an advantage in reducing the length of the beam until one of two things happens: either it is difficult to find a sufficiently fine torsion thread that will safely carry the beam and produce the required period—and this, I believe, has up to the present time prevented the use of a beam less than $\frac{1}{2}$ metre in length—or else, when the length becomes nearly equal to the diameter of the attracting balls, they then act with such an increasing effect on the opposite suspended balls, so as to tend to deflect the beam in the opposite direction, that the balance of effect begins to fall short of that which would be due to the reduced length if the opposite ball did not interfere. Let this shortening process be continued until the line joining the centres of the masses M M makes an angle of 45° with the line $m\,m$; then, without further moving the masses M M, a still greater degree of sensibility can be obtained, provided the period remains unaltered, by reducing the length of the beam $m\,m$ to half its amount, so that the distance between the centres of M M is $2\sqrt{2}$ times the new length $m\,m$, at which point a maximum is reached.

It might be urged against this argument that a difficulty would arise in finding a torsion fibre that would give to a very short beam, loaded with balls that it will safely carry, a period as great as five or ten minutes, and until quartz fibres existed there would have been a difficulty in using a beam much less than a foot long, but it is now possible to hang one only half an inch long and weighing from twenty to thirty grains by a fibre not more than a foot in length, so as to have a period of five minutes. If the moment of inertia of the heaviest beam of a certain length that a fibre will safely carry is so small that the period is too rapid, then the defect can be remedied by reducing the weight, for then a finer fibre can be used, and since the torsion varies approximately as the square of the strength (not exactly, because fine fibres carry heavier weights in proportion), the torsion will be reduced in a higher ratio, and so by making the suspended parts light enough, any slowness that may be required may be provided.

Practically, it is not convenient to use fibres much less than one ten-thousandth of an inch in diameter, and these have a torsion 10,000 times less than that of ordinary spun glass. A fibre one five-thousandth of an inch in diameter will carry a little over thirty grains.

Since with such small apparatus as I am now using it is easy to provide attracting masses which are very large in proportion to the length of the beam, while with large apparatus comparatively small masses must be made use of owing to the impossibility of dealing with balls of lead of great size, it is clear that much greater deflections can be produced with small than with large apparatus. For instance, to get the same effect in the same time from an instrument with a 6-foot beam that I get from one in which the beam is five-eighths of an inch long, and the attracting balls are 2 inches in diameter, it would be necessary to provide and deal with a pair of balls each 25 feet in diameter, and weighing 730 tons instead of about $1\frac{3}{4}$ pound apiece. There is the further advantage in small apparatus that if for any reason the greatest possible effect is desired, attracting balls of gold would not be entirely unattainable, while such small masses as two piles of sovereigns could be used where qualitative effects only were to be shown. Owing to its strongly magnetic qualities, platinum is unsuited for experiments of this kind.

By far the greatest advantage that is met with in small apparatus is the perfect uniformity of temperature which is easily obtained, whereas, with apparatus of large size, this alone makes really accurate work next to impossible. The construction to which this inquiry has led me, and which will be described later, is especially suitable for maintaining a uniform temperature in that part of the instrument in which the beam and mirror are suspended.

With such small beams as I am now using it is much more convenient to replace the long thin box generally employed to protect the beam from disturbance by a vertical tube of circular section, in which the beam with its mirror can revolve freely. This has the further advantage that, if the beam is hung centrally, the attraction of the tube produces no effect, and the troublesome and approximate calculations which have been necessary to find the effect of the box are no longer required. The attracting weights, which must be outside the tube, must be made to take alternately positions on the two sides of the beam, so as to deflect it first in one direction and then in the other. For this purpose they are most conveniently fastened to the inside of a larger metal tube, which can be made to revolve on an axis coincident with the axis of the smaller tube. There are obviously two planes, one containing and one at right angles to the beam, in which the centres of the attracting balls will lie when they produce no deflection. At some intermediate position the deflection will be a maximum. Now, it is a matter of some importance to choose this maximum

position for the attracting masses, because, in showing the experiment to an audience, the largest effect should be obtained that the instrument is capable of producing; while in exact measures of the constant of gravitation this position has the further advantage that the only measurement which there is any difficulty in making, viz. the angle between the line joining the large masses and the line joining the small, which may be called the azimuth of the instrument, becomes of little consequence under these circumstances. In the ordinary arrangement the slightest uncertainty in this angle will produce a relatively large uncertainty in the result. I have already stated that if an angle of 45° is chosen, the distance between the centres of the large balls should be $2\sqrt{2}$ times the length of the beam, and the converse of course is true. As it would not be possible at this distance to employ attracting balls with a diameter much more than one and a half times the length of the beam, and as balls much larger than this are just as easily made and used, I have found by calculation what are the best positions when the centres of the attracting balls are any distance apart.

If the effect on the nearer ball only is considered, then it is easy to find the best position for any distance of the attracting mass from the axis of motion. Let P (Fig. 2) be the centre of the attracting ball, N that of the nearer

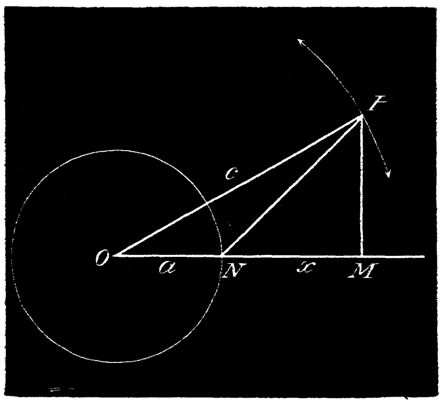


FIG. 2.

attracted ball, O the axis of motion, c and a the distances of P and N from O, and x the distance from N of the foot of the perpendicular from P on ON produced. Then the moment of N about O will be greatest when

$$x^2 + \frac{3a^2 + c^2}{a} x = 2(c^2 - a^2),$$

or what comes to the same thing when

$$\cos^2 \theta + \frac{c^2 + a^2}{ca} \cos \theta = 3.$$

Now, as the size of the attracting masses M M is increased, or, as is then necessarily the case, as the distance of their centres from the axis increases, their relative action on the small masses $m\,m$ at the opposite ends of the beam increases, and so but a small fraction of the advantage is obtained, which the large balls would give if they acted only upon the small balls on their own side. For instance, if the distance between the centres of M M is five times the length of the beam, the moment due to the attraction on the opposite small balls is nearly half as great as that on the near balls, so that the actual sensibility is only a little more than half that which would be obtained if the cross action could be prevented.

I have practically overcome this difficulty by arranging the two sides of the apparatus at different levels. Each large mass is at or near the same level as the neighbouring small one, but one pair is removed from the level of the other by about the diameter of the large masses which in the apparatus figured below is nearly five times as great as the distance *in plan* between the two small masses.

In order to realize more fully the effect of a variety of arrangements, I have, for my own satisfaction, calculated the values of the deflecting forces in an instrument in which the distance between the centres of the attracting balls is five times the length of the beam, for every azimuth and for differences of levels of 0, 1, 2, 3, 4, and 5 times the length of the beam.

The result of the calculation is illustrated by a series of curves in the original paper. The main result, however, is this.

In the particular case which I have chosen for the instrument, *i e.* where the distance between the centres of M M and the axis, and the difference of level between the two sides are both five times the length of the beam, as seen in plan, and where the diameter of the large masses is 6·4 times the length of the beam, the angle of deflection becomes 18·7 times as great as the corresponding angle in the apparatus of Cavendish, provided that the large masses are made of material of the same density in the two cases and the periods of oscillation are the same.

Having now found that with apparatus no bigger than an ordinary galvanometer it should be possible to make an instrument far more sensitive than the large apparatus in use heretofore, it is necessary to show that such a piece of apparatus will practically work, and that it is not liable to be disturbed by the causes which in large apparatus have been found to give so much trouble.

I have made two instruments, of which I shall only describe the second, as that is better than the first, both in design and in its behaviour.

The construction of this is made clear by Fig. 3. To a brass base provided with levelling screws is fixed the vertical brass tube t, which forms the chamber in which the small masses $a\,b$ are suspended by a quartz fibre from a pin at the upper end. These little masses are cylinders[1] of pure lead 11·3 millimetres long and 3 millimetres in diameter, and the vertical distance between their centres is 50·8 millimetres. They are held by light brass arms to a very light taper tube of glass, so that their axes are 6·5 millimetres from the axis of motion. The mirror m, which is 12·7 millimetres in diameter, plane, and of unusual accuracy, is fastened to the upper end of the glass tube by the smallest quantity of shellac varnish. Both the mirror and the plate-glass window which covers an opening in the tube were examined, and afterwards fixed with the refracting edge of each horizontal, so that the slight but very evident want of parallelism between their faces should not interfere with the definition of the divisions of the scale. The large masses M M are two cylinders[1] of lead 50·8 millimetres in diameter, and of the same length. They are fastened by screws to the inside of a brass tube, the outline of which is dotted in the figure, which rests on the turned shoulder of the base, so that it may be twisted without shake through any angle. Stops (not shown in the figure) are screwed to the base, so that the actual angle turned through shall be that which produces the maximum deflection. A brass lid made in two halves covers in the outer tube, and serves to maintain a very perfect uniformity of temperature in the inner tube. Neither the masses M M, nor the lid, touch the inner tube. The period of oscillation is 160 seconds.

With this apparatus placed in an ordinary room with

[1] Cylinders were employed instead of spheres, because they are more easily made and held, and because spheres have no advantage except when absolute calculations have to be made. Also the vertical distance $a\,b$ was for convenience made only about four times the length $a\,b$ in plan.

draughts of air of different temperatures and with a lamp and scale such as are used with a galvanometer, the effect of the attraction can easily be shown to a few, or, with a lime-light, to an audience. To obtain this result with apparatus of the ordinary construction and usual size is next to impossible, on account chiefly of the great disturbing effect of air currents set up by difference of temperature in the case. The extreme portability of the new instrument is a further advantage, as is evident when the enormous weight and size of the attracting masses in the ordinary apparatus are considered.

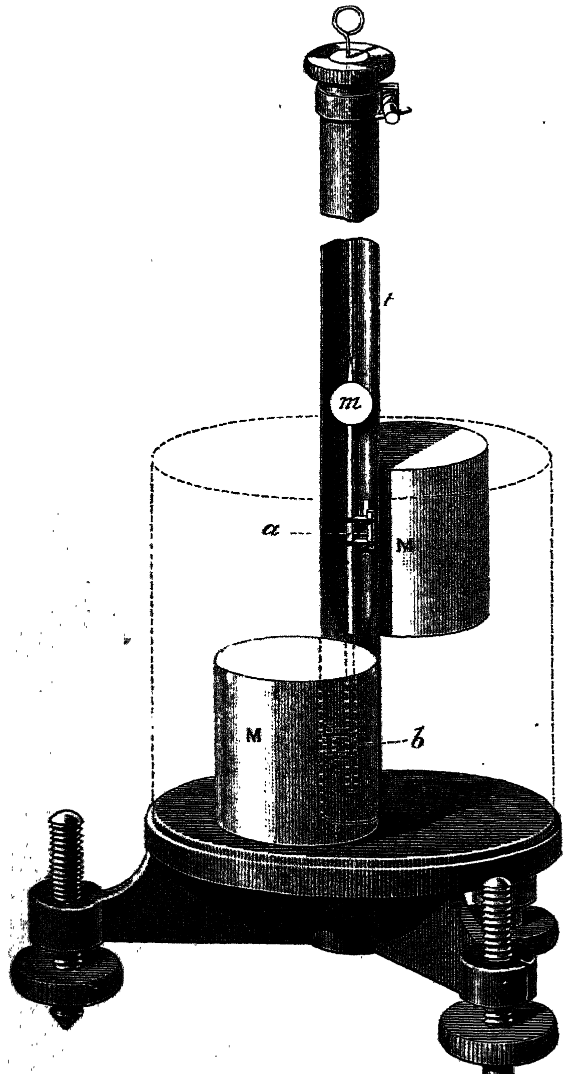


FIG. 3.

However, this result is only one of the objects of the present inquiry. The other object which I had in view was to find whether the small apparatus, besides being more sensitive than that hitherto employed, would also be more free from disturbances and so give more consistent results. With this object I have placed the apparatus in a long narrow vault under the private road between the South Kensington Museum and the Science Schools. This is not a good place for experiments of this kind, for when a cab passes overhead the trembling is so great that loose things visibly move; however, it is the only place at my disposal that is in any degree suitable. A large drain-pipe filled with gravel and cement and covered by a slab of stone forms a fairly good table. The scale is made by etching millimetre divisions on a strip of clear plate glass 80 centimetres long. This is secured at the other end of the vault at a distance of 1053·8 centimetres from the mirror of the instrument. A telescope 132 centimetres long with an object-glass 5·08 centimetres in diameter, rests on V's clamped to the wall, with its object-glass 360 centimetres from the mirror. Thus any disturbance that the observer might produce if nearer is avoided, and at the same time the field of view comprises 100 divisions. While the observer is sitting at the telescope he can, by pulling a string, move an albo-carbon light, mounted on a carriage, so as to illuminate any part of the scale that may happen to be in the field of the telescope. The white and steady flame forms a brilliant background on which the divisions appear in black. The accuracy of the mirror is such that the millimetre divisions are clearly defined, and the position of the cross-wire (a quartz fibre) can be read accurately to one-tenth of a division. This corresponds to a movement of the mirror of almost exactly one second of arc.

The mode of observation is as follows: When all is quiet with the large masses in one extreme position, the position of rest is observed and a mark placed on the scale. The masses are moved to one side for a time and then replaced, which sets up an oscillation. The reading of every elongation and the time of every transit of the mark are observed until the amplitude is reduced to 3 or 4 centimetres. The masses are then moved to the other extreme position and the elongations and transits observed again, and this is repeated as often as necessary.

On the evening of Saturday, May 18, six sets of readings were taken, but during the observations there was an almost continuous tramp of art students above, producing a perceptible tremor, besides which two vehicles passed, and coals were twice shovelled in the coal cellar, which is separated from the vault in which the observations were made by only a 4½-inch brick wall. The result of all this was a nearly perpetual tremor, which produced a rapid oscillation of the scale on the cross-wire, extending over a little more than 1 millimetre. This increased the difficulty of taking the readings, but to what extent it introduced error I shall not be able to tell until I can make observations in a proper place.

In spite of these disturbances, the agreement between the deflections deduced from the several sets of observations, and between the periods, is far greater than I had hoped to obtain, even under the most favourable conditions. In order to show how well the instrument behaved, I have copied from my note-book the whole series of figures of one set, which sufficiently explain themselves.

Elongation.	Amplitude.	Decrement.	True Position of Rest.	Time of Transit of 36·09.	Correction for Transit of True Position of Rest.	True Half Period.
				h. m. s.		
15·05	38·15	0·805	36·18	9 8 25·0	+ 0·08	80·2
53·20	30·72	0·808	36·20	9 45·5	− 0·18	80·2
22·48	24·80	0·807	36·21	11 5·3	+ 0·24	80·0
47·28	20·00	0·807	36·20	12 25·8	− 0·28	79·9
27·28	16·12	0·805	36·22	13 45·0	+ 0·41	80·1
43·40	12·98	0·806	36·21	15 6·0	− 0·47	80·1
30·42	10·46	0·802	36·22	16 25·0	+ 0·63	79·5
40·88	8·38	0·808	36·24	17 46·0	− 0·91	80·5
32·50	6·77	0·808	36·24	19 4·5	+ 1·13	79·8
39·27	5·47	0·814	36·26	20 27·0	− 1·58	80·5
33·80	4·45		36·26	21 44·0	+ 1·94	
38·25						
		0·8066				80·08

It will be noticed that the true position of rest is slightly rising in value, and this rise was found to continue at the rate of 0·36 centimetre an hour during the whole course of the experiment, and to be the same when the large masses were in the positive or negative position. The motion was perfectly uniform, and in no way interfered wlth the accuracy of the experiments. It was due, I believe, to the shellac fastening of the fibre, for I find that immediately after a fibre has been attached, this movement is very noticeable, but after a few days it almost entirely ceases; it is, moreover, chiefly evident when the fibre is loaded very heavily. At the time that the experiment was made the instrument had only been set up a few hours.

The mean decrement of three positive sets was 0·8011, and of three negative sets, 0·8035. The observed mean period of three positive sets was 79·98, and of three negative sets, 80·03 seconds, from both of which 0·20 must be deducted as the time correction for damping.

The deflections, in centimetres, obtained from the six sets of observations taken in groups of three, so as to take into account the effect of the slow change of the position of rest, were as follows:—

From sets 1, 2, and 3	...	17·66 ± 0·01
,, 2, 3, and 4	...	17·65 ± 0·02
,, 3, 4, and 5	...	17·65 ± 0·02
,, 4, 5, and 6	...	17·65 ± 0·02

An examination of these figures shows that the deflection is known with an accuracy of about one part in two thousand, while the period is known to the 4000th part of the whole. As a matter of fact, the discrepancies are not more than may be due to an uncertainty in some of the observations of ½ millimetre or less, a quantity which, under the circumstances, is hardly to be avoided.

The result of these experiments is complete and satisfactory. As a lecture experiment, the attraction between small masses can be easily and certainly shown, even though the resolved force causing motion is, as in the present instance, no more than the 1/200,000 of a dyne (less than 1/10,000,000 of the weight of a grain), and this is possible with the comparatively short half period of 80 seconds. Had it been necessary to make use of such half periods as three to fifteen minutes, which have been employed hitherto, then, even though a considerable deflection were produced, this could hardly be considered a lecture experiment. So perfectly does the instrument behave, that there can be no difficulty in making a fairly accurate measure of the attraction between a pair of No. 5, or, I believe, even of dust shot.

The very remarkable agreement between successive deflections and periods shows that an absolute measure made with apparatus designed for the purpose, but on the lines laid down above, is likely to lead to results of far greater accuracy than any that have been obtained. For instance, in the original experiment of Cavendish there seems to have been an irregularity in the position of rest of one-tenth of the deflection obtained, while the period showed discrepancies of five to fifteen seconds in seven minutes. The experiments of Baily, made in the most elaborate manner, were more consistent, but Cornu was the first to obtain from the Cavendish apparatus results having a precision in any way comparable to that of other physical measurements. The three papers, published by him in the *Comptes rendus* of 1878, referred to above, contain a very complete solution of some of the problems to which the investigation has given rise. The agreement between the successive values, decrement, and period is much the same as I have obtained, nevertheless the means of the summer and of the winter observations differ by about 1 per cent.

I have not referred to the various methods of determining the constant of gravitation in which a balance, whether with the usual horizontal beam, or with a vertical beam on the metronome principle, is employed. They are essentially the same as the Cavendish method, except that there is introduced the friction of the knife-edges and the unknown disturbances due to particles of dust at these points, and to buoyancy, without, in my opinion, any compensating advantage. However, it would appear that if the experiment is to be made with a balance, the considerations which I have advanced in this paper would point to the advantage of making the apparatus small, so that attracting masses of greater proportionate size may be employed, and the disturbance due to convection reduced.

It is my intention, if I can obtain a proper place in which to make the observations, to prepare an apparatus specially suitable for absolute determinations. The scale will have to be increased, so that the dimensions may be determined to a ten-thousandth part at least. Both pairs of masses should, I think, be suspended by fibres or by wires, so that the distance of their centres from the axis may be accurately measured, and so that, in the case of the little masses, the moment of inertia of the beam, mirror, &c., may be found by alternately measuring the period with and without the masses attached. The unbalanced attractions between the beam, &c., and the large masses, and between the little masses and anything unsymmetrical about the support of the large masses, will probably be more accurately determined experimentally by observing the deflections when the large and the small masses are in turn removed, than by calculation.

If anything is to be gained by swinging the small masses in a good Sprengel vacuum, the difficulty will not be so great with apparatus made on the scale I have in view, *i.e.* with a beam about 5 centimetres long, as it would with large apparatus. With a view to reduce the considerable decrement, I did try to maintain such a vacuum in the first iustrument, in which a beam 1·2 centimetre long was suspended by a fibre so fine as to give a complete period of five minutes, but though the pump would click violently for a day perhaps, leakage always took place before long, and so no satisfactory results were obtained.

With an apparatus such as I have described, but arranged to have a complete period of six minutes, it will be possible to read the scale with an accuracy of 1/10,000 of the deflection, and to determine the time of vibration with an accuracy about twice as great.

I hope early next year, in spite of the difficulty of finding a suitable place to observe in, to prepare apparatus for absolute determinations, and I shall be glad to receive any suggestions which those interested may be good enough to offer.

C. V. BOYS.

WILLIAM RAMSAY McNAB.

WILLIAM RAMSAY McNAB, M.D., whose sudden death from heart-disease we have already recorded, was born in Edinburgh in November 1844. He was educated at the Edinburgh Academy, and afterwards in the University of that city, obtaining the degree of Doctor of Medicine when twenty-two years of age.

His grandfather and father, in succession, held office as Curators of the Edinburgh Botanic Garden; and the late Dr. McNab early manifested an inherited capacity for botanical work; for, while still an undergraduate, he was appointed assistant to Prof. Balfour, who then held the Edinburgh botanical chair. He also entered the University of Berlin as a student—in botany under Profs. Braun and Koch, and in pathological anatomy and histology under Prof. Virchow. Three years of his later life were spent in medical practice; but his love of botany was his dominant feeling, and in 1870 he embarked upon a purely biological career, having been then appointed to the Professorship of Natural History

in the Royal Agricultural College, Cirencester. Two years later he succeeded to the Chair of Botany in the Royal College of Science, Dublin, and this post he held until his death. During his student life he paid considerable attention to the practical study of geology; and for many years he collected Coleoptera, of which he possessed a very fine collection, now in the Dublin Museum of Science and Art.

During the nineteen years exclusively devoted to natural science, Prof. McNab published a considerable number of technical papers; most of these were short, but some forty or fifty of them are fit to rank as original communications. The work by which he is best known was that upon the movements of water in plants. Following a suggestion of Prof. A. H. Church, that lithium might prove useful in his researches, he instituted experiments which proved the value of this method, and paved the way for later investigators. McNab's chief claim to distinction lay, however, not in the direction of pure research, but in the fact of his having been the first to introduce to British students the methods of Sachs, now universally adopted. He inaugurated the modern methods of teaching botany at Cirencester, in the year 1871, and at Dublin two years later; and he fully admitted his indebtedness to the first edition of Sachs's celebrated "Lehrbuch der Botanik." Dr. McNab was, at the time of his death, an examiner in botany to the Victoria University, Manchester. The appointment of Scientific Superintendent of the Royal Botanic Gardens, Glasnevin, Dublin, was created for him in 1880, and in connection with this office he issued, five years later, an enlarged and considerably revised Guide-book. He was joint author, with Prof. Alex. Macalister, of a "Guide-book to the County of Dublin," prepared on the occasion of the visit of the British Association to that city. In 1878 he published, in Longmans' "London Science Series," two botanical class-books, entitled "Outlines of Morphology and Physiology," and "Outlines of Classification"; and he leaves behind him the first few chapters, and a large amount of manuscript in a nearly completed condition, of a contemplated "Text-book of Botany," which he was to have written for Messrs. C. Griffin and Co. In 1888 he was appointed Swiney Lecturer to the British Museum of Natural History, and in that capacity he has lectured for two sessions. His discourses, which were upon "The Fossil Plants of the Palæozoic Epoch" and "Ferns and Gymnosperms of the Palæozoic and Mesozoic Epochs, and dawn of the Angiospermous Flora" respectively, were attended with much success. He has left behind him carefully written manuscript lectures, which it is sincerely hoped may be published as a memorial volume. At the time of his decease he was actively engaged upon his intended third course, in which he would have dealt with the Cainozoic flora. He was an excellent teacher, possessed of a natural aptitude for the work; and his laboratory instruction was characterized by thoroughness and precision. As a lecturer he was fluent and entertaining; and, in his several capacities, he endeared himself to those with whom he came in contact. Friends, colleagues, and students, alike mourn his loss.

NOTES.

THE death of Prof. Lorenzo Respighi, Director of the Osservatorio Campidoglio, Rome, which we deeply regret to announce, is a great loss to science. He died on December 10.

IN a recent number we gave some account of a meeting held in Manchester on November 25 for the purpose of preparing the way for the erection of a memorial of James Prescott Joule in that city. It was resolved that the memorial should be in the form of a white marble statue, and a committee was appointed to carry out this resolution. At the first meeting of the committee, on November 29, an executive committee was appointed, and the following motion was adopted:—"That the movement be directed to secure, not only a marble statue of the late Dr. James Prescott Joule as a companion to that of the late Dr. Dalton by Sir Francis Chantrey, but also a replica in bronze to occupy some public place in the city, and that the executive committee be instructed to take all needful steps for that purpose." Many subscriptions have been already promised.

AN attempt is being made to secure the erection of an international monument to James Watt at Greenock, his birthplace. It is proposed that the memorial shall be "a large and thoroughly equipped technical school."

A NEW fortnightly scientific periodical is about to be published in Paris. It will be entitled *Revue Générale des Sciences Pures et Appliquées*, and will deal with the mathematical, physical, and natural sciences, and with their applica-tions in geodesy, navigation, engineering, manufactures, agriculture, hygiene, medicine, and surgery. According to the preliminary statement, the new periodical will take as its model the method of exposition adopted in NATURE. The editor is M. Louis Olivier, and the list of contributors includes many of the most eminent French men of science. The first number will appear on January 15, 1890.

THE second Report of the Committee appointed by the British Association to inquire into, and report upon, the present methods of teaching chemistry, which was presented at the Newcastle meeting, and to which we called attention in these columns a short time ago, has now been put on sale by the Council. It may be obtained from the office of the Association, 22 Albemarle Street, W.

ON Tuesday evening, after the distribution of the prizes and certificates to the students of the City and Guilds of London Institute, at Goldsmiths' Hall, Sir Henry Roscoe congratulated the students of the various schools upon the reports he had heard. He observed that the City Guilds were now engaged separately and collectively in nobly carrying out the work for which they were, to a certain extent, originally founded. The Technical Instruction Bill which was passed in the last session of Parliament had materially changed the whole aspect of affairs, and sooner or later a complete scheme for technical education would have to be framed. The beginning of such a scheme had been made by the efforts of the City of London Institution, which, with its many branches, was a nucleus of such a system, the importance of which would only be recognized when the history of that important movement came to be written. It was a satisfactory thing to hear that employers of skilled labour were beginning to find out that the men who had been trained at such Colleges as these were of greater value than those who had not received such training. It was not only necessary to educate the craftsman; the employer needed it equally, if not more. He thought that the Council of the Institute had fully recognized that fact at their Central Institution, but a demand for high-class education had yet to be created.

THE *British Medical Journal* says that owing to the somewhat late period in the year at which the invitation to hold the annual meeting of the British Medical Association in Birmingham was received and accepted, the arrangements are not yet so complete as in former years; but a large general committee and all the necessary sub-committees have been formed, and the use of the requisite public buildings has been obtained.

ON March 1, 1890, a new marine laboratory will be opened at Saint-Wast-la-Hougue.

WE are glad to know that there will soon be well-equipped physical and chemical laboratories at Bedford College, London. Mr. Tate, who has already given £1000 towards the new College buildings, which are on the eve of completion, has

offered a second £1000 towards the fitting up and equipment of the laboratories, contingent on the friends of the College contributing an equal amount. We purpose shortly giving an account and plans of these laboratories.

MORE than a quarter of a century has passed since it was decided that the *Entomologist's Monthly Magazine* should be started. The editors have now resolved to issue a new series, each volume of which will begin in January and end in December. There will be no radical change in the constitution of the magazine, but the number of pages and illustrations will often be increased.

THE result of the poll for a free library at Whitechapel, declared last Saturday night, is interesting and significant. On a register of 6000, there were 3553 affirmative votes and only 935 dissentients. This is the more noteworthy, because about eleven years ago a like proposal was rejected by a majority of about two to one.

THE following science lectures will be given at the Royal Victoria Hall during January: January 7, "A Visit to the Chief Cities of Italy," by Rev. W. W. Edwards; January 14, "The Bottom of the Sea," by Dr. P. H. Carpenter; January 21, "To Vancouver's Island and back," by Mr. W. L. Carpenter; January 28, "Musical Sounds and how we hear them," by Dr. F. W. Mott.

A SECOND edition of Sir William Aitken's "Animal Alkaloids" (H. K. Lewis) has been published. The work has been carefully revised, and the author's aim has been to bring the book up to the present state of knowledge regarding the important subject to which it relates.

THE first part of a monograph of Oriental *Cicadidæ*, by W. L. Distant, has been published by order of the Trustees of the Indian Museum, Calcutta. It is printed in clear type, and includes two fine plates. The monograph, when completed, will evidently be of much scientific value.

M. VAYSSIÈRE has now completed the publication of his "Atlas d'Anatomie Comparée des Invertébrés." It comprises sixty plates, with corresponding letterpress, and is much appreciated by French zoologists.

THE Proceedings and Transactions of the International Agricultural Congress held in Paris last summer have just been issued.

A REUTER'S telegram from Madrid says that a shock of earthquake was felt at Granada on the evening of December 16. There was great alarm for the moment, and the people rushed in panic out of the theatre, where a performance was going on at the time. Apparently no damage was done.

THE Pilot Chart of the North Atlantic Ocean for December states that stormy weather has been prevalent during the month of November. Two notable cyclones have occurred; the first moved eastward from Chesapeake Bay on the night of the 9th. On the 11th it was central in about latitude 41° N., longitude 57° W.; and from this position it moved nearly due north-east, and rapidly increased in energy. The other cyclone moved eastward from the New Jersey coast on the 13th, and was central on the 14th in latitude 42° 40′ N., longitude 63° 20′ W. This storm attained great violence during the 14th and 15th. After the 16th, gales of varying force occurred along the coast north of Florida. There was very little fog during the month; a dense bank was reported on the 17th on the north coast of Cuba. A number of icebergs are still reported in the vicinity of Belle Isle, and several smaller bergs have been seen over the Newfoundland Banks.

AT the meeting of the French Meteorological Society on November 5, M. Teisserenc de Bort gave an account of his researches on barometric gradients. He distinguished two kinds of gradients, one due to the differences of temperature, and another due to the earth's rotation, and pointed out that these two gradients are always superposed, and that their distinction was a matter of importance, for if the first case predominates (a gradient due to difference of temperature), the wind force may increase and the depression become deeper, while in the second case the depression tends to disappear. He thought it was not impossible to make this distinction, for if we know the force of the wind we might calculate the moment of inertia and the corresponding gradient. He also presented a work on the distribution of atmospheric pressure over the surface of the globe. He showed that the distribution of pressure over different meridians varies upwards of an inch on the same parallel according to the season. With the view of finding out the arrangement of the isobars in higher regions of the atmosphere, the author has calculated the pressures by formulæ at various heights, from the pressure and temperature observed at the earth's surface, and compared their accuracy by the readings at some mountain stations, and he has found that most of the irregularities in the distribution of the isobars tend to disappear as we reach the higher regions of the air, and to be replaced by inflexions in the opposite sense. A summary of this paper will be found in the *Comptes rendus* of the French Academy for December 2.

AT a meeting of the Linnean Society of New South Wales on October 30, Mr. A. Sidney Olliff called attention to the extraordinary abundance of a large Noctuid moth—apparently *Agrotis spina*, Gu. (*A. vastator*, Sc.)—during the early part of October in various parts of the country, especially in the vicinity of Sydney, where it appeared in such vast numbers as to cause great consternation amongst those who were not aware that its food in the larval state is confined to low-growing herbage, and that at no stage of its existence does it eat cloth, furs, or feathers. A similar visitation of these moths occurred in October 1867. Mr. Olliff said that *Agrotis spina* was found in great numbers on the summit of Mount Kosciusko and other high points in the Australian Alps, and added that he was of opinion, after extended inquiry, that this species, and no other, was the true Bugong moth, which formerly formed an important article of food amongst the blacks of the Upper Tumut district.

MR. THOMAS CORNISH, Penzance, recently recorded in *Th Zoologist* the occurrence of the "Old English" or "Black" Rat, captured at a place about five miles north-east of Penzance. In the current number of the same periodical he says that immediately after that capture a perfectly trustworthy observer saw near Cambourne, at a place ten miles south-east from where the first specimen was obtained, a Black Rat, which was certainly not the ordinary Hanoverian Rat; and at a later time Mr. Cornish saw and handled another specimen, captured in Paul Parish, about three miles south-west of Penzance. "These facts," says Mr. Cornish, "apparently point to an incursion of this animal, which is gregarious certainly, and probably a vagrant in herds, but not a migrant."

MR. J. R. DOBBINS, San Gabriel, California, contributes to the new number of *Insect Life* (vol. ii. No. 4) a note on the spread of the Australian ladybird. The note is dated July 2, 1889. At that time the Vedolia had multiplied in numbers, and had spread so rapidly that every one of Mr. Dobbins's 3200 orchard trees was literally swarming with them. All his ornamental trees, shrubs, and vines which had been infested with white scale were practically cleansed by this wonderful parasite. "About one month since," says Mr. Dobbins, "I made a public statement that my orchard would be free from 'Icerya by November 1,' but the work has gone on with such amazing speed and thoroughness, that I am to-day confident that the pest will have been exterminated from my trees by the middle of August. People are coming here daily, and by placing infested branches upon the ground beneath my trees for two hours, can secure

colonies of thousands of the Vedolia, which are there in countless numbers sucking food. Over 50,000 have been taken away to other orchards during the present week, and there are millions still remaining, and I have distributed a total of 63,000 since June 1. I have a list of 130 names of persons who have taken the colonies, and as they have been placed in orchards extending from South Pasadena to Azusa, over a belt of country ten miles long and six or seven in width, I feel positive, from my own experience, that the entire valley wiil be practically free from Icerya before the advent of the new year."

COCOA-NUT butter is now being made at Mannheim, and, according to the American Consul there, the demand for it is steadily increasing. The method of manufacture was discovered by Dr. Schlunk, a practical chemist at Ludwigshafen. Liebig and Fresenius knew the value of cocoa-nut oil or fat, but did not succeed in producing it as a substitute for butter. The new butter is of a clear whitish colour, melts at from 26° to 28° C., and contains 0·0008 per cent. water, 0·006 per cent. mineral stuffs, and 99·9932 per cent. fat. At present it is chiefly used in hospitals and other State institutions, but it is also rapidly finding its way into houses or homes where people are too poor to buy butter. The working classes are taking to it instead of the oleomargarines against which so much has been said during the last two or three years.

A POINT of great importance for the progress of Western science in the Chinese Empire is whether it should be taught in the Chinese or in a foreign language. The subject has been frequently discussed, and quite recently the opinions of a large number of men most prominently engaged in the education of Chinese were collected and published in a Shanghai magazine, the *Chinese Recorder*. The editor says that nine-tenths of these authorities are of opinion that the Chinese language is sufficient for all purposes in teaching Western science. One gentleman states that Chinese students can only be taught science in their own language, and that the long time necessary for them to acquire English for this purpose is wasted; another says that "science must be planted in the Chinese language in order to its permanent growth and development"; a third sees no reason why the vernacular should not be enough to allow the Chinese student to attain the very highest proficiency in Western science, although he admits that there is at present a want of teachers and text-books. Prof. Oliver, of the Imperial University at Pekin, says he has never found English necessary, but has always taught in Chinese. Prof. Russell, of the same institution, finds Chinese sufficient for popular astronomy. On the other hand, Mr. Tenney says that it can only be for the most popular views of science that the vernacular is sufficient. "It is impossible," he says, "for scholars who are ignorant of any European language to attain any such excellence in modern sciences as to enable them to bear comparison with the finished mathematical and scientific scholars of Europe and America." Thus, he continues, as a medium of thought, any Western language is incomparably superior to Chinese in precision and clearness; the student acquainted with a foreign language has a vast field of collateral thought open to him which does not and never will exist in Chinese, and he can keep abreast of the times, which the Chinese student who must depend on translations cannot do. The relation of the Chinese student "to the world of thought is analogous to that of a blind and deaf person in the West, whose only sources of knowledge are the few and slowly increasing volumes of raised type letters which make up the libraries of the blind." As has been said, however, the weight of opinion is against Mr. Tenney.

IN a recent number of *Humboldt*, Herr Fischer-Sigwart describes the ways of a snake, *Tropidonotus tessellatus*, which he kept in his terrarium in Zurich. It was fond of basking in the sun on the top of a laurel, from which it climbed easily to a high cherry-tree fixed against a wall, its night quarters. Sometimes, after lying still for hours, it would hasten down into a snall pond (about 4 square yards surface) containing gold-fish, and hide itself for a long time, quite under water, behind some stone, or plants, the tongue constantly playing. When a fish came near, the snake would make a dart at its belly. Often missing, it would lose patience, and swim after the fishes, driving them into some corner, where it at length seized one in the middle of the belly, and carried it to land, much as a dog would a piece of wood. Curiously, the fish, after being seized, became quite still and stiff, as if dead. If one then liberated it, the skin of the belly was seen to be quite uninjured, and the fish readily swam away in the water. The author thinks the snake has a hypnotic influence on its prey (and he had observed similar effects with a ringed snake). It would otherwise be very difficult for the snake to retain hold of a wriggling fish. The snake usually carried off the fish some distance to a safe corner, to devour it in peace.

A SPLENDID find of minerals containing the rare metals of the yttrium and thorium groups has been made in Llano County, Texas (*Amer. Journ. of Science*, December 1889). The whole district for many miles round consists almost entirely of Archæan rocks, granite being met with everywhere, and forming the common wayside rock. Throughout the granite are dispersed veins of quartz, and it is in these veins, and especially the swellings of the veins, that large masses of rare minerals have been found. The largest of these deposits consist of gadolinite and fergusonite, and of two entirely new minerals, to which the names yttrialite and thoro-gummite have been given. The first discovery of gadolinite in Texas was made in 1886, when a pocket of huge crystals and masses aggregating to about 500 kilogrammes was unearthed. Since that time a more complete prospection of the district has revealed the existence of still larger quantities. The gadolinite is generally found in small lumps weighing about half a pound, but frequently also in much heavier masses, and sometimes in immense crystals. One double crystal was found weighing 42 pounds, and a still larger single crystal weighed no less than 60 pounds. And these immense crystals actually contain over 50 per cent. of oxides of the yttrium metals, as do also the massive varieties. The crust of the gadolinite crystals, which appear to be of monoclinic habit, was generally altered into a brownish-red hydrate of waxy lustre; but occasionally, as in case of two particular specimens, the crystals were found in a state of rare beauty and perfection. The new mineral yttrialite, a thorium-yttrium silicate, was discovered associated with and often upon the gadolinite. It was generally altered at the surface to an orange-yellow hydrate of quite different structure to that of the hydrate of gadolinite. One mass of this incrustation was found to weigh over 10 pounds. It contains 46 per cent. of oxides of the yttrium metals. Fergusonite, hitherto an exccedingly rare mineral, occurs in large quantities in the Llano County district, generally in the form of broken interlacing prisms several inches long. Two varieties of it have been identified—one a monohydrated and the other a trihydrated variety. The monohydrated kind forms tetragonal prisms with acute pyramidal terminations, of dull gray exterior, but possessing a brilliant bronze-like fracture. It contains 42 per cent. of yttrium earths and 46 per cent. of columbic acid, Cb_2O_5. The trihydrated variety is similar, but of a dark brown colour. Associated with the fergusonite is the new mineral thoro-gummite, a hydrated uranium thoro-silicate. This mineral is frequently found in well-developed crystals resembling, and having angles very nearly the same as those of zircon. It contains 22 per cent. of UO_3, 41 per cent. of ThO_2, and 6 per cent. of yttrium earths. Its probable essential composition is $UO_3 . 3ThO_2 . 3SiO_2 . 6H_2O$. Besides these four minerals of special interest to chemists, many more—such as

cyrtolite, molybdite, allanite, tengerite, and a new hydrated thorium-yttrium-lead uranate, termed nivenite—have been found. Altogether, this is the richest find of rare earths which has been heard of for some time, and will probably exert a fresh impetus upon the attempts to set our knowledge of the rare-earth elements upon a surer foundation.

THE additions to the Zoological Society's Gardens during the past week include a Ring-tailed Coati (*Nasua rufa* ♂) from South America, presented by Mrs. Petre; a Common Squirrel (*Sciurus vulgaris*), British, presented by Mrs. S. Stutterd; a Short-eared Owl (*Asio brachyotus*) from Hampshire, presented by Mr. E. Hart, F.Z.S.; two Owen's Apteryx (*Apteryx oweni*) from New Zealand, presented by Captain C. A. Findlay, R.N.R., R.M.S.S. *Ruapehu*; four Common Vipers (*Vipera berus*) from Hampshire, presented by Mr. W. H. B. Pain; a Marsh Ichneumon (*Herpestes galera*) from South Africa, purchased; a —— Troupial (*Xanthosomus frontalis*) from Brazil, received in exchange.

OUR ASTRONOMICAL COLUMN.

OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at Greenwich at 10 p.m., December 19 = 3h. 54m. 45s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G. C. 826	—	—	4 9 15	−13 1
(2) γ Eridani	3	Yellowish-red.	3 52 53	−13 46
(3) ε Persei	3	Yellowish-white.	3 50 30	+39 39
(4) δ Persei	3	Bluish-white.	3 51 6	+47 26
(5) 43 Schj.	8	Very red.	4 44 37	+28 20
(6) S Tauri	Var.	—	4 23 10	+ 9 42

Remarks.

(1) This is described in the General Catalogue as "a globular cluster, very bright, small, round, very suddenly brighter in the middle, barely resolvable (mottled as if with stars)." In 1864 Dr. Huggins observed the spectrum, and noted that it was apparently continuous, extending from the orange to the blue, without any traces of either bright or dark lines. It was again observed by Winlock at Harvard College in December 1868, and, strange to say, a bright line spectrum was recorded. "Two distinct bright lines, near each other, and coincident with air-lines λ 5020 ± and λ 4990; a third faint line λ 4900 ±" ("Harvard College Observations," vol. xiii. Part 1, p. 64). These lines were in all probability the three ordinary nebula lines near λ 500, 495, and 486. Winlock describes the nebula as planetary, and gives exactly the same co-ordinates as those given by Huggins and in the General Catalogue. If both observers really saw the same nebula, the results are highly suggestive of variability; but even then there is the difficulty of the recorded resolvability. It is quite possible that, in the four years which elapsed between the observations, the spectrum changed from an apparently continuous one to a discontinuous spectrum, by some action similar to that producing variability in such stars as Mira, but at the same time a change of brightness would also be expected, and of this there is no record. In any case, the nebula is well worthy of further examination.

(2) This star of Group II. is interesting, as being a connecting-link between stars like α Herculis, in which the bands are very wide and dark, and those like Aldebaran, in which there is a line spectrum with only the remnants of the bands in the red. Dunér states that the bands 2–8 are visible, but all of them are narrow and pale. *b*, and presumably D, are very strong. Further observations, with special reference to the lines of hydrogen, are suggested.

(3) A star, hitherto described as of the solar type, of which the usual observations are required. If the star appears to be of the same type of the sun or Capella, special attention should be directed to the presence or absence of dark carbon flutings. It is highly probable that stars like the sun, in which there is a photographic indication of carbon absorption, will subsequently cool down and become stars of Group VI., in which carbon absorption is predominant. If this be the case, all the intermediate stages of mixed metallic lines and dark carbon flutings should be represented amongst the stars.

(4) A star of Group IV., of which the usual observations are required.

(5) This is a star of Group VI. The three ordinary bands of carbon are visible, band 6, near λ 564, being rather pale. A study of Dunér's catalogue of the stars of this group shows that some of those in which band 6 is pale give secondary bands, whilst others do not. This appears to be mainly, though not entirely, due to differences of magnitude. Comparative observations with the same telescope and spectroscope, with reference to this point, are suggested.

(6) Gore states the period of this variable as 378 days, and the magnitudes at maximum and minimum as 9·9 and < 13 respectively. The colour is described as trifling, but the spectrum has not yet been recorded. The maximum will occur on December 28.

A. FOWLER.

PERIOD OF U CORONÆ.—Mr. S. C. Chandler (*Astronomical Journal*, No. 205), from the observations of the period of this star, finds an inequality of the same order as those detected in U Ophiuchi and U Cephei, variables of the Algol type. The period appears to be shortening by 0·0036s. from minimum to minimum. The results depend upon forty-five very unequally distributed minima; thirty-eight, however, lie in the interval 1870–74, and afford a basis to work upon. A larger series of observations is required to elucidate Mr. Chandler's hypothesis, which, however, is quite conformable within the limits of the purely accidental errors of the observations that have been investigated.

IDENTITY OF BROOKS'S COMET (*d* 1889) WITH LEXELL'S COMET 1770.—In the same publication as the above, Mr. Chandler gives some most interesting results of an investigation into the orbits of these comets. The following is a summary of the principal conclusions:—

(1) The encounter of the comet with Jupiter in 1886 effected a complete transformation of the comet's orbit. Instead of the present seven years' ellipse, it was previously moving in a large one of twenty-seven years' period.

(2) Several months before reaching its perihelion, it passed, near the beginning of 1886, into the sphere of Jupiter's attraction, and was deflected into a hyperbolic path about that planet, and narrowly escaped being drawn into a closed orbit, as a satellite of Jupiter.

(3) At the point of closest approach to Jupiter, May 20, 1886, the comet was distant from it only about nine diameters of the planet, passing a little outside of the orbit of the third satellite.

(4) In 1779, and not before, the comet must have come so near to Jupiter as to pass under his control and experience a radical change of orbit at the point of longitude where Lexell's comet underwent its notable disturbance in that year. Moreover, the elements of Lexell's comet before the disturbance were strikingly similar to those found for the present comet previous to 1886.

Taking all the points presented into consideration, the argument for the identity of the two comets is overwhelming. A fuller investigation will be made as soon as the observations for the whole apparition have been received.

SOME PHOTOGRAPHIC STAR SPECTRA.—An examination has been made by Dr. Scheiner of the star spectra photographed at Potsdam (*Astr. Nachr.*, No. 2923). The wave-lengths of lines in the spectra were determined by comparison with the solar spectrum, and as the probable error of the measures is estimated so small as 0·005, the identification of the lines seems beyond doubt. The following are some descriptive results:—

γ *Cassiopeiæ*. Continuous spectrum; hydrogen lines and D_3 bright.

α *Coronæ*. The magnesium line at 448·2 appears as a broad line in this star.

α *Lyræ*. Some fine lines, supposed to be due to iron or calcium, are seen, but have not been measured.

Sirius. 91 similar fine lines to those in the above star have been measured, and 43 ascribed to iron. Even more of these lines occur in Procyon.

α *Aquilæ*. The spectrum of this star appears almost identical with that of the sun.

β *Orionis*. The hydrogen and other lines appear broad, but are not diffused at the edges as in α Lyræ and similar stars. 20 lines have been measured from λ 400 to λ 460.

a Aurigæ. 291 lines have been measured in the spectrum of this star between λ 410 and λ 470, all of which appear identical with solar lines.

MAGNITUDE AND COLOUR OF η ARGÛS.—Observations of this variable have been made at Cordoba since 1871, and some comparisons made by Mr. Thome (*Astr. Nachr.*, No. 2922) show that it steadily decreased in magnitude until about the end of 1886, when a minimum of 7·65 was reached, and it is now about 6·6. In 1843, Maclear gave the brightness of η Argûs as − 1·0, or between that of Sirius and Canopus, so that the variation in magnitude is 8·5, and not 6 as heretofore assumed, this variation, extending over 44 years, gives an average yearly rate of diminution of 0·2.

It is interesting to note that the change in magnitude was accompanied by a change in colour; for although before minimum the star was of a dull scarlet the colour became lighter, until in June 1889 it was a bright orange.

ORBIT OF BARNARD'S COMET 1884 II.—From an investigation of all the available observations of this periodic comet, Dr. Berberich has computed the following elements (*Astr. Nachr.*, 2938–39).

Epoch 1884 August 16·5, Berlin Mean Time.

$$\begin{aligned} M &= 359^\circ\ 59'\ 49''\cdot13 \\ \omega &= 301\ \ 1\ \ 58\cdot63 \\ \Omega &= 5\ \ 8\ \ 59\cdot12 \\ \iota &= 5\ \ 27\ \ 38\cdot40 \\ \phi &= 35\ \ 44\ \ 50\cdot92 \\ \mu &= 657''\cdot0839 \pm 0''\cdot8876 \\ \log a &= 0\cdot4882572 \end{aligned}$$

Perihelion passage = 1884 August 16·516543
Period = 1972·35 ± 2·66 days.

It will be seen from the foregoing period, that the comet will be at perihelion again in 1890 January 9·87.

ALGOL.—At the meeting of the Royal Prussian Academy of Sciences, held on November 28, Prof. Vogel gave the results he had obtained from photographs of the spectrum of this variable. Prof. Pickering had pointed out, some years ago, that if the variation in stars of the Algol class were due to the transit of a dark satellite across the disk of its primary, producing a partial eclipse, then since in every case yet known the two bodies must be close to each other, and of not very disproportionate size, the primary must revolve with very considerable rapidity in an orbit round the common centre of gravity of the two; and, therefore, be sometimes approaching the earth with great rapidity and sometimes receding from it. Six photographs of the spectrum of Algol—obtained, three during last winter, and three during the November just past—have shown that before the minimum the lines of the spectrum of Algol are markedly displaced towards the red, showing a motion of recession; but that after the minimum the displacement is towards the blue, showing a motion of approach. Assuming a circular orbit for the star, and combining the details given by the spectroscope with the known variation of the star's light, Prof. Vogel derives the following elements for the system of Algol:—

Diameter of Algol...	1,074,100 English miles.
Diameter of the dark companion	840,600 ,, ,,
Distance of centre...	3,269,000 ,, ,,
Speed of Algol in its orbit	27 miles per second.
Speed of the companion in its orbit ...	56 ,, ,,
Mass of Algol	4/9 of the sun.
Mass of the companion	2/9 ,, ,,
Speed of translation of the entire system towards the earth	2 miles per second.

It will be seen that the density both of Algol and its companion is much less than that of the sun—less than a quarter, in fact. This is what we might expect, for Algol and all the variables of its class yet examined give spectra of Group IV., and should therefore represent a less advanced stage of condensation than that seen in our sun. This demonstration of the truth of the satellite theory of variation of the Algol type derives also an especial interest from Prof. Darwin's researches on tidal evolution, for assuming, as we well may, that the cause of variation is the same in all members of the class, we now know of nine stars in which a large companion is revolving round its primary at but a very short distance from it, and in a very short space of time. The companion of U Ophiuchi must, indeed, be almost in contact with its parent star.

DISCOVERY OF A NEW COMET.—A faint comet was discovered by M. Borrelly at Marseilles, on December 12, at 7h. 49·5m. G.M.T. R.A. 18h. 7m.; daily motion in R.A. + 1m. 12s. N.P.D. 41° 7′; daily motion + 60′.

GEOGRAPHICAL NOTES.

WE regret to have to record the death of Major Peter Egerton Warburton, whose name will always be intimately associated with the history of exploration in Australia. He died at Beaumont, Adelaide, in his seventy-sixth year. His most famous achievement, undertaken in 1873, was the crossing of the continent from a point on the overland telegraphic line to the De Grey River, in Western Australia. Nothing was heard of him for about twelve months, during which he and his party suffered terrible privations in their march across the desert. After the expedition, Major Warburton visited England, and was awarded a Gold Medal of the Royal Geographical Society for his efforts towards increasing our knowledge of the interior of Australia. He received the Companionship of the Order of St. Michael and St. George in 1875.

THE death is announced of Cardinal G. Massaja in his eighty-first year, at St. Georgio a Cremano. For nearly half a century the name of this distinguished explorer has been intimately associated with the progress of geographical discoveries in Abyssinia and the surrounding regions. It was at his suggestion that the Italian Geographical Society organized the Antinori Expedition to Shoa, which has resulted in the occupation of a vast region, and the extension of Italian influence over the whole of Ethiopia. His chief work, "I miei trentacinque Anni nell' alta Etiopia," abounds in valuable geographical, historical, and ethnological information on the East African regions for so many years explored and studied by him. The Cardinal was born at Piovà in 1809, and, in 1846, appointed Vicar Apostolic of the Galla nation.

FROM the Berlin Correspondent of the *Daily News* we learn that a full account of the ascent of Kilimanjaro by Dr. Hans Meyer and Prof. Purtscheller has been received at Berlin. It is dated "Marangu Jagga, October 9." The journey from Zanzibar to Uawela took exactly a fortnight. On September 25 the travellers reached Marangu. On October 2 they encamped, with a Pangani negro, on the ridge of the plateau, at a height of 14,450 feet. At 2.30 a.m. they started for the lava-ribs surrounding the valley of glaciers to the south about 1200 feet above. At 7 o'clock, on the right side of the valley, at an elevation of about 16,500 feet, the first snow was seen under cover of the rocks. The higher they went, the more clefts and fissures the field of ice had. The travellers say:—"After great exertions we reached, at 1.45, the snow-line, and it was seen that the highest peak, which was formed of rocks jutting out of the snow, was about one and a half hour's march to the left. After resting a day and a half we set off, on October 5, to bivouac in the Lava Cave, at a height of about 15,200 feet, and on the next day we repeated the ascent. The peaks were gained without particular difficulty, and on the central and highest one, 19,680 feet above the sea, the German flag was planted." Dr. Meyer proposes to call this peak Kaiser Wilhelm Peak. The view from here on to the Kibbs Crater—which is 6600 feet broad and 660 feet high, and the lower half of which is encased in a mighty belt of ice, whilst a volcanic cone of about 500 feet rises in the centre—is magnificent. The beauties of the landscape in the Kilimanjaro region seem to be quite extraordinary. On October 10 the Kimawensi was to be ascended. The two travellers enjoy the best of health.

THE double number of the *Bollettino* of the Italian Geographical Society for October and November, which appears some weeks behind time, is largely devoted to African subjects, and more particularly to the north-eastern region, which is rapidly becoming an "Italian colony." Captain D. Stasio publishes a summary of Don Francesco Alvarez's "Travels in Ethiopia" in the sixteenth century, enriched with valuable notes and additions. Alvarez, a priest attached to an embassy forwarded by Portugal, in 1520, to the Emperor of Abyssinia, shows himself a careful observer of men and things, and his work, which was included in Ramusio's "Navigationi et Viaggi" (Venice, 1588), abounds in details regarding the political, social, and economic relations of that region in the sixteenth century. Giulio D. Cocorda brings to a conclusion his important series of papers

on the South African gold-fields, which include much information on the present condition of the whole of South Africa as far north as the Zambesi. The observer points out that, while the Delagoa Bay and other lines of communication are much discussed, the fine artery of the perfectly navigable Limpopo is entirely neglected, notwithstanding Captain Chaddock's navigation of it a few years ago. The writer remarks that "this river flows mainly through regions under the influence or protectorate of England; the Transvaal people on the one side, and those of Matabeleland on the other, would certainly be glad to avail themselves of this outlet for their produce. As it traverses only a small tract of Portuguese territory about its estuary, I hope and believe that Portugal will not be allowed to treat the Limpopo as she is now attempting to treat the Zambesi. The subject is of such importance that it cannot fail soon to be brought before the British Parliament." Referring to the negotiations at present going on in connection with the Swaziland question, he observes, in the same spirit:—"The Swazi people must, sooner or later, yield either to the Transvaal or to England, and if to the former, it must be to the entire detriment of British interests. England, as the suzerain power in South Africa, should be the first in the field, both in her own interest and in that of her other colonies and subjects. If she does not assume the protectorate of Swaziland, besides losing the control of a vast and rich mineral district, she will deprive the colony of Natal of all further hope of expansion. If she ignores her responsibility in this matter, and allows the Transvaal Republic to absorb Swaziland, she will add another to the long list of blunders that threaten to destroy all prospect of consolidating a dominion as large as Canada, and may end disastrously for British interests in South Africa."

A FRENCH traveller has just achieved a feat of great interest. Captain Trivier, equipped by the newspaper *La Gironde*, started some eighteen months ago for the Congo State. He went up the river to Stanley Falls, and thence proceeded to Central Africa and the Lake region, accompanying caravans. He has just arrived at Mozambique.

Globus reports that during the past summer M. Thoroddsen, the well known student of Iceland, has carried out a journey in the waste region known as Fiskivötn, lying between Hecla and the Vatna Jökul, which has hitherto been unvisited for the most part by any inquirer. To the east and north of Hecla he discovered a new obsidian region. Crossing the Tunguaa, he went to the Fiskivötn group of lakes, all true crater lakes. The district between this and the Vatna Jökul has absolutely no plant-life whatever; it consists of lava-fields, and plains of volcanic sand. In it he found a lake, Thorisvatn, the second largest in the island. Thence, after a day's journey through an utterly desolate district, he reached the hitherto unknown source of the Tunguaa. To the south of this he discovered, between three ranges of hills, previously unknown, a new and very long lake.

MR. DAUVERGNE has, says the *Times of India*, completed an adventurous journey in the regions of North-West Cashmere. His course was from Leh northwards to the Kilian Pass, in Kashgaria, and then northwards across the Pamir to the Upper Oxus. He reached Sarhad in safety, and after six days' halt there, crossed the Hindu Kush by the Baroghil Pass, as he did not wish to visit Chitral. He then turned eastwards, and after a trying journey through the snow, crossed the Ishkaman Pass, north of Yasin. Thence he travelled southwards by the Karambar Valley, and eventually reached Gilgit, a short time after Captain Durand had started for Chitral. Mr. Dauvergne reports that the Russian explorer, Captain Grombchevsky, whose attempt to reach Kafiristan was noticed some time ago, was stopped at Kila Panjah on the Oxus, by the Afghan authorities.

THE ST. PETERSBURG PROBLEM.

THIS celebrated problem, which is first mentioned before 1708 in a letter from the younger Nicholas Bernoulli to Montmort, has been frequently discussed by Daniel Bernoulli (1730) and other eminent mathematicians. It may be briefly stated as follows:—

A tosses a coin, and undertakes to pay B a florin if head comes up at the first throw, two florins if it comes up at the second, four florins if it be deferred until the third throw, and so on. What is the value of B's expectation?

The chance of head appearing at the
1st, 2nd, 3rd, 4th . . . . nth throw is
$\frac{1}{2}$, $\frac{1}{4}$, $\frac{1}{8}$, $\frac{1}{16}$ . . . . $\frac{1}{2}^n$. A promises to pay for head
1, 2, 4, 8 . . . . 2^{n-1} florins, hence B's expectation is
$\frac{1}{2}$, $\frac{2}{4}$, $\frac{4}{8}$, $\frac{8}{16}$ . . . . $2^{n-1}/2^n = \frac{1}{2}$ florin.

Hence the total value of B's expectation is an *infinite* series, each term of which is a shilling, or it is infinite.

This result of the theory of probability is apparently directly opposed to the dictates of common-sense, since it is supposed that no one would give even a large finite sum, such as £50, for the prospect above defined.

Almost all mathematical writers on probability have allowed the force of the objection, which they have endeavoured to evade by various ingenious artifices all more or less unsatisfactory.

The real difficulty of the problem seems to lie in the exact meaning of *infinite* and *value of the expectation.*

Since the infinite value of the result is *only* true if an infinite number of trials are paid for and made, all such considerations as want of time and the bankruptcy of A or B are precluded by the terms of the question.

The value of B's expectation is frequently confused with how much he can or ought to pay for it; thus Mr. Whitworth ("Choice and Chance," p. 234) finds that if B have 1024 florins, he may give very little more than 6 florins for the venture. This ingenious, solution seems to have no reference to the original problem, which has been modified by Mr. Whitworth's introduction of the word "advantageously" (p. 232).

B can pay for his expectation in three ways: (i.) a sum before each toss; (ii.) a sum before each series of tosses ending with head; (iii.) a sum for the total result of A's operations.

Mr. Whitworth apparently assumes the first method of payment, and shows that the larger B's funds are the more he may safely pay for each toss, since he can continue to play longer. Many mathematicians take the second method of payment. "However large a fee I pay for each of these sets, I shall be sure to make it up in time" ("Logic of Chance," p. 155).

It is easy to show in this case also that what may be safely paid before each series increases with the number of series.

Suppose a very large number of tosses made, about half would come up heads and half tails; each head would end a series, when a fresh payment must be made by B. Suppose the tosses limited to one series, if B pays one florin he cannot possibly lose, if he pay anything more he may lose by head coming up the first time, and the more he pays the greater will his chance of loss be, since the series of tails must be longer to cover it. But, however large a finite sum he pays, he is not *certain* to lose, *e.g.* head *may* not come up till the hundred and first toss, when he would receive

2^{100} = 1,267 650,600 228,229 401,496 703,205 376 florins.

If the sets are limited to one hundred, about

50 heads would probably come up the			1st	toss.	B would receive for each series 50 florins.
25	,,	,,	2nd	,,	
13	,,	,,	3rd	,,	
6	,,	,,	4th	,,	
3	,,	,,	5th	,,	
2	,,	,,	6th	,,	
1	,,	,,	7th	,,	

Hence for the hundred sets, B would receive about 350 florins, or he could pay without loss seven shillings for each set.

If N be the number of sets, the total amount received by B will probably not be less than n terms of the series

$$\left\{\frac{N \times 2^0}{2^1} + \frac{N \times 2^1}{2^2} + \&c.\right\} = n\{\tfrac{1}{2}\}N,$$

but n is the number of times which N is successively divisible by 2, or $2^n = N$, or $n = \log N/\log 2$. But the amount x which B can afford to pay per set when multiplied by the number of sets is equal to the amount which he receives, or—

$$xN = \frac{\log N}{\log 2}\{\tfrac{1}{2}\}N,$$

hence $x = \log N/0{\cdot}6$ nearly.

This formula, though inexact for low, is very convenient for high, values of N.

N = 1	x = 0	N = 10^6	x = 10
= 50	= 2·7	= 10^9	= 15
= 100	= 3·3	= 10^{12}	= 20
= 1000	= 5	= 10^{15}	= 25

x increases with, though much more slowly than, N, and becomes infinite when N does. But to justify a payment of

£50 per set, we must expect a number of sets represented by 301 figures.

Lastly, what is the value of B's expectations if A's operations are continued indefinitely. With great deference to contrary opinions, I believe this to be the correct meaning of the problem in its original form. The theoretical result is in this case easily realized by the aid of the following illustration. Suppose the person A replaced by an automatic machine similar to that used for weighing sovereigns, which tosses continuously ten times per minute. On the average of a large number of tosses, B cannot receive less than one shilling a toss, £1 every two minutes, or £720 a day for ever. If the current rate of interest be 3 per cent., he may safely pay for this perpetual annuity £8,760,000. Suppose, instead of this comparatively slow rate, the machine increased the rapidity of its operations indefinitely, the sum to be paid for the result would also increase indefinitely, or the expectation would become infinite.

SYDNEY LUPTON.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—The Newall Telescope Syndicate has drawn up a scheme for building a dome for the telescope on a site adjoining the present Observatory, with an observer's house; and they recommend that an observer be appointed, at a stipend of £250 per annum, with a house, to devote himself to research in stellar physics, under the general direction of the Director of the Observatory.

The results of this year's commercial examination, held by the School's Examinations Board, are satisfactory. Geography was still very imperfect. Elementary mechanics has now been added to the list of compulsory subjects.

An influential syndicate has been appointed to consider the question of the mechanical workshops, their management and utility.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, December 12.—"An Experimental Investigation into the Arrangement of the Excitable Fibres of the Internal Capsule of the Bonnet Monkey (*Macacus sinicus*)." By Charles E. Beevor, M.D., F.R.C.P., and Victor Horsley, B.S., F.R.S. (from the Laboratory of the Brown Institution).

After an historical introduction, the authors proceed to describe the method of investigation, which was conducted as follows. The animal being narcotized with ether, the internal capsule was exposed by a horizontal section through the hemisphere. By means of compasses the outline of the basal ganglia and capsule were accurately transferred to paper ruled with squares of one millimetre side, so that a projection of the capsule was thus obtained, divided into bundles of one millimetre square area. Each of these squares of fibres was then excited by a minimal stimulus, the same being an induced or secondary interrupted current. The movements were recorded and the capsule photographed.

In all forty-five experiments were performed, and they are arranged in eight groups, representing eight successive levels (*i.e.* from the centrum ovale to the crus) at which the capsule was investigated.

Before the results are described in detail a full account is given of previous investigations, experimental, clinical, and anatomical, on the arrangement of the internal capsule.

The anatomy of the part and the relation of the fibres to the basal ganglia are then discussed, and a full description given of each of the groups examined.

The general results are next given at length, of which the following is a *résumé*.

Firstly, the rare occurrence of bilateral movement is discussed, and the meaning of the phenomenon defined. Secondly, the lateral arrangement and juxtaposition of the fibres are considered. Thirdly, the antero-posterior order in which the fibres for the movements of the different segments are placed is described, and that order found to be practically identical with that observed on the cortex, viz. from before back:—

Movements of eyes.
" head.
" tongue.
" mouth.
" upper limb (shoulder preceding thumb).
" trunk.
" lower limb (hip preceding toes).

The character or nature of these movements is set out in a table giving the average localization of each segment. Speaking generally, it may be said that the movements are arranged in the same way as has already been shown by the authors to exist in the cortex (*vide* previous papers in Phil. Trans., 1887, 1888), viz. that the representation of extension is situated in front of flexion for the segments of the upper limb, while for the toes flexion is obtained, as in the cortex, in front of extension.

Numerous tables and diagrams are appended, showing the extent of appropriation of fibres for each movement.

Physical Society, November 15.—Prof. Reinold, F.R.S., President, in the chair.—Mr. Enright resumed the reading of his paper on the electrification due to contact of gases with liquids. Repeating his experiments with zinc and hydrochloric acid, the author, by passing the gas into an insulated metallic vessel connected with the electrometer, proved that it was always charged with electricity of the opposite kind to that of the solution. The electrical phenomena of many other reactions have been investigated, with the result that the gas, whether H, CO_2, SO_3, SH_2, or Cl, is always electrified positively when escaping from acids, and negatively when leaving a solution of the salt. In some cases distinct reversal is not obtainable, but all these seem explicable by considering the solubility and power of diffusion of the resulting salts. Various other results given in the paper tend to confirm this hypothesis. Seeking for an explanation of the observed phenomena, the author could arrive at no satisfactory one excepting "contact" between gases and liquids, and if this be the true explanation he hoped to prove it directly by passing hydrogen through acid. In this, however, he was unsuccessful, owing, he believes, to the impossibility of bringing the gas into actual contact with the liquid. True contact only seems possible when the gas is in the nascent state. Some difficulty was experienced in obtaining non-electrified gas, for the charge is retained several hours after its production, even if the gas be kept in metallic vessels connected to earth. Such vessels, when recently filled, form condensers in which the electricity pervades an inclosed space, and whose charge is available on allowing the gas to escape. Soap bubbles blown with newly generated hydrogen were also found to act as condensers, the liquid of which, when broken, exhibited a negative charge. This fact, the author suggested, may explain the so-called "fire-balls," sometimes seen during thunderstorms; for if, by any abnormal distribution of heat, a quantity of electrified air becomes inclosed by a film of moisture, its movements and behaviour would closely resemble those of fire-balls. A similar explanation was proposed for the phenomenon mentioned in a recent number of NATURE, where part of a thundercloud was seen to separate from the mass, descend to the earth, and rise again. The latter part of the paper describes methods of measuring the contact potential differences between gases and liquids, the most satisfactory of which is a "water dropper," and by its means the P.D. between hydrogen and hydrochloric acid was estimated to be about 42 volts. Prof. Rücker asked if the experiment with zinc and hydrochloric acid could be started in the second stage by having the acid partly saturated with salt. Dr. C. V. Burton thought it probable that contact could be made between a gas and a liquid by shaking them up together in a bottle. In reply, Mr. Enright said the experiment could be started at any stage, and reversal effected as often as desired by adding either acid or a solution of salt to the generating vessel.—Mr. Herbert Tomlinson, F.R.S., read a paper on the effect of repeated heating and cooling on the electrical resistance and temperature coefficient of annealed iron. In a paper recently presented to the Royal Society, the author has brought forward an instance of an iron wire, which when subjected to magnetic cycles of minute range alternately at 17° and 100° C., had its molecular friction and magnetic permeability reduced respectively to about one-quarter and one-half their original values. The present experiments were undertaken to see whether by

such heatings and coolings the temperature coefficient of iron could be brought down to something approaching the number given by Matthiessen for "most pure metals." The wire experimented on was first annealed by heating to 1000° C. for several hours and allowing to cool slowly in a furnace placed at right angles to the magnetic meridian; the process was repeated three times. Afterwards the wire was covered with paper and wound doubly into a coil. This coil was inclosed in a water-jacketed air-chamber, and connected with a sensitive Wheatstone bridge. Thermo-electric and Peltier effects were eliminated by always keeping the galvanometer circuit closed. By repeated heating to 100° C. and cooling to 17° C. for long intervals, the specific resistance at 17° C. was reduced from 11,162 to 10,688 C.G.S. units, after which the operations produced no further change. At the same time the temperature coefficient increased in the proportion of 1 : 1·024. From careful determinations of the resistance at different temperatures, the formula $R_t = R_0(1 + 0·005131t + 0·0000081 5t^2)$ was deduced, whilst that obtained from Matthiessen's results for pure iron annealed in hydrogen is $R_t = R_0(1 + 0·005425t + 0·0000083t^2)$. Taking his own determination of specific resistance of impure iron as correct, coupled with Matthiessen's law connecting the resistances and temperature coefficients of metals and their alloys, the author finds that the specific resistance of pure iron deduced from Matthiessen's results is from 4 to 5 per cent. too high. In conclusion, Mr. Tomlinson expresses a hope that the B.A. Electrical Standards Committee may be induced to determine the absolute resistance and temperature coefficient of the pure metals which are in ordinary use. Prof. Ayrton thought Matthiessen's results were expressed in B.A. units, and hence might appear 1 or 2 per cent. too great. Mr. Tomlinson, however, believed the number he took were expressed in legal ohms. Dr. Walmsley asked for what value of the magnetizing force the permeability of the iron mentioned in the beginning of the paper was determined; to which Mr. Tomlinson replied that they were much smaller than the earth's horizontal component.—Dr. Thompson's paper on geometrical optics was postponed.

Edinburgh.

Royal Society, December 2.—Sir Douglas Maclagan, Vice-President, in the chair.—Prof. Tait communicated a paper by Dr. G. Plarr, on the transformation of Laplace's coefficients.—Mr. A. C. Mitchell read a preliminary note on the thermal conductivity of aluminium. A comparatively rough first experiment shows that this metal slightly exceeds good copper in conductivity.—Dr. John Murray discussed the question of the origin and nature of coral reefs and other carbonate of lime formations in recent seas. He first referred to experiments which have recently been made regarding secretion and solution of carbonate of lime. Carbonate of lime remains are found in great abundance at the sea bottom in shallow waters, but the amount steadily diminishes as the depth increases, until at 4000 fathoms almost every trace has disappeared. This is due to solution, as the organisms slowly fall to the bottom. Everywhere within 500 fathoms of the surface the ocean teems with life. The Greely Expedition was starving within ten feet of abundant food which might have been obtained by breaking a hole through the ice and using a shirt as a drag-net. Dr. Murray then proceeded to discuss his theory of the formation of coral reefs, bringing forward in reply to objections by Dana and others, some recently obtained facts regarding the existence of shallow regions in what is, on the whole, deep water. He showed that carbonate of lime is continually produced in great quantity in warm tropical water by the action of sulphate of lime in solution on effete products. This explains the great growth of coral in tropical regions. The absence of coral on certain shores in tropical districts is explained by the uprise of cold water due to winds blowing off shore. His paper was illustrated by an elaborate series of lime-light diagrams.

Paris.

Academy of Sciences, December 9.—M. Hermite in the chair.—On the nitrification of ammonia, by M. Th. Schlœsing. In a recent communication (September 9) the author described three experiments on the nitrification of ammonia in vegetable humus, tending to prove that this phenomenon is accomplished without any appreciable loss of nitrogen liberated in the gaseous state. He now reports the results of two other experiments, showing that this is no longer the case when a larger proportion of ammonium carbonate is introduced into the soil.—Correction in the tables of Jupiter's movement worked out by Le Verrier, by M. A. Gaillot. Comparing the secular terms of the eccentricity and perihelion of Jupiter's and Saturn's orbits as determined by Le Verrier, Hill (*Astronomical Journal*, No. 204) came to the conclusion that there must be an error of sign in the terms of the second order relating to Jupiter's orbit. M. Gaillot has now gone over the calculations again, and finds that Le Verrier's manuscript is correct, but that, as conjectured by Hill, a misprint of a sign occurs in the published work. In vol. x. p. 242, the sign + appears instead of − before the term $0''·015,554\beta' \cos(\omega - \pi')$.—On the characteristic temperatures, pressures, and volumes of bodies, by M. Ladislas Netanson. These researches tend to show that for every gas there exists an infinite number of characteristic values, t, p, v, which, being adopted as units of the general variables t, p, v, have the remarkable property of eliminating all difference in the characteristic equations of the different gases. The systems usually employed in measuring temperatures, pressures, and volumes, having nothing in common with the intimate nature of the bodies themselves, give rise to differences in the equation $F(t, p, v) = 0$, which disappear when for each body the physicist employs a special system in accordance with its properties.—On the localization of the interference fringes in thin isotropic plates, by M. J. Macé de Lépinay. In studying the exact conditions of the fringes in thin prismatic plates, the author finds a complete verification of the general theory expounded by him in a previous communication (*Comptes rendus*, July 22, 1889). The consequences of the theory may be considered as entirely verified by these experiments.—On the want of accuracy in thermometers, by M. E. Renou. On a recent occasion (July 1) M. Cornu remarked that hitherto these instruments have been liable to an error of from 0°·2 to 0°·3. It is now shown that observations hitherto recorded may give rise to the greatest inconvenience, more perhaps in future than at present. These remarks were supplemented by M. Cornu, who pointed out that errors in the mercury thermometer as great as 0°·2 or 0°·3 occur only in observations taken at considerable intervals of temperature and with instruments not sufficiently tested.—Variations in the mean temperature of the air at Paris, by M. Renou. Twenty years ago the author attempted to show that severe winters return in groups of five or six every forty-one years. This somewhat elastic period is perhaps reproduced better in groups of years than in single years. It also appears that the Observatory of Paris gives a mean temperature higher by 0°·7 than that of the surrounding rural districts—10°·7 as compared with 10°·0 of the Parc Saint-Maur Observatory.—On the observations of temperature on the top of the Eiffel Tower, by M. Alfred Angot. These observations, begun on July 1, are being still continued with a Richard registering thermometer, placed 336 metres above the sea, and about 301 above the ground. Compared with those of the Parc Saint-Maur (50 metres) they show that the normal decrease of about 1° for every 180 metres is greatly exceeded in summer and during the day (means of the maxima), and correspondingly diminished in winter and at night (means of the minima); or there is generally even an inversion in the temperatures, the air being then warmer at 300 metres than near the ground.—Papers were submitted by M. Raoul Varet, on the ammoniacal cyanides of mercury; by M. L. Prunier, on the simultaneous quantitative analysis of sulphur and carbon in substances containing sulphur; by M. E. Guinochet, on an acid isomerous with tricarballylic acid; by M. C. Tanret, on two new sugars extracted from quebracho (*Aspidosperma quebracho*); by M. Arnaud, on carotine, its probable physiological action on the leaf; and by MM. André Thil and Thouroude, on a micrographic study of the woody tissues of native trees and shrubs, prepared for the special exhibition of the Forest Department.—The sealed paper, by M. A. Joannis, on compounds of potassium and sodium with ammonia gas, was opened by the Secretary.

Berlin.

Physical Society, November 22.—Prof. du Bois Reymond, President, in the chair.—Dr. Lehmann spoke on the nature and distribution of the Babylonian metrical system. He expressed his desire to lay before the competent judgment of the Physical Society, the results of his most recent archaeological researches, so far as they are of direct physical interest, and then proceeded to describe the numerical system employed by the ancient Baylonians, explaining that it consisted of a sexagesimal system with decimal subdivisions. The unit of time, the double-

minute, was the time occupied by the sun's rising, measured at the Equinox, and could thus be recovered at any time. It was measured by the mass of water which flowed out of a certain vessel from the instant at which the upper edge of the sun appeared above the horizon to the moment at which his lower edge was exactly touching the horizon. The day consisted of 720 of these units. The unit of length was the ell, which was used in two forms, either as a single- or double-ell; subdivisions used were the foot = ⅓ double-ell, the hand-width, and the finger-length. The unit of weight was the mine, also occurring as single-mine or double-mine. The derivation of units of weight from units of length, as in the modern case of grams and centimetres, was also known, but of course the water used was not distilled and was not weighed at 4° C. The speaker had, however, succeeded in discovering a measuring-scale on an ancient monument dating from the year 2500 B.C., which had enabled him to compare the Babylonian measures with those of our own time. It appeared from this that a hand-breadth = 99·4–99·6 mm.; a double-ell = 994–996 mm.; and the foot = 331–332 mm. He had further discovered several stamped weights, and thus found that the double-mine = 982·4–985·8 grams. The single-mine weighed half as much as the double-mine, but the gold-mine and silver-mine were equal to five-sixths of a single-mine. The royal-mine was 1 per cent. heavier than the gold-mine, and was employed for the payment of tribute. The coinage was based upon the mine and its sexagesimal division. Dr. Lehmann remarked how surprising it is to find that the length of a seconds-pendulum at Babylon is 992·5 mm., and was inclined to advance the hypothesis that the Babylonian unit of length was derived from a seconds-pendulum, the foot being one-third the lengh of the pendulum. He next proceeded to give an account of the spread of the Babylonian mine, and of the Phœnician which was derived from it, as a unit of weight among the civilized nations of Europe. The discovery of an old Roman balance had enabled him to determine that the old Etrurian pound was equal in weight to the Babylonian mine. The Babylonian unit of weight is found not only in Italy and the Mediterranean generally, but also the old Dutch and French pounds and the Russian pood are equal in weight to the mine. The speaker considered it to be quite impossible that in all the above cases we are dealing with a chance correspondence between the several weights. In the discussion which ensued, objections were raised on several sides against the hypothesis that the ancient Babylonians had knowledge of the seconds-pendulum, which had subsequently been lost. On the other hand, it was pointed out by others that the ancients were not improbably acquainted with the plummet, and used it for squaring stones, &c.; and since, further, they employed the double-minute as unit of time, it is not impossible that they were acquainted with the seconds-pendulum. The cause of their not having employed this instrument to supply a time-unit may perhaps be found in their ignorance of any means by which the pendulum could be kept in continuous and uniform motion. In conclusion, the speaker laid stress on the high state of culture which the Babylonians had attained three thousand years B.C., and expressed his regret that a complete blank exists with regard to everything of an earlier date than the cuneiform inscriptions.

STOCKHOLM.

Royal Academy of Sciences, November 13.—On the vegetation of the southmost part of the Isle of Gotland, by Prof. Wittrock.—Myxochæte, a new genus of fresh-water Algæ, by Herr K. Bohlin.—On determinations of the longitude and observations on the pendulum executed in Sweden during the year 1889, by Prof. Rosén.—On a reform in the analysis of gaseous bodies, by Prof. O. Pettersson.—On the invariants of linear, homogeneous differential equations, by Prof. Mittag-Leffler.—The form of the observations on linear differential equations, by Herr A. M. Johanson.—On the case of Poincaré as to the three bodies problem and some analogous dynamical propositions, by Herr E. Phragmén.—On the observations made at the Observatory of Upsala for the determination of the equinoctium in the spring of 1889, by Dr. K. Bohlin and Herr C. A. Schultz-Steinheil.—Definitive orbit elements of the comet 1840 iv., by Herr Schultz-Steinheil.—Study of the infra-red spectra of carbonic acid and of carbonic oxide, by Dr. K. Ångström.—On the action of nitric acid on naphthalin-α-sulphon acid, by Prof. P. J. Cleve.—On naphthalin-1-5, calogene-sulphon-acids, by Herr R. Manselius.

DIARY OF SOCIETIES.

LONDON.

THURSDAY, DECEMBER 19.

ROYAL SOCIETY, at 4.30.—(1) Comparison of the Spectra of Nebulæ and Stars of Groups I. and II., with those of Comets and Auroræ; (2) the Presence of Bright Carbon Flutings in the Spectra of Celestial Bodies: Prof. J. N. Lockyer, F.R.S.—Some Observations on the Amount of Luminous and Non-luminous Radiation emitted by a Gas-flame: Sir J. Conroy, Bart.—On the Effects of Pressure on the Magnetization of Cobalt: C. Chree.—On the Steam Calorimeter: J. Joly.—On the Extension and Flexure of Cylindrical and Spherical Thin Elastic Shells: A. B. Basset, F.R.S.

LINNEAN SOCIETY, at 8.—Intensive Segregation and Divergent Evolution in Land Mollusca of Oahu: Rev. John T. Gulick.—Dictopteris; with Remarks on the Systematic Position of the Dictyotaceæ: T. Johnson.

CHEMICAL SOCIETY, at 8.—On Frangulin: Prof. Thorpe, F.R.S., and H. H. Robinson.—Arabinon, the Saccharon of Arabinose: C. O'Sullivan, F.R.S.—Note on the Identity of Cerebrose and Galactose: H. T. Brown, F.R.S., and Dr. G. H. Morris.

SUNDAY, DECEMBER 22.

SUNDAY LECTURE SOCIETY, at 4.—Algeria and Morocco; some Artistic Experiences (with Oxyhydrogen Lantern Illustrations): Henry Blackburn.

SATURDAY, DECEMBER 28.

ROYAL INSTITUTION, at 3.—Electricity (adapted to a Juvenile Auditory): Prof. A. W. Rücker, F.R.S.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

East Africa and its Big Game: Sir J. C. Willoughby (Longmans).—Measurement of Small Mammals, &c.: Dr. C. H. Merriam (Washington).—North American Fauna, Nos. 1 and 2: Dr. C. H. Merriam (Washington).—Report of the Ornithologist and Mammalogist for 1888; Dr. C. H. Merriam (Washington).—Physical Memoirs, vol. i., Part 2 (Taylor and Francis).—Journal of the Royal Agricultural Society, October (Murray).—Mitteilungen des Vereins für Erdkunde zu Halle A/s, 1889 (Halle).—Proceedings of the Academy of Natural Sciences of Philadelphia, Part 2, 1889 (Philadelphia).—Notes from the Leyden Museum, vol. xi., No. 3 (Leyden, Brill).—

CONTENTS.

THURSDAY, DECEMBER 26, 1889.

RECENT ORNITHOLOGICAL WORKS.

Notes on Sport and Ornithology. By His Imperial and Royal Highness the late Crown Prince Rudolph of Austria. Translated, with the Author's permission, by C. G. Danford. Pp. i.–viii., 1–648. (London: Gurney and Jackson, 1889.)

Matabele Land and the Victoria Falls. A Naturalist's Wanderings in the Interior of South Africa. From the Letters and Journals of the late Frank Oates, F.R.G.S. Edited by C. G. Oates, B.A. Second Edition. Pp. i.–xlix., 1–433. (London: Kegan Paul, Trench, and Co., 1889.)

Index Generum Avium. A List of the Genera and Subgenera of Birds. By F. H. Waterhouse, A.L.S. Pp. i.–v., 1–240. (London: R. H. Porter, 1889.)

The Birds of Oxfordshire. By O. V. Aplin. With a Map. Pp. i.–vii., 1–217. (Oxford: Clarendon Press, 1889.)

The Birds of Berwickshire; with Remarks on their Local Distribution, Migration, and Habits, and also on the Folk-lore, Proverbs, Popular Rhymes, and Sayings connected with them. By George Muirhead, F.R.S.E. Vol. I., pp. i.–xxvi., 1–334. (Edinburgh: David Douglas, 1889.)

The Birds in my Garden. By W. T. Greene, M.A., M.D. (London: Religious Tract Society, 1889.)

NO naturalist can peruse the pages of the handsome volume which contains the record of the sporting journeys of the late Crown Prince Rudolph, without sincere feelings of pity and regret. Here was a young man, whose scientific instincts were of the truest, and for whom, in every way, a splendid future might have been predicted, whose opportunities for the advancement of science were unlimited; and it is most sad that so promising a life should have been cut short by the decrees of fate. One-third of the volume before us is devoted to "Fifteen Days on the Danube," and the narrative affords a striking experience among the varied forms of bird-life which are to be met with on that famous river in April. This is a really valuable sketch of the ornithology of the district, and will be useful to everyone who is interested in the distribution of European birds. The same may be said of the chapters entitled "Sketches of Sport in Hungary" (pp. 391–98), "Miscellaneous Notes on Ornithology" (pp. 409–54), "Ornithological Sketches in Transylvania" (pp. 559–72), and the various "Ornithological Notes" from the neighbourhood of Vienna, &c. Throughout the work the great affection which the author entertained for the birds of prey is manifested, and the "Ornithological Sketches from Spain" (pp. 455–502), are entirely devoted to Raptorial birds, as are also many other chapters in the book. Prince Rudolph thoroughly believed in the races of Golden Eagle (*Aquila chrysaëtus*), which are admitted by A. E. Brehm and other Continental authors. The "Stein" Eagle is generally supposed to be a distinct bird from the true Golden Eagle, and we remember how the Crown Prince overhauled the series of specimens in the British Museum, and pointed out the differences between the supposed races; but when the discussion was over, we could only see that the "Stein" Eagles consisted mostly of immature birds, while the "Golden" Eagle was represented by the older birds in the collection, the alleged difference of habitat being due to the fact that the more lowland country frequented by the "Stein" Eagle was due to their being driven from the mountain eyries by the older birds. The discussion of many points by the Crown Prince on his visit to the British Museum was sufficient to show what a thoroughly sound ornithologist he was. Mr. Danford has done his work as a translator with evident care and a sympathetic knowledge of his subject. Over much of the ground traversed by the Prince the translator has also travelled, and he has evidently fully appreciated the enthusiasm of the author. In the "Ornithological Sketches from the East," wherein are detailed the results of the Crown Prince's journeys in Egypt and Nubia, and afterwards in Palestine, we notice several identifications which strike us as remarkable, and which we believe to be wrong. Was not *Falco feldeggii*, the Lanner Falcon, the species identified by the Prince as *F. barbarus? Acrocephalus turdoides* (p. 513). Surely this is *A. stentoreus? Certhilauda duponti*, "seen in considerable numbers, but only among the bushes and scattered pastures of the islands near the Barrage of the Nile." We should like some confirmation of such an eastward extension of this Algerian bird's range. Generally, however, the nomenclature is good, though slightly Brehmian in character, and Mr. Danford has detected some obvious errors, though the above statements appear to have escaped him.

The late Mr. Frank Oates was a young naturalist who travelled in South Africa in 1873, 1874, and 1875, and died from fever in February of the latter year after his return from the Zambesi. He was a fine specimen of the English traveller, devoted to the pursuit of natural history, and gifted with indomitable perseverance and pluck. His intention on going to South Africa was to penetrate into the interior beyond the Zambesi, and he seems to have regarded his Matabele journey as but a preliminary to more important explorations. The difficulties, however, of getting to the Victoria Falls were very great, and the traveller only succeeded in reaching this desired goal after four attempts and after excessive difficulties and delays. He seems to have won the friendship of Lobengula, and readily obtained the support of the latter for his expedition, but the inferior chiefs and the natives generally were very troublesome. The narrative shows that at the date of Frank Oates's expedition it was by no means easy to get to the Zambesi, especially when the traveller was bent upon collecting *en route*. He gave himself no rest in his pursuits; and the attack of fever which carried him off at the very time when one of his brothers was on the way to join him in the interior was doubtless accentuated and rendered fatal by his untiring devotion to work, which seems to have been one of his most pronounced characteristics. After the traveller's death, a friend, Mr. Gilchrist, went into the interior and brought down all Oates's effects and his natural history collections, and the story of the expedition was originally told by his

brother, Charles Oates. The collections were worked out by different naturalists, and the whole results embodied in appendices which were, moreover, thoroughly well illustrated. Scarcely had the book appeared and met with a cordial appreciation from the public, when a fire at the publishers' destroyed the whole of the unsold copies; and now, after a lapse of some years, Frank Oates's brother and faithful biographer, Charles Oates, has brought out a second edition. Although the necessity of residing abroad has prevented the latter from finishing his labour of love before the present year, the work has lost nothing in consequence. The narrative must always remain of value as a simple record of a naturalist's journey, and the maps of the route are laid down with a fidelity and minuteness not to be exceeded if the traveller had been on a cycling tour instead of in the wilds of Matabele Land, while the lapse of time has enabled the authors of the various appendices to give additional information, to correct errors, and generally to bring their work up to date. Several species undetermined in the first edition have now been identified and described, new plates have been added, and the results as now given to the public by Mr. Charles Oates form a very material and valuable contribution to our knowledge of the natural history of Southern Africa, with the development of which the name of Frank Oates will be for ever connected. All the authors of the various appendices—the late Prof. Rolleston (to whose memoir Mr. Hatchett Jackson, of the Oxford Museum, has added some further information), Prof. Westwood, Mr. Distant, Mr. Olliff, and Mr. Rolfe—seem to have been actuated by a desire to work out the collections intrusted to them for description with the utmost care; and the present writer can only say that the writing of the ornithological portion of the volume was not only a pleasing task, but took the form of an absolute duty to do justice to the memory of the traveller, and to aid Mr. Charles Oates in his fraternal enthusiasm for his brother's fame. Would that every traveller in the Dark Continent attached as much importance to its natural history as did Frank Oates, and that the work of each one was edited by a loving friend, possessed of a desire to place on record the scientific results of the expedition, as has been done in the present work, so that volumes of travel, important as they are, might be rendered still more valuable by biological appendices such as are to be found in Oates's "Matabele Land."

Mr. F. H. Waterhouse, the well-known Librarian of the Zoological Society, has just issued a very useful book, which supplies a great want. The splendid library under his charge has given him the opportunity of personally verifying his references, and many inaccuracies which had been copied from one author to another are herein set right. He has applied himself so diligently to his task, that we believe that about 500 names, of which the origin was obscure, have been traced by the industrious author to their original source, and this fact alone should commend the work to the attention of every working ornithologist. It should be mentioned, however, that Mr. Waterhouse does not pretend to be a practical ornithologist, and he has been dependent to a great extent upon the *Zoological Record* for recent additions. As the volume for 1887 appeared only while the present work was going through the press, several new genera proposed in that year do not find a place in Mr. Waterhouse's book, and therefore the student who interleaves his copy must begin with the *Record* of 1887 if he wishes to have a complete "catalogue" of ornithological generic names.

Of the making of county lists of birds there is apparently no end, and "a good job too!" Little by little, enthusiastic observers are compiling ornithological lists for the different counties of the British Islands, and by these means alone can we hope to obtain a thoroughly accurate knowledge of the distribution of the birds of Great Britain. Mr. O. V. Aplin has long been known to us as an excellent observer, and we hope that the success of his first work, the results of several years of assiduous labour, will encourage him to still more ambitious efforts. The somewhat irregular shape of the county of Oxfordshire, and its generally narrow diameter, preclude the anticipation of a very varied avifauna; but the record of 242 species for the district is by no means bad, and some very interesting notes are given, the principal rarity being the Alpine Chough, of which the only British occurrence has taken place in Oxfordshire, and of which a good plate, by Mr. S. L. Moseley, is given. One of the most inviting features of Mr. Aplin's book is its conciseness. In the capital introduction he gives a very complete account of the configuration of the county and its natural features, all of which can be easily studied with the aid of the excellent map which accompanies the work.

A more ambitious volume is Mr. Muirhead's "Birds of Berwickshire," which is got up in a Bewickian style, as a book matured in such close proximity to Northumberland should be. Mr. Muirhead's book is a complete exemplification of that better style of county record which has been the order of the day during recent years, when a sober statement of facts of distribution and habits has taken the place of strenuous efforts to record rare, and often impossible, visitants. After an introduction which deals with the physical features of the county, aided by a very clear map, the author gives an account of the birds, from the Thrushes to the end of the Accipitres. The accounts of these birds not only contain ample, yet concise, information, but are interspersed with poetry, of a Scottish and local flavour, which successfully combats any notion of dulness, while the folk-lore of the district appears to have special attractions for the author. In some instances, notably that of the Rook, very full details of the breeding-haunts are given in tabular form. It is interesting to note how, on the border-lands, some species have increased in numbers, and have gradually extended their range towards Scotland. The illustrations of nests are drawn by Mrs. Muirhead, and very good they are; and the book is replete with woodcuts by Mr. John Blair, aided by some excellent reproductions of etchings by W. D. M'Kay, R.S.A., and other well-known artists. We trust that in the second volume Mr. Muirhead may be tempted to give us a few details respecting some of the places illustrated in the text, that his readers may share the evident pleasure with which he has illustrated some of the interesting localities of Berwickshire.

Dr. W. T. Greene's little work, "The Birds in my Garden," is an entertaining idyll of a London suburb. Many of the author's experiences agree with our own, and such a book as the present is just the one to encourage a love for the birds which are still to be seen in

the vicinity of London, although, as the operations of the builder are extended in every direction year by year, their number gradually, but surely, diminishes. Where Dr. Greene writes from his own experience, he is always worth listening to, but he has a faith in Morris, which, as might be expected, often leads him awry. He quotes from the Bible about the "Sparrow" on the house-top (p. 13), but the bird alluded to is the Blue Rock Thrush (*Monticola cyanea*), for which cf. Canon Tristram's "Fauna and Flora of Palestine" (p. 31). The illustration on p. 23 is not that of the common Sparrow, but of the Tree-sparrow. At p. 46 he gives a tabular list of characters by which to distinguish the Missel-thrush from the Song-thrush, in which the former bird is said to have "no song to speak of." Evidently, Dr. Greene has never heard a "Storm-cock" in full swing. He does not love the Greenfinch, but this need not lead him to say that the species likewise "has no song." A cock Greenfinch, perched on the top of a tree in the nesting season, and singing to his mate sitting on the nest below, has a charming and varied song, like that of a very powerful Canary. The Whitethroat, of which Dr. Greene appears to know only one species, is placed in the sub-family *Motacillidæ*, and it will surprise many ornithologists to hear that the song of the Chiff-chaff is continued even till late in September (this information is derived from the Rev. F. O. Morris !). The Blackcap does *not* winter in Eastern Africa, and it can hardly be said that the Siskin "rarely nests in this country." We mention these points at the risk of appearing hypercritical, but we recognize in Dr. Greene an author who has the knack of writing good natural history books for the young, and it is therefore the more incumbent upon him to be scrupulously accurate. Let him discard Morris, and stick to Seebohm's "History of British Birds," or to the new edition of "Yarrell." Some pretty illustrations by Mr. Whymper form an additional attraction to his little book.

R. BOWDLER SHARPE.

DESCARTES.

History of Modern Philosophy. "Descartes and his School." By Prof. Kuno Fisher. Translated by J. P. Gordy, Ph.D., and edited by Noah Porter, D.D., LL.D. (London: T. Fisher Unwin, 1887.)

AMONG the many histories of modern philosophy few are so interesting and attractive as that by Prof. Kuno Fisher. The present volume consists of a translation of the third revised German edition, which includes the period of Descartes and his school; and the admirable way in which the author deals with so difficult a subject and his boldness in overcoming it are worthy of the highest praise.

The book is divided into three parts, the first of which is preceded by an introduction to the subject, showing the course of development of the Greek philosophy and that of the Middle Ages, with an account of the early history of Christianity and the Church, concluding with the periods of the Renaissance and the Reformation.

In Part I. we have an account of the early history of Descartes. He was born in the year 1596, a few days before the death of his mother, and was a weak and sickly child. Throughout his childhood he showed a strong desire for knowledge, and it was on this account that his father called him his "little philosopher."

Descartes was among the first pupils in the new school that was started at the Royal palace at La Flèche by Henry IV.; at the age of seventeen he was committed to the care and tutorage of Father Dinet. During his school life he was among the chosen pupils who, on June 10, 1610, solemnly received the heart of the king, which, by Henry's will, was to be buried in the church of La Flèche.

While going through a two years' course on philosophy, he became completely fascinated by mathematics, and was thereby incited to make a further study of it; and later on in life, seeing the true spirit of mathematics as a method of solving problems, he began by algebraical equations to solve geometrical problems, and thus to him is due the discovery of analytical geometry. On the completion of his school career, the state of his mind may be gathered from his own words—" . . . I found myself involved in so many doubts and errors, that I derived no other result from my desire of learning than that I had more and more discovered my own ignorance."

The next few years of his life were spent in military service in Holland and Germany, after which, at the age of five-and-twenty, he travelled for nine years; to him his travels were studies in the great book of life, and during them he "did nothing but wander now here, now there, since I wished to be a spectator rather than an actor in the dramas of the world." The last period of his life consisted of the development and publication of his works, and the founding of a school of philosophy, concluding with his illness and death during his stay in Stockholm, to which place he was invited by Christina, then Queen of Sweden, who, being deeply interested in his works, found the difficulties in his system could better be explained by Descartes himself than by anyone else.

Although the philosophy of Descartes treats of the whole realm of Nature, we will here touch only upon those parts that are interesting to us from the scientific point of view. Not by any means the least important is his attempt to explain the origin of the world by purely mechanical laws. He bases his theory on the rest and motion of solid and liquid bodies, and the influence of the latter upon the former. Before entering upon this hypothesis, the mechanical principle of his explanation of Nature is first brought before us. He treats motion as a mode of extension, and explains it as the "translation of place (transport) of one part of matter or of one body from the vicinity of those bodies which directly touch it, and are considered at rest, into the vicinity of others."

The causes of motion are next dealt with, showing us that all changes are due to outward collision, and that since space is by no means empty, but is full of bodies moving in every direction, we may get a great number of collisions, the various possible results of which he then goes on to discuss. According to his principles, then, bodies are quite destitute of force, excepting that of resistance; changes in the material world are due to external collisions, and motion, therefore, is due to impacts. Comparing the views of Descartes with those of Galileo and Newton, we cannot do better than quote

what the author says on this point:—"Gravity is regarded as . . . . an original property of a body belonging to it of itself. Descartes denies it. Therein consists the opposition between Galileo and Descartes; with gravity he was obliged to reject gravitation and the power of attraction. Therein consists the subsequent opposition of Newton and Descartes; he is, therefore, compelled to deny the so-called central forces, as well as every *actio in distans*."

The two essential pre-suppositions of his hypothesis are the "immeasurableness of the universe and the nullity of empty space. From the first it follows that the universe is not a spherical body, and does not consist in concentric spheres to which the stars are fastened; that there is, therefore, no celestial sphere beyond the farthest planet (Saturn), and that the sun does not lie in the same spherical superficies. From the second, it follows that the spaces of the heavens are filled with fluid matter, and that the heavenly bodies are surrounded by the latter, and subject to its influences."

Descartes supposes the earth to be completely surrounded by this fluid, and "acted upon uniformly in every direction, or carried along by its current, as a solid body in liquid matter. The planets follow also the same rule. Each is at rest in the heavens in which it is, and all the change of place which we observe in those bodies follows from the motion of the matter of the heavens which surrounds them on all sides."

By supposing, again, that this flow of the matter, which surrounds the earth and planets, describes a current "spinning round like a vortex," with the sun in the centre and the earth and planets going round it; he obtains, without considering their weight and attraction, a method by means of which their various motions may be explained. He compares this "vortex" motion of the matter with eddies of water, "as waters when they are forced to a reflux form an eddy, and draw violently within their rotary motion, and carry along with them, light floating bodies, as, for example, straws; as then these bodies, seized by the eddy, turn about their own centre, and those nearer the centre of the eddy always complete their rotation earlier than the more distant ones; as, finally, this eddy always, to be sure, describes a circular figure, but almost never a perfect circle, but extends itself, now more in length and now in breadth, wherefore the parts at the periphery are not equally distant from the centre,—so one can easily see that the motion of the planets is of the same character, and that no other conditions are necessary to explain all their phenomena."

Thus Descartes agrees with Copernicus and Galileo with regard to the heliocentric motion of the earth and planets, although basing his hypothesis on different mechanical laws; he also teaches that the earth is a planet, and rotates on its axis daily, and revolves yearly in an elliptical orbit round the sun.

The author then tells us how Descartes, after the completion of his hypothesis, postponed its publication, on account of the fate of Galileo, and how he (Descartes) expressly stated at the end that "his hypothesis not only may be, but in certain respects is, false." Although he denied the movement of the earth, it was only in a sense that followed from his idea of motion which he applied to the heavenly bodies; for, with reference to the other bodies in the heavens, it does move, but is at rest in relation to the fluid matter around it, or, as the author says, "it moves exactly as a man who is asleep in a ship, while it takes him from Dover to Calais."

In conclusion, we must add that the work of both translator and editor has been honestly done, though, as the above quotation shows, the style of the translator is susceptible of improvement, and that this volume will form a valuable addition to the libraries of students of moral philosophy. To the readers of such a work as this, consisting as it does of so many historical facts, an index is imperative, and we hope in future editions to see one inserted.

W. J. L.

A TEXT-BOOK OF ORGANIC CHEMISTRY.

A Text-book of Organic Chemistry. By A. Bernthsen, Ph.D., formerly Professor of Chemistry in the University of Heidelberg. Translated by George McGowan, Ph.D., Demonstrator in Chemistry, University College of North Wales, Bangor. (London: Blackie and Son, 1889.)

THIS work furnishes an excellent elementary account of the principles of organic chemistry. An introduction treating of the general theory of organic compounds, including the subjects of constitution, isomerism, physical properties, &c., is followed by the detailed description of the various classes of compounds and their relations to one another, the fatty compounds being first discussed, and then those belonging to the group of aromatic substances and to the pyridine group. The treatment of the various compounds in "series," all the hydrocarbons of the fatty series—paraffins, olefines and acetylenes—being, for example, fully described before any of their halogen derivatives or of the alcohols are discussed, cannot be commended from the point of view of the novice to the science, for whom the book is avowedly designed. This evil is, however, largely compensated for in the present work by the clear language invariably employed, and more especially by the frequent introduction of semi-diagrammatic tables showing the connection between various related series, such, for example, as the glycols, hydroxy-acids and dibasic acids.

The description of the aromatic compounds, prefaced by a short account of the benzene theory, is grouped about the typical hydrocarbons, benzene and its derivatives being first treated, then diphenyl with its derivatives, triphenyl-methane and its group, naphthalene, &c. Mere description of compounds is sternly and consistently avoided, its place being supplied, whenever possible, by tabulated statements, showing at a glance both the chemical and physical relations of a whole series of derivatives. These tables are a distinguishing feature of the book, and impart to it a clearness and conciseness which will render it welcome to every student.

Abundant references are provided to the original papers concerning subjects which fall without the elementary scope of the work, such as, among many others, the diazo-derivatives of the fatty series, the syntheses of glucosides, and the grouping of atoms in space, which last is treated in language which will perhaps be apt to mislead, and scarcely receives a degree of attention commensurate with its importance.

The translator has performed his work with great success, and he is to be congratulated on the almost complete absence of printers' errors, which so often mar the pages of works of this class. It is to be regretted that he has in some instances neglected to adopt the nomenclature employed by the Chemical Society, since uniformity of usage in this respect is greatly to be desired. An excellent index forms a fitting conclusion to the work, which is sure to take as high a place among the elementary text-books of organic chemistry in the English language as it has already done in the Fatherland.

OUR BOOK SHELF.

The Viking Age; the Early History, Manners, and Customs of the Ancestors of the English-speaking Nations. By Paul B. Du Chaillu. Two Vols. 1366 Illustrations, and Map. (London: Murray, 1889.)

THE author of this work has persuaded himself that the invaders who conquered and settled in Britain after the departure of the Romans were not, as we have been taught to believe, Low Dutch tribes, but Norsemen. It is unfortunate that he should have hampered himself in his researches by so arbitrary a theory. Of course, no one disputes that there is a strong Scandinavian element in England; the fact has always been perfectly well understood by historians, and has received from them due attention. But to say that the English people are wholly or mainly descended from Scandinavians is to advance a proposition opposed to all the most vital evidence we possess on the subject. The evidence of language alone would suffice to dispose of so crude a doctrine. Mr. Du Chaillu has not approached the consideration of the question in a scientific spirit, and has too lightly brushed aside the difficulties in his way.

He has tried to give an account of the ideas, customs, manners, and institutions of the ancient Scandinavians; and we need scarcely say that there are some lively and attractive passages in his chapters on these subjects. From his book, English anthropologists will learn that there is valuable material for them in the old northern laws and Icelandic Sagas. They will, however, be unable to make use of his translated extracts, because he does not attempt to estimate the date and weight of the documents used, late forged Sagas being treated precisely as authentic early poems or contemporary histories.

The work has, in fact, no scientific value. It will amuse "the general reader," but it is unsuitable for serious students. To the archæologist it may serve as a rough index to the chief finds made in the three Scandinavian countries; but even for this purpose he will need to refer to the original plates and cuts from which the illustrations in these volumes are more or less happily reproduced. This will be obvious to anyone who studies the originals in the papers of Montelius, the Proceedings of the Stockholm Congress, 1874, the splendid Copenhagen Museum Catalogues, or the "Aarbøger for Nordisk Old-kyndighed og Historie." F. Y. P.

A Glossary of Anatomical, Physiological, and Biological Terms. By T. Dunman. Second Edition. Edited, and supplemented with an Appendix, by W. H. Wyatt Wingrave, M.R.C.S. (London: Griffith, Farran, Okeden, and Welsh.)

IT is now eleven years since the first edition of this book appeared. The senior author outlived its publication by but a short period. The editor of the present edition has left its pages unaltered, and has taken upon himself to add thereto (in the form of an appendix) twenty-five pages, embracing some 400 physiological and morphological terms, to the paucity of which, in the original edition, he directs attention. Many of his supplementary words are superfluous, others are obsolete, and by no means a few are either insufficiently or inaccurately explained. The original edition was by no means free of like defects: in it we read, by way of example, that the "*Sepiostaire*" is "the only representative of an endoskeleton in the cuttle-fishes"; that the "*Septum lucidum*" is "the partition which separates from each other the lateral ventricles of the brain"; that by "*Schizocœle*" is meant "a term applied to the peri-visceral cavity of the Invertebrata, when formed by a splitting of the mesoblast of the embryo." The present editor, while preserving the above and many other similar misstatements, has, in turn, shown himself wanting in power of accurate definition of fundamentals. This is seen, for example, in his renderings of "*Endomysium*," "*Inhibition*" (defined as "checking or controlling influence, exercised by a nerve-centre over some subordinate organ or process"), "*Metabolism*," "*Meckelian bar*," and *Negative variation*" (which, we are told, embraces "changes in the natural nerve or muscle currents which occur during contraction"). The little volume has hitherto recommended itself to students chiefly by its compactness. There has always characterized it a want of expressiveness and of finish. A single instance will suffice: "*Glomerulus*" has all along stood, and still stands, as "the small ball of capillaries in the Malpighian capsules of the kidney." It is the first duty of an editor of a new edition to rectify original defects; and, until that shall have been done, he has no right to add supplementary matter. The volume, as it now stands, must be speedily revised, if the recommendation of experienced teachers is to be looked for; and it is upon the same that it can alone maintain its honoured position.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Acquired Characters and Congenital Variation.

BEING one of those who do not believe that either the theory of Darwin or the theory of Lamarck gives any adequate or rational account of the "origin of species," I am always glad to see any controversy which pits the one of them against the other. It is by such controversy that the weak points of each are best exposed. But I now write in the interests of peace and conciliation. Prof. Ray Lankester seems to me to be much too belligerent. I see no necessary antagonism between "congenital variation" and the transmission of "acquired characters." If an acquired character affects the whole organism, and especially the reproductive elements, then its hereditary transmission would perfectly reconcile the two conceptions. And this is probably the universal fact. I have no doubt of the hereditary transmission of acquired characters. So far is it from being "unproved," it is consistent with all observation and all experience. It lies at the foundation of all organic development. But it implies no denial of "congenital" causes. It is very probable that every "acquired character" is necessarily correlated with some physical modifications in organic structure, and that it is only transmitted to progeny through, and by means of, this physical modification.

This being so, the question arises, Why is it that the idea of acquired characters becoming hereditary is so fiercely opposed by extreme Darwinians? Is it the mere jealousy of an exclusive worship—the mere dislike of the great name of Lamarck being mentioned, even in the same day, with the name of Darwin? It is partly this, no doubt. But it is something more. It is jealousy of any conception which tends to break down the empire of mere fortuity in the phenomena of variation. Darwin himself is not wholly responsible for this feeling. He expressly guarded himself against the interpretation which has been affixed to his language about "accidental" variation. He knew well

enough that variations must be governed by some law. But as we are absolutely ignorant what that law is, he thought it allowable to make provisional use of the word accidental. But the "neo-Darwinians" (as Prof. Ray Lankester calls them) are not content with this dethronement of their idol, Fortuity. The supreme and everlasting rule of pure accident is their creed and worship. Hence comes Prof. Ray Lankester's simile of the kaleidoscope, by which he illustrates the genesis of "new characters" in organic life. There is, he indicates, no more connection between those "new characters" and their origin in the parent, than there is between the new patterns which tumble in a kaleidoscope and the tap upon the tube which shakes them out.

There is no argument so false as a false analogy. And this is a case in point. Every illustration or analogy must be false which confounds mere mechanical arrangement with organic structure. They are not only different, but they are different in kind. Neither mechanical aggregation, nor mechanical segregation, can possibly account for the building up of organic tissues. To attempt to account for such structures by causes similar to those which determine the arrangement of tumbling bits of glass, is even more irrational than it would be to account for the structure of a great cathedral by explaining to us how its bricks or its stones were made. There is one grand peculiarity in all organic structures which all such illustrations are framed to conceal. That grand peculiarity is this—that they are all made for work, for the discharge of some function. They are where they are not merely because somehow they have been put there. But they are what they are, and where they are, because they have some given work to do. But more than this: they all pass through stages of development in which their work cannot as yet be done. In all these stages, that work lies before them in respect to time, and behind them in respect to adaptation. They are all of the nature of an "apparatus." This is the word which the profound but unconscious metaphysic of human speech has invented for them. It is the word chosen by natural selection, and, as such, it ought to secure the homage even of Prof. Ray Lankester himself. The idea, however, comes before the word—shapes it, and inspires it—just as the needs of function, and the organic necessities imposed by inorganic laws, have shaped and inspired the growth and development of every organic apparatus.

I am very glad to see that under the stress of controversy the Professor admits—and even hotly denies that it has ever been doubted—that natural selection cannot account for the preexistence of the structures which are presented for its choice. And not only must selected organs exist before they can be chosen by natural selection, but they must have been already sufficiently developed to possess some functional activity. This was my contention thirty years ago, and to this day I have always found it either denied or evaded by the whole ultra-Darwinian school. I rejoice to see it now admitted as unquestionable. "Natural selection can account for the origin of nothing"—so says Mr. Cope. The Professor indignantly replies: "How can Mr. Cope presume to tell us this? Who has ignored it? when? and where?" So ends a long and a hard fight. The enemy not only lays down his arms, but denies he has ever carried them.

ARGYLL.

Who Discovered the Teeth in Ornithorhynchus?

IT is almost superfluous to add anything to Prof. Flower's reply (p. 151) to Dr. Hart Merriam. In justice, however, to Mr. Poulton, it ought, I think, to be stated that he fully refers to Home's paper in the Philosophical Transactions. In the *Quart. Journ. Micr. Sci.*, vol. xxix. p. 27 (a paper to which Dr. Hart Merriam alludes as though he had read it) Mr. Poulton, describing the horny plates of Ornithorhynchus, writes as follows: "Home (Phil. Trans., 1802, p. 71) correctly describes these horny plates as differing 'from common teeth very materially, having neither enamel nor bone, but being composed of a horny substance only embedded in the gum,'" &c. I observe too, with great interest, that in the same paper Home makes use of the expression (p. 70) "the teeth, *if they can be so called.*" On p. 28 Mr. Poulton quotes in full the passage from Owen given by Prof. Flower. Perhaps Dr. Hart Merriam does not accept Owen's correction of Home's hypothesis. It is hardly necessary to point out that the teeth which Mr. Poulton describes (p. 15 *et seq.*) under the headings (1) tooth papilla; (2) dentine; (3) enamel; (4) inner epithelium of enamel organ; (5) stratum intermedium of Hannover; (6) middle membrane of enamel organ; and (7) outer membrane of enamel organ, *must* be very different from those which Home calls "cuticular," and further qualifies as in the sentence which I have quoted.

Comparison of Home's figures with Mr. Oldfield Thomas's (Proc. Roy. Soc., vol. xlvi. pl. 2) renders it highly probable that the true teeth of Home's younger specimen had only recently dropped out from the horny plates; the dimensions given by the two authors being almost identical. But Home's description is perfectly definite, and no hint whatever is made to true teeth situated upon the horny plates such as those described and figured by Mr. Oldfield Thomas. The length of the skull of Home's specimen, as given in his figure, is 71 millimetres, while that of Thomas's female specimen is 65 millimetres; the male is slightly larger. Probably, therefore, Home's specimen was considerably older than Thomas's, and had lost the true teeth for some little time.

The only conclusion at which I can arrive is that Dr. Hart Merriam did not read any of the three papers bearing on this subject with sufficient care and attention to enable him to fully understand the facts ascertained by their respective authors, if indeed he proceeded further than the introductory remarks prefacing Mr. Oldfield Thomas's communication to the Royal Society.

OSWALD H. LATTER.

Anatomical Department, Museum, Oxford, December 20.

Galls.

IN answer to Mr. Ainslie Hollis, I should like to observe that, in my opinion, the theory of natural selection is *not* "seriously assailed by investigations into the formation of galls by insects." On the contrary, in reply to what appeared to be a challenge from Mr. Mivart, I pointed out the manner in which natural selection might here be fairly supposed to have operated. But, while doing this, it appeared desirable to add that the case is a highly peculiar one. If galls were merely amorphous tumours, or even if they presented but as small an amount of specialization for the benefit of the larvæ as is presented by animal tissues for the benefit of their parasites, the case would not be so peculiar. But the degree of morphological specialization which the "pathological process" presents in the case of some galls—and this, of course, for the exclusive benefit of the contained parasites—is very remarkable. And although I doubt not that it is but a higher exhibition of the same principles as obtain in the case of animal tissues and their parasites, it is a case of much greater interest from the Darwinian point of view. For, if the explanation given in my last letter be accepted, the facts show how enormous must be the power of natural selection in building up adaptive structures, seeing that it can do this in so high a degree even when working, as it were, at the end of a long lever of the wrong kind—*i.e.* acting *indirectly* on the vegetable tissues through the benefits thereby conferred on their animal parasites. I am not aware that there is any other instance of "symbiosis" where so high a degree of adaptive specialization is presented by one of the "partners" for the exclusive benefit of the other.

GEORGE J. ROMANES.

London, December 13.

MR. W. AINSLIE HOLLIS has involuntarily misrepresented me as saying that the theory of natural selection can be "seriously assailed" by investigations respecting galls. I said, indeed (NATURE, November 14, p. 41), that it would be "very interesting to learn how" natural selection could have caused them; but I was careful to add that doubtless an explanatory hypothesis was ready to hand. I do not myself believe they were so caused; but if they were not, they would none the less, like almost all biological phenomena, be explicable by an unlimited use of gratuitous hypotheses concerning physiological correlations and imaginary ancestors.

I confess I do not see that calling them "pathological" (an epithet I certainly would not deny them), and comparing them with inflammatory renal foci due to Bacilli, will explain them, unless it be affirmed that pathological conditions favourable to parasites are always due to the action of "natural selection" on the parasites themselves—an affirmation which appears to ask too much.

Herr Wetterhan's argument from *symbiosis* sins against natural selection itself. For that theory requires that, in the arduous and incessant struggle for life it supposes, any prejudicial growth should, in time, be eliminated unless carrying with it some preponderating advantage. The insect and the plant are

not "partners," for the latter does not participate in the gain of the former. How, then, on symbiotic principles, can "natural selection" have been the means of producing a growth which, though important, if not necessary, to the animal symbiont, is more or less prejudicial to the symbiont vegetable organism?

There can, of course, be no doubt, as Mr. McLachlan says, that the various peculiarities of gall-structure "could be" explained "on purely physiological grounds if carefully studied;" but that "natural selection" will suffice to explain them, seems to me by no means equally free from uncertainty.

St. George Mivart.

Hurstcote, Chilworth, December 13.

The Permanence of Continents and Oceans.

I can find no flaw in the reasoning on the dynamical question of the permanence of continents and oceans, in Mr. Starkie Gardner's letter in Nature of December 5 (p. 103), by which he endeavours to show the universal "tendency for deep oceans to become deeper, and for mountain chains to grow into higher peaks." But when he says it is opposed to no known facts, I wish to ask how it is to be reconciled with the fact of the general distribution of marine deposits over the face of the earth, so that every part of what is now land appears to have once been ocean?

I fully concede that the change of ocean spaces into land spaces is an extremely slow process, taking, probably, millions of years, but it seems to me that it must have occurred, though I cannot suggest through what agency.

Belfast, December 14. Joseph John Murphy.

Does the Bulk of Ocean Water Increase?

Mr. Jukes-Browne (Nature, December 12, p. 130) admits that "if the area of the land were larger, and the depth of the oceans less," in early geological times, a further inference must be drawn—"that the bulk of the ocean water was less then than it is now."

So far we are in agreement; indeed, we could scarcely be otherwise, as the proposition admits of complete demonstration. When, however, Mr. Jukes-Browne proceeds to give his reasons for holding that the bulk of ocean water *was* less in early times than now, he enters upon a more controversial subject.

I am familiar with the arguments he urges partly on the authority of Mr. Fisher, and have to some extent discussed them in chapter xii. of the "Origin of Mountain Ranges." I desire, however, to point out a further objection that when stated will, I think, appear extremely obvious.

According to Dr. George Darwin and many other astronomers who follow him, our satellite, the moon, was once an integral portion of the earth, having been thrown off when the earth was in a molten condition. If this theory be correct, it is a fair assumption that the magma out of which the moon has consolidated was composed of matter similar to that of our earth. Even if their relations were never so intimate as this, I think most physicists and astronomers will admit a similarity of material constitution of the two spheres.

If then volcanic action on the earth is, as Mr. Jukes-Browne contends, accompanied by a separation of water initially contained in the magma, and its condensation on the surface in such quantities as to materially increase the bulk of ocean water, why has not the same effect followed volcanic action on the moon? Why, in fact, do we not see oceans on the surface of the moon instead of a dry and desert waste of volcanic rings, mountain protuberances, and arid plains? In face of this great fact it appears to me that ingenious arguments as to the amount of water contained in the fluidal cavities of granite, which most geologists think is explicable by percolation, have not much weight.

At all events, it seems a reasonable question to ask why oceans should be supplied with water from the perspiring pores of mother earth, while her offspring, the moon, is so dry as to have absorbed into herself all evidence of any aqueous envelope that may have formerly existed.

T. Mellard Reade.

Park Corner, Blundellsands, December 14.

A Natural Evidence of High Thermal Conductivity in Flints.

A rather curious effect of the recent frost attracted my attention in the gravel foot-paths leading over Addington Hill, near Croydon, on the beautifully bright day of the 1st inst. The clear nights and frosty air of the closing week of last month had been productive of continued low temperatures in that locality, and the result observed was that the flint pebbles, which in neighbouring gravel-beds and here and there on the paths, are of the size of hens' eggs, and remarkably well rounded, had, in places, sunk in the frozen clunch or clay-earth of the foot-paths, and in the peaty ground or turf beside the paths, as it appeared, like filberts shrunk and resting at the bottoms of their shells; or else as if the pebbles' earthy moulds had, by expanding upwards, left such a large vacuity above each stone, that the tops of some of the large ones, instead of being level (as at first they must have been, by the appearance of the moulds) with the surface of the ground, were now, in a somewhat turfy place, about as much as half an inch below it. The physical enigma which hereupon offered itself for elucidation was, how the pebbles could remain at the much lower level, while such a considerable expansion upwards had been brought about by freezing in the moist earth immediately surrounding them; and this problem had certainly, in looking at the thickly-clustered cavities in the frozen ground, at first a very paradoxical appearance.

But if the question how the inclosing cavities of moist earth round flint pebbles which are nearly embedded in it, are distended upwards so curiously by a strong frost's predominance, has presented, it may be, to some of your readers who may have noticed in similar conditions a similar appearance, as it at first did to me, a subject for rather puzzled contemplation and conjectures, it will be worth pointing out, perhaps, that there is a well-ascertained thermal property of siliceous rocks and flint, of which it seems not improbable that this not unfrequently occurring action of a strong frost, in such conditions, may really be an interesting illustration.

Among a series of about a hundred different descriptions and varieties of commonly occurring rocks whose thermal conductivities were experimentally determined by a Committee of the British Association in the years 1874–78, it was found that such entirely siliceous ones as quartz, flint, and pure siliceous sandstone, &c., so much surpass all other ordinary rocks in their rates of transmitting both heat and temperature, that in flint pebbles these conducting powers are, for example, about four or five times as great as in damp sandy mould, or in wet clayey earth.

Instead of the layers of cold temperature, therefore, produced in wet pebbly ground by continued frosty winds and radiation, proceeding in plane levels downwards from one depth below the surface to another, large flints exposed in it must grow cold very quickly through their whole substance, and must freeze the wet earth under them almost as soon as the soil's surface-layer round them is beginning to be frozen. The effect of this freezing process's expansion, it seems evident, will hardly be so much to raise the pebbles and the earth's exposed surface upwards very differently from each other, by the frost's nearly equal action on them both, as, during the frost's continuance, to force up towards the surface a large superfluity of soft earth from between the bedded stones, carrying the cast or mould of the stone's upper sides, itself to some height above them. We would require, perhaps, as an aid to this interpretation of the process, to regard the congelation round the stones, as rooting them down, perhaps to lower-lying ones, so that the upward thrust of the extruded earth may not be able to dislodge them, but can be effective to raise up their frozen caps; but some such supposition as this does not appear to be a very impossible conjecture. By this recourse to the pre-eminent thermal conductivity of flints above that of moist turf and clay, in which they are embedded, it seems at least not impracticable to give a somewhat intelligible explanation of the frozen ground's abnormal elevation round them, lifting the moulded caps of earth-covering off their upper sides until their roadside clusters present the curious appearance of shrunken petrifactions of some nest of fossil yolks in half-empty egg-shells.

It is, indeed, true that when by long continuance of a frost the sodden earth may have become entirely penetrated and frozen by it to some considerable and tolerably even depth (we may suppose) below a layer of embedded flints, it should be noticed, to simplify the process's consideration, that the form which the frozen ground will then have acquired between and round the flints could be nowise affected in the end by any various shapes, plane or contorted by irregularly formed and differently conducting solid bodies in its course, wherewith the tract of

reezing temperature after entering the ground approaches by stages of quick or slow rates, in different parts, towards the supposed nearly even depth at last, if we might only presuppose that, because of the endless material obstruction to its motion in any horizontal direction, no channels for the earth's lateral expansion in freezing should subsist; but that in all places and in all conditions where the freezing happens, the only line of escape of the earth's increase of volume should be vertically upwards towards a direction where no insuperable forces are, at least, opposed to it.

Were this assumption of upward reliefs only of all of the expansions a really true and valid one, every vertical fibre of the wet earth's mass would behave in freezing quite independently of every other one, and would take up its fully expanded length at last, no matter at what times and in what order congealing overtook its individual portions. A stone, in this supposition, just embedded in the ground, would have its lower half lifted at last in its socket, and the upper half of the socket lifted off the stone (whether its thermal conductivity is great or small), to the height, in either case, of a water-column's change of length by freezing, whose initial height is but half the vertically measured thickness of the round embedded stone—that is to say, about one-eleventh of an inch for a stone 2 inches in diameter, instead of nearly half an inch, which was about the depth of the settlement, in some of the large-sized flint stones, which was actually observed.

To return to the reality, however, from this artificial supposition, the actual course of the expansions, and the effects produced by the freezing dilatations must, no doubt, be very different. Supposing that the flint-stones, by their good thermal conductivities, soon become covered with a thickening coat of frozen earth, flow of the soft, unfrozen earth between them will really spring up and be maintained by direct outward expansions from the stones of the icy coats surrounding them. On account of the firm rigidity of the exposed earth-surface, to which the stones themselves must soon become fast fixed, the resultant flow of soft earth from between the stones, instead of finding an upward path the easiest, will rather choose a vertically downward one for its escape from its confinement, and lift the stones and icy covering together, rather than seek by an upward course to break through the latter. Yet this last effect may also perhaps occur to some extent, raising the frozen earth-caps in some measure off the stones' upper sides, and stretching them, it may be, a little upwards, so as to leave between them and the stones clear empty spaces. That this last effect must be only a secondary and inconspicuous one, however, seems to be pretty obvious from this passingly essayed, and as it now appears all too uselessly pursued and desultory *aperçu* of the frost's real mode and process of expansive action.

Regarding the peculiar structures, in fact, altogether from another point of view, and rejecting the imperfect explanation which any one of these presumed congelation processes might at first have been supposed to furnish, of the curiously sunken-looking assemblages of the wayside pebbles, an exactly opposite interpretation of their semi-interred condition seems, perhaps, indeed, to afford a more satisfactory and likely explanation of it, than the expansive effects of frost in the moist earth were ascertained and shown to have any capabilities and physical resources for. The warmth of the sun, or of wind and rain in some thawing daytime temperature of the generally frosty week, may in short be supposed (which the weather-table of the week, on the 26th and 27th ult. confirms) quite plainly and certainly enough, in consequence of the flints' good thermal conductivities, to have melted and shrunk again to its natural dimensions the hard frozen earth under them, without lowering the level equally of the badly conducting frozen earth surrounding them. Alternate days of thaw and nights of frost would, by progressive stages which can be easily traced out and understood, tend quite naturally to exaggerate this difference. Thus in another way, but complementary to and at returning times just fitly supplemented by that first supposed, the problem which the winter scene presented is, still more simply and clearly than before, seen to be solved quite truly and correctly by the relatively high thermal conductivity of the rounded flints as compared with that of the hard frozen earth in which they are enveloped.

This gradual subsidence, therefore, of flint stones during alternate frosts and thaws, into frozen earth, by consolidation and lateral expansion, followed by liquefaction and vertical contraction of the water in the earth beneath them, is, it would seem that we may reckon it accordingly, a phenomenon on land just analogous and similar to the familiar thermal process which small stones scattered on a smooth frozen glacier-field display in summer-time, by intercepting the heat of the sun's rays, and by sinking to the bottom of the deep water-holes which they thus scoop and delve out for themselves, wherever they happen to have found a lodgment in the naked ice.

A. S. HERSCHEL.

Observatory House, Slough, December 9.

Foreign Substances attached to Crabs.

AT the last meeting of the Linnean Society I exhibited a number of crabs and certain shells of the genus Phorus having various foreign substances attached to them, about which it is desirable that more should be known. Some of the crabs manage to fasten bits of sea-weed to the hairs on the carapace and legs; Polyzoa, Balani, Serpulæ, &c., in their earlier stages fasten themselves on others; a crab of the Indian Seas—*Camposcia retusa*—is sometimes completely covered on every part with sand, small shells, and bits of sea-weed—Corallina chiefly. These could only be attached by some adhesive matter, but whence derived? *Dromia vulgaris* is occasionally found with a sponge extending over the carapace and almost completely hiding the animal. The species of this genus have the two hinder pairs of legs much reduced, flattened, and lying close to the back, and this is assumed to be an adaptation for the purpose of retaining the sponge. Out of a number of specimens dredged in the Bay of Naples, I recollect only getting one with a sponge on it, and that very soon shrivelled up, leaving a leathery-looking substance attached to the base of the carapace, not held by the legs apparently.[1] Two crabs—*Æthusa mascarone* and *Dorippe lanata*—having similarly reduced hind-legs, but directed upwards, seem much better adapted for retaining a foreign substance, which, however, they are not known to do. In a Mauritian crab—*Dynomene hispida*—the hind pair only are reduced, but to such an extent as to be merely rudimentary and incapable of any use. *Paramithrax barbutus*—a New Zealand crab—has, like some others, hooked hairs, but in the specimen exhibited they appear to be free of any foreign substances, although many small fragments of an uncertain nature appear between them.

In Phorus a strong cement only could hold on those large and heavy substances—shells, stones, &c.—completely covering the shell, as in *P. agglutinans*. I have not seen any account of their *modus operandi*, but, as the animals have a long proboscis, it is possible that that may be the organ employed, but it is difficult to believe that it would be able to lift any large substance, or that it could reach the top of the shell. Another difficulty is that they must cast off, from time to time as they grow, the smaller substances, to replace them by larger ones. There is one Phorus, however—*P. calyculatus*—in which small shells imbed themselves at short intervals along the whorls, leaving the greater part of the shell uncovered; these little cup-shaped depressions are marked inside, as far as the mouth of the shell will permit them to be seen, by corresponding protuberances. This would seem to indicate a certain softening of the shell at one time or other.

I do not see where protection comes in, in any of these cases.

December 14. FRANCIS P. PASCOE.

A Marine Millipede.

IN the hopes of arousing the interest and the energies of British entomological collectors, "D. W. T.," in a short notice on p. 104 of the present volume of NATURE, draws attention to the recent discovery in Jersey, by Mr. Sinel, of that remarkable marine centipede *Geophilus* (*Schendyla*) *submarinus* (not *submaritimus* by the way), of Grube.

Those who observed this notice, and are interested in the fauna of Great Britain, may be glad to hear in addition that more than twenty years ago a number of specimens of this then undescribed species were taken by Mr. Laughrin at Polperro on the south coast of Cornwall. These specimens, which were presented to the British Museum in 1868, were found associated with *Linotænia maritima* (Leach)—also a marine centipede—

[1] Bell, in his "British Crustacea" (p. 371), states having received "numerous young specimens from Sicily, every one of which had the carapace entirely covered with a sponge, which had grown over it, concealing even the two hinder pairs of legs, which were closely placed against the back, and rendered immovable." No mention is made of a sponge on those that came from the Channel.

among the rocks on the sea-shore; but whether the place of their capture was above or below high-water mark, is not stated on the ticket with which the specimens are labelled.

Dr. Grube's specimens were taken at St. Malo.

December 17. R. I. Pocock.

SUGGESTIONS FOR THE FORMATION AND ARRANGEMENT OF A MUSEUM OF NATURAL HISTORY IN CONNECTION WITH A PUBLIC SCHOOL.

Having lately been asked by Dr. Warre, Head Master of Eton, to give him some assistance in the fitting up, arrangement, and management of the museum about to be inaugurated at that College, I put down some notes, which he was pleased to think might be of use in pointing out the lines that should be followed with most advantage. As these notes are equally applicable to other school museums, I venture to publish them for the information of those who may be in position to profit by them, premising that they are mere outlines, which are susceptible of much elaboration in detail, and of some modifications according to special circumstances.

The subjects best adapted for such a museum are zoology, botany, mineralogy, and geology.

Everything in the museum should have some distinct object, coming under one or other of the above subjects, and under one or other of the series defined below, *and everything else should be rigorously excluded.* The curator's business will be quite as much to keep useless specimens out of the museum, as to acquire those that are useful.

The two series or categories under which the admissible specimens should come are the following:—(1) Specimens illustrating the teaching of the natural history subjects adopted in the school, arranged in the order in which the subjects are, or ought to be, taught. (2) Some special sets of specimens of a nature to attract boys to the study of such branches of natural history as readily lie in the path of their ordinary life, especially their school life, and to teach them some of the common objects they see around them.

The specimens of the first class should be all good of their kind, carefully prepared and displayed, and fully labelled. They should also be so arranged that they can be seen and studied without being removed from their position in the case or in any way disturbed or damaged. It would be best that they should never be taken out of the museum, but if it is necessary to remove them for the purpose of demonstration at lectures or classes, special provision should be made by which a whole tray or case can be moved together, with due precautions against disturbing the individual specimens. As a rule, the teachers should either bring the classes into the museum for demonstrations, or they should rely upon a different set of specimens kept in store in the class-rooms, and only brought out when required, and which may be handled and examined without fear of injury. Really good permanent preparations may be looked at, but not touched except by very skilled hands.

In zoology the collection should consist of illustrations of the principal modifications of animal forms, living and extinct, a few selected typical examples of each being given, showing the anatomy and development as well as the external form. The series now in the course of arrangement in the Central Hall of the Natural History branch of the British Museum, in the Cromwell Road, may, as far as it is complete, be taken as a guide, but for a school museum it will not be necessary to enter so fully into detail as in that series.

In botany there should be a general morphological collection, showing the main modifications of the different organs in the greater groups into which the vegetable kingdom is divided, and illustrating the terms used in describing these modifications. Such a collection may also be seen (although still far from complete) in the same institution.

For a teaching collection of minerals, an admirable model has for several years past been exhibited in the Mineralogical Gallery of the Natural History Museum, being, in fact, the various paragraphs of Mr. Fletcher's "Introduction to the Study of Minerals" cut up, and with the statements in each illustrated by a choice specimen.

The geological collection would best be limited mainly to a series illustrating the rocks and characteristic fossils of the British Isles, arranged stratigraphically. There would be no difficulty in making such a series on any scale, according to the space available, and if well selected and arranged, it would be extremely instructive and form a complete epitome of the whole subject. It should be placed in a continuous series along one side of the room, beginning with the oldest and ending with the most recent formations. It might be preceded by some general specimens illustrating the various kinds of rock structures, &c.

Mineral and fossil specimens are generally to be procured as wanted from the dealers, and as they require little or no preparation, collections illustrating these subjects can be quickly made, if money is available for the purpose. This is not, however, the case with zoological and botanical specimens, most of which require labour, skill, and knowledge to be expended upon their preparation before they can be preserved in such a manner as to make them available for permanent instruction.

We will next proceed to consider what objects may be included under the second head, many of which need not be constantly exhibited, but may be preserved in drawers for special study. These may be—

(1) A well-named collection of the commoner British insects, especially those of the neighbourhood in which the school is situated, with their larvæ, which should (if means will allow) be mounted on models of the plants upon which they feed. All should have their localities and the date of capture carefully recorded. These are best kept in a cabinet, with glass-topped drawers, with a stop behind, so as to allow them to be pulled out for inspection, but not entirely removed. Such a collection, formed of specimens prepared and presented by Lord Walsingham, can now be seen in the British Room of the Natural History Museum.

(2) A similar collection of British shells, especially the land and freshwater shells of the neighbourhood.

(3) If space and means allow, a collection of British birds, especially the best-known and more interesting species. Rare and occasional visitors, reckoned in the books as British, which are the most expensive and difficult to procure, are the least important for such a collection. Variations in plumage in young and old, and at different seasons, should be shown in some common species. Every specimen must be good and well mounted, or it is not worth placing in the museum.

(4) The principal British mammals of smaller size, especially the bats, shrews, and mice.

(5) The British reptiles, Amphibia, and commoner fishes, so shown that their distinctive characters may be recognized.

(6) A collection, as complete as may be, of British plants, or at all events of the plants of the neighbourhood. By far the best way of preserving and exhibiting such a collection is in glazed frames, movably hinged upon an upright stand, as may be seen in the Botanical Gallery of the Natural History Museum. A collection arranged in this manner should find a place in every local museum of natural history.

(7) A collection of the fossils found in the quarries of the neighbourhood, should there be any.

Every collection or series should be kept perfectly dis-

tinct from and independent of the others, and its nature and object clearly indicated by a conspicuous label.

The exhibited specimens should be arranged in upright wall-cases or in table-cases on the floor of the room. For the latter a high slope is preferable, and in all the exhibition space should not extend too high or too low for comfortable inspection. Between three to six or seven feet from the floor should be the limits for the exhibition of small objects. The three feet nearest the floor may be inclosed with wooden doors forming cupboards or fitted with drawers. Glass in this situation is liable to be broken by the feet or knees.

The museum should have a permanent curator—a man of general scientific attainments, and who is specially acquainted with, and devoted to, museum work, and who might also be one of the teachers, if too much of his time is not so occupied. But, as he is not likely to have special knowledge of more than one branch of natural history, the teachers of the other branches represented in the museum would probably each give advice and assistance with regard to his own department. It is also probable that some of the boys may be sufficiently interested in the work to render valuable aid in collecting and preparing specimens.

If ethnographical, archæological, historical, or art collections be also part of the general museum scheme, they should be kept quite distinct from the natural history collections, preferably in another room.

Above all things, let the following words of Agassiz be remembered: "The value of a museum does not consist so much in the number as in the order and arrangement of the specimens contained in it."

W. H. FLOWER.

THE FISHERY INDUSTRIES OF THE UNITED STATES.

THE volumes which form the subject of the present article are the continuation of a complete monograph of the fisheries and fishing industries of the United States, of which the first and second sections have already been published under the titles of "A Natural History of Useful Aquatic Animals," and "A Geographical Review of the Fisheries of the United States."

The direction of the immense investigation necessary for the preparation of this work has been in the hands of Mr. G. Brown Goode, who, as early as 1877, had drawn up a scheme for an exhaustive exploration of the coast of the United States in connection with the fishing industry. The enterprise was undertaken jointly by the United States Fish Commission and the Census Bureau, and the expenses of investigation, compilation, office and field work, and publication, have been shared by these two departments.

A work of this magnitude was quite beyond the powers of an individual, and we find accordingly that a number of authors, whose names are given at the back of the title-page, have been associated with Mr. Brown Goode in his undertaking. Among them are many names well known to science from their contributions to the natural history of the United States. Chief among these are Messrs. Marshall MacDonald, J. A. Ryder, and other members of the United States Fish Commission.

An English reader will invariably use his knowledge of British fisheries as a standard for comparison with those of a foreign country, and, in doing so, will find many difficulties, owing, not only to the difference in the species of fish which are found on the two sides of the Atlantic, but to the fact that many of our common names, such as pollack and hake, are applied to different fish in America, and that the Americans often use an altogether peculiar zoological nomenclature, which may throw even an experienced zoologist into confusion. Many American fishes of great commercial importance are unknown in Great Britain, such as the tautog (*Tautoga onitis*), the squeteague (*Cynoscion regale*), the blue-fish (*Pomatomus saltator*), the menhaden (*Brevoortia tyrannus*), and the shad (*Clupea sapidissima*). The most favourite edible crab of North America (*Callinectes hastatus*), the blue crab, is a perfectly distinct species from our common *Cancer pagurus*, and the American lobster (*Homarus americanus*) and oyster (*Ostræa virginica*) are different from our own. The European sole is unknown in American waters, as are our turbot and brill; the halibut, which has only recently become important in British fisheries, is of great importance in America, and their "plaice" (*Paralichthys dentatus*) differs entirely from the fish known to us by that name. These and many other differences in the species of marketable fish are important, as they serve in part to explain the different methods pursued by American fishermen; why, for instance, beam-trawling is unknown in their waters.

Of the third section of the monograph, which forms a half of the first of the four volumes under consideration, Mr. Brown Goode himself says:—"It is the first report of the kind ever written. It describes the locations, the characteristics, and the productiveness of the numerous grounds resorted to by the fishermen of the United States, extending from Greenland to Mexico, from Lower California to Alaska, and including the fishing grounds of the great lakes." For the Atlantic seaboard this work is carried out on a scale of completeness never before attempted. Not only does the text abound with information relative to the different fishing grounds and banks, their history, productiveness, the character of their bottom, and the weather prevailing there at different seasons, but the whole of this is graphically represented in a series of admirable charts which form in themselves a complete fisherman's guide to the whole coast from Greenland to Mexico. In addition to this, the migrations of different species of fish from locality to locality are alluded to, and the characters of the invertebrate fauna are, in some instances, adduced in explanation of these migrations. It is impossible to criticize this part of the work: to do so one must have a thorough knowledge of all the principal fishing-grounds of America; but, granted that the information and observations on which the charts and text are founded are correct, the method of displaying this information is unimpeachable.

Not the least valuable part of Section III. is the appendix containing the temperature observations from 1881 to 1885 inclusive. A word as to the manner of making these observations will not be out of place. The Census Bureau was, of course, unable to undertake this kind of work, and the Fish Commissioners, whose steamers were constantly engaged in expeditions to various localities, found that they could not keep a sufficiently continuous record of the temperatures observed at different points along the coast. Application was accordingly made to the United States Lighthouse Board and Signal Service, and these departments instructed their *employés* to make the required observations as part of their regular duties, and without extra compensation. The editor acknowledges the thoroughness with which these men performed the gratuitous services demanded of them, and the result is a large number of charts of temperature curves for each observing station, and charts showing the isothermal lines connecting the stations in different years.

The Pacific fisheries are dealt with in a much less complete manner, and are referred to as being undeveloped. The Alaskan fisheries are more fully dealt

[1] "The Fisheries and Fishery Industries of the United States." By George Brown Goode, Assistant Secretary of the Smithsonian Institute, and a staff of Associates. Section III. The Fishing-Grounds of North America, with 49 Charts, edited by Richard Rathbun. Section IV. The Fishermen of the United States, by George Brown Goode and Joseph W. Collins. Section V. History and Methods of the Fisheries; in Two Volumes, with an Atlas of 255 Plates. (Washington: Government Printing Office, 1887.)

with, and have a special interest as forming the chief, if not the only means of subsistence of the native population. The methods of fishing adopted there are of the most primitive character, and very few civilized fishermen are employed in the industry. Fish, however, is exceedingly abundant, and its value is shown by the price of salmon (*Onchorhynchus*) in the Yukon River. Dried salmon is called *ukali*, and the best quality *chowichee ukali*. One *chowichee ukali* is accounted a sufficient day's food for six men or dogs, and can be purchased for one leaf of tobacco, or five to eight musket-balls.

The fourth section of the monograph relates to the United States fishermen themselves. In 1880 there were 101,684 *bonâ fide* professional fishermen in the United States, those men only being reckoned as fishermen who make more than half their income by fishing. At the same time there were in Great Britain and Ireland between 90,000 and 100,000 fishermen who would come under this definition. It appears that whalers and sealers are reckoned among the American fishermen, and as they are certainly not reckoned in the English computation, the number of men engaged in fishing, properly so called, would be about equal in the two countries. Of the United States fishermen, the majority, including the negroes of the Southern States, and the Alaskans, are native-born American citzens, while from 10 to 12 per cent. are foreigners. The majority of the latter are natives of British provinces; the remainder are made up of Portuguese from the Azores, Scandinavians, Irish, and Englishmen, Italians, Indians, and, on the Pacific coast, Chinese. The chapters devoted to the fishermen of the different States are very interesting. The description of the Maine fishermen might be taken from any English fishing port. They are hardy, self-reliant, and honest, but are ill educated, inveterate grumblers, and entirely in the hands of the middleman. They will work hard when fishing, but are reluctant to undertake any other work, even for good pay. They marry early, and have large families, whilst their profits are low, the average annual return to each fisherman being $175 (about £36).

Oyster-dredging seems to have a peculiarly demoralizing effect in the United States, the white oystermen of Maryland being reckoned as the lowest of their class. The New England fishermen are the best educated, the most enterprising, and the most successful in the United States. Unlike the majority of European fishermen, they do not form a class apart, and have no peculiar traits or characteristics marking them off from their fellow-countrymen. They are good men of business, and many of them have left the fishing trade altogether, and been highly successful in other branches of business. Their fishing-craft, nearly all schooner-rigged, are the finest and largest in the world, and their life on board is far more civilized and comfortable than anything met with in Europe. Their earnings are far higher than those of the Maine fishermen. A Gloucester man will commonly make $1000 (more than £200) in a year, whilst skippers who are partly owners have on rare occasions made as much as $10,000 to $15,000 in a single year (from £2000 to £3000). Men living under such conditions are naturally of a high standard of intelligence, and the U.S. Fish Commission have profited largely from the co-operation of the New England fishermen. They have from the first recognized the value of a scientific inquiry in fishing matters; have in many instances devoted themselves heartily to assisting the labours of the Commissioners; have kept regular records of their journeys, including observations on tides, temperatures, weather, and sea-bottoms; have collected the fauna of the different fishing-grounds, and otherwise have been instrumental in helping scientific observation. They have one and all been ready to profit by the information gained by the Commission, and have readily tried and adopted novel methods of fishing, such as gill-nets for cod-fishery, and purse-seines for catching mackerel.

It is obvious, from a perusal of this volume, that the American fishermen are far more careful of their fish than Englishmen; they do not thump them down on the deck and stamp about on them, as is too commonly done on a British smack; they carefully clean them on board, and store them in proper receptacles, and, where fish is cured, it is commonly done on board when the fish is perfectly fresh. The reputation of the Gloucester, Mass., fishermen is curiously illustrated by a petition sent to the Lord-Lieutenant of Ireland this year. It was reported that several American schooners were coming to fish for mackerel off the coast of Ireland, and the fishermen, who do not fear the competition of English and French boats, were in great alarm lest the Americans with their purse-seines and large boats should utterly sweep the seas of fish.

Section IV. closes with a description of the dangers to which American fishermen are exposed, and an account of the management of fishing-craft. The whole is most interesting reading.

Section V. comprises two thick volumes of text and one of plates. The subjects it deals with range from whale-fishing to sponge-gathering, from baiting hooks to preparing sardines. Each branch of the fishing industry is minutely described in the text; the history of the fishery is given; old and new methods are compared; the boats, crews, fishing-gear, methods of packing and curing on board are carefully explained, and the descriptions are supplemented by a profuse number of illustrations.

It will be unnecessary to follow the various branches of fishing in detail, but a few remarks on special forms of fishing will be of interest. As has been said above, the Americans have no beam-trawl fishery: the flat-fish which are so highly prized in Europe are either absent from the American shores, or are held in low estimation, and we find no special mention of flat-fish fisheries in this section, with the exception of the extensive fishery for halibut. There appears to be a prejudice against flat-fish in many parts of America, and there is certainly a prejudice against the use of the beam-trawl. If the latter were introduced, and the several flat-fishes which are abundant in some parts of the United States waters were thrown freely into the market, an important branch of fishery would no doubt be established. Halibut are caught in deep water by means of long lines, known in America as "trawls," just as they are by the Grimsby boats working in the neighbourhood of the Faroe Islands. The method of setting several long lines round the schooner by means of smaller boats called "dories," is well worth noticing, but the great risk to life entailed by the use of the "dories" is an objection to introducing this mode of fishing into British waters.

The cod-fishery of the United States is very large, and is carried on to a large extent on the Great Bank of Newfoundland, as well as on the Labrador and St. Lawrence coasts. There appears to be a fine cod-fishery off Alaska, but it has only been partially worked by a small fleet hailing from San Francisco. The cod-fishery was formerly, and still is to a large extent, carried on by hand lines and long lines, or "trawls," but in 1880 the U.S. Fish Commission succeeded in introducing gill-nets, long since used by the Norwegians, among the fishermen of Gloucester. The obvious advantages of the cod gill-nets are that they save the fishermen the trouble and expense of obtaining bait, which is often as difficult to procure as it is in England, and thus increase their profit; they are easily set and worked, they catch more than the long lines working on the same ground, and as the size of the mesh is adapted only for cod of a certain size, the small fish or "trash" pass through and escape. This is a good example of the practical usefulness of the U.S. Fish Commission.

The accounts of the menhaden and mackerel fishing show that the Americans are as prone to complain of particular modes of fishing as English fishermen: the purse-seine is as obnoxious to some of them as the beam-trawl is in England, and the use of steam is at least equally unpopular. Steam is used chiefly in the menhaden fishery, and this, in combination with the purse-seine, a net practically unknown in England, has, it is alleged, utterly destroyed the menhaden fishing in certain districts. This led to petitions to Congress for the protection of the menhaden fishery, and in 1882 and 1883 the matter was inquired into, and protective legislation recommended. The evidence of actual decrease in the fishery does not appear in the Report on the fishery, but as the Commissioner of Fisheries was a member of the Committee which drew up the Report recommending legislative interference, it is to be presumed that he was satisfied that the fact of a diminution of the menhaden, due to over-fishing, was established.

Mackerel-fishing is conducted entirely by sailing-boats, most of them schooners of sixty tons register and upwards, and in these days it is carried on almost entirely by means of the purse-seine. In England, the summer fishing for mackerel is carried on by means of hand lines, and small boats may be seen "railing" or "whiffing" amongst the schools of mackerel. This method was formerly followed in America, but is now, to all intents and purposes, a thing of the past, the figures of small boats "jigging" and "drailing," as it is called in America, being given only in illustration of an obsolete industry.

The purse-seine first came into general use in 1850, but its greatest development dates only from 1870, and since the latter date there has been great opposition to its use, on the score of its destructiveness. The statistics of the mackerel-fishery do not, however, warrant this opposition. Mackerel-fishing has always been uncertain, and, as early as 1660, prohibitory laws of various kinds were passed to prevent, as it was supposed, the destruction of this industry. In 1838, twelve years before the introduction of purse-seines, the catch of mackerel was very small, and then the blame was laid on "the barbarous method of taking mackerel called gigging." The largest take of mackerel in a single year was in 1831, when 449,950 barrels of pickled mackerel were officially inspected; the second largest catch was in 1881, when 391,657 barrels were inspected. The worst catch was in 1877, when 127,898 barrels were inspected. A glance at the official tables shows that the fluctuations in the mackerel-fishery are quite independent of the usual method of fishing. The use of purse-seines might advantageously be tried in England, though it was found a failure by American schooners fishing off the Norwegian coasts, because, as it was alleged, the mackerel moved there in smaller schools than on the opposite side of the Atlantic.

In the second volume, on history and methods, English readers will find especial interest in the account of the great fur-seal industry of Alaska, which is regulated, as is well known, by a wise law prohibiting the destruction of more than a fixed number of seals every year.

No one who reads these volumes can fail to be struck with the practical national benefit of the United States Fish Commission. The production of this great work is only a small part of their active usefulness, but if it be judged by its utility alone, it is an exceedingly important part. When finished, this monograph of the fishing industry of the United States will form a complete textbook of American fisheries in all their branches, and will serve not only to interest the American public in a great national industry, but as a reliable guide to all those who are engaged in the fishing trade itself. In many cases it will be eminently serviceable as a book of reference to the practical fisherman, informing him of the localities and characteristics of fishing-grounds with which he is unacquainted, of the kinds and abundance of fish that he may expect there at different seasons, and of the best methods of prosecuting fisheries to which he is unaccustomed. Capitalists and manufacturers will learn from it how they may most profitably embark in a new industry, and the consumer will know from it how to judge of the quality of the article he consumes, and where to obtain it to the best advantage. It is impossible to refrain from drawing a comparison between this enlightened support given to an industry which from its very nature is incapable of being benefited by private effort, and the comparatively small support given by the English Government to our own fisheries, which, when the whale and seal fisheries are discounted, are at least of equal value with those of the United States. There are, indeed, signs that it is being generally recognized that the *laissez faire* policy as applied to national fisheries is a mistake. It is to be hoped that, when our Government takes another step forward, the example of the United States may not be lost sight of, and that, in addition to a central office with its necessary clerks and official administrators, a staff of skilled scientific investigators and practical men may be appointed, such as will be able to produce as exhaustive a work as that under review.

NOTES.

On Friday evening last, Sir Lyon Playfair, having distributed the prizes and certificates gained by the students of the City of London College, delivered an interesting address, taking as his chief subject the need for vital improvements in English methods of education. There had been, he said, a marked change going on over the world in regard to work. Machinery had been taking the place of muscular labour. Less human labour was employed, but it was much better paid than formerly. The workman must adapt himself by trained intelligence to these changes, otherwise he would go to swell the ranks of unskilled labour. Foreign countries had been quicker awake to the changes that were going on than we had been. We were proposing technical education, while France, Germany, Belgium, and Switzerland had been adapting themselves to the altered state of things by improved schools, secondary schools, commercial, building, and other special schools, which they had been promoting for many years. Germans and Frenchmen were taking places in English counting-houses, because the youth of London had not been educated in those languages which were necessary to commerce. We were now beginning to awake to the necessity of doing what was being done in other countries. Until comparatively lately we had nothing but classical schools. The learned classes had been entirely separated from the people; but the people's knowledge of trade improved science, and science improved trade. The learned classes were ignorant of this. This was not the way that the magnificent science and literature of Greece and Rome arose. Their great philosophers were busy in commerce, and were acquiring experience and knowledge among the masses of their own countrymen. This, he was rejoiced to see, was what we were now trying to bring about in this country.

The formation of two new Microscopical Societies has recently been announced. One of these is the Scottish Microscopical Society, meeting in Edinburgh, with the following office-bearers: President, Prof. Sir W. Turner; Vice-Presidents, Prof. Hamilton and Mr. Ad. Schulze; Secretaries, Dr. A. Edington and Mr. Geo. Brook. This Society has already held two successful meetings. The other Society is the Italian Microscopical Society, intended to bring together microscopists from the whole of Italy. The subjects for research, specially mentioned in the prospectus, are animal and vegetable histology, petrology, bacteriology, and the structure of the microscope and its appliances.

At Leyden there is a fine ethnographical collection, which is especially valuable so far as it relates to the Dutch East Indian

territories. At present this collection is seen to great disadvantage, but there is some prospect that it may soon be transferred to better quarters. A Parliamentary Committee has recommended that proposals should be submitted to Parliament for the erection of a suitable building.

THE Public Free Libraries Committee of Manchester, in their annual report, just issued, state that the success which has so long attended the working of the public free libraries in that city still continues in all departments. During the last twelve months the number of readers and borrowers at the various libraries and reading rooms (*i.e.*, the number of visits they made) reached an aggregate of nearly four millions and a half (4,442,499), being over 70,000 in excess of the previous year. The number of books used for home reading and for perusal in the reading rooms was 1,649,741. In the preceding year the number was 1,606,874, the increase being 42,867. The daily average of volumes used in all the libraries was 4700. Of the volumes issued to readers at the libraries, 336,058 were read in the reference library, 507,964 in the reading rooms attached to the branches, and 64,770 in the Bradford, Harpurhey, and Hyde Road reading rooms. The number of volumes lent out for home reading was 740,949. Out of these only sixteen are missing. There are now 197,947 volumes in the libraries. The committee express regret that the limited resources at their disposal prevent the extension of branch libraries and public reading rooms, but they trust that the Council will, before long, enable them to take the necessary measures for giving effect to the resolution of the Council passed unanimously on December 21, 1887, with regard to obtaining parliamentary powers for the removal of the present restriction of the rate (a 1*d*. in the £) to be expended for library purposes.

THE following scientific lectures will probably be delivered at the Friday evening meetings of the Royal Institution before Easter, 1890:—January 24, Prof. Dewar, F.R.S., scientific work of Joule; January 31, Sir Frederick Abel, F.R.S., smokeless explosives; February 14, Prof. J. A. Fleming, problems in the physics of an electric lamp; February 21, Shelford Bidwell, F.R.S., magnetic phenomena; February 28, Prof. C. Hubert H. Parry, evolution in music; March 7, Francis Gotch, Esq., electrical relations of the brain and spinal cord; March 14, Prof. T. E. Thorpe, F.R.S., the glow of phosphorus; March 21, Prof. G. F. Fitzgerald, F.R.S., electromagnetic radiation. On Friday, March 28, a lecture will be given by Lord Rayleigh, F.R.S.

ON December 8, at 6.30 a.m., a severe shock of earthquake was felt in Upper and Central Italy, Dalmatia, the Herzegovina, and Bosnia. At Serajewo three shocks were felt, the direction being from south-east to north-west. They lasted for five seconds each.

THE inhabitants of the town of Reggio d'Emilia, in Upper Italy, are very much alarmed by the activity of the volcano, the Queccia de Salsa, which is situated about eight kilometres from the town. During the last two or three weeks it has thrown up lava, stones, and ashes.

IN the *Comptes rendus* of the French Academy of Sciences for December 9, M. Angot has published an interesting paper on the observations of temperature at the top of the Eiffel Tower. The mean monthly maxima and minima for July to November inclusive are compared with those recorded at the Parc Saint-Maur. According to the usual decrease of temperature with height, the tower observations should be about 2°·9 lower than at the ground station, but the difference is much greater in summer during the day, and much less in winter during the night. In calm and clear nights especially, the temperature has been found to be nearly 11° higher at the summit than at the base. At the time of a change of atmospheric conditions, the change is manifested some hours, or even days, at the higher station. A striking instance of this occurred in November. After a period of high pressure, with calms and easterly breezes, the wind on the surface became strong, and shifted to south-south-west, and temperature rose. But the change had manifested itself on the tower on the evening of the 21st, and during the whole period from the evening of the 21st to the morning of the 24th, the temperature at the tower was higher than at the base, at some times even exceeding 18°. Observations made by a "swinging" thermometer at 11h. a.m. on the 22nd showed that the inferior limit of the warm current was approximately between 500 and 600 feet above the ground.

THE Third Report of the Meteorological Institute of Roumania for the year 1888 shows that much progress is being made, with very scanty means, thanks to the willingness of the observers and to the voluntary assistance rendered in the preparation of the observations for publication. The Institute has been established only four years, and at the beginning of 1889 it numbered 21 stations of various classes, in addition to 42 rainfall stations. The observations are regularly published in the *Annales* of the Institute, a quarto volume of about 600 pages, about half of the volume being devoted to discussions, in French and Roumanian.

FOR a year past Mr. R. W. Schufeldt has been working at a memoir on the morphology and life-history of *Heloderma suspectum*, the well-known poisonous lizard of the south-western part of the United States. This memoir is now nearly ready for publication. Biologists have hitherto denied *Heloderma* even the rudiment of a zygomatic arch, and Dr. Günther, of the British Museum, has said in his article "Reptiles," in the ninth edition of the "Encyclopædia Britannica" (p. 451), that "the skull of *Heloderma* is very remarkable in that it has no zygomatic arch whatever." We learn from Mr. Schufeldt that his recent dissections of this lizard go to prove that such statements must be qualified. Upon examining skulls of both old and young individuals of *H. suspectum*, he has found at least a very substantial vestige of the arch in question. It consists of a freely articulated, conical ossicle, standing on the top of the quadrate, being moulded to the outer side of the posterior end of the squamosal, with which it also freely articulates. It is seen to be present upon both sides. That this is the osseous rudiment of the hinder end of the zygomatic arch in this reptile, there cannot, Mr. Schufeldt thinks, be the shadow of a doubt.

AT a recent meeting of the American Ornithologists' Union, Mr. Jonathan Dwight, Jun., read a paper on birds that have struck the statue of Liberty, Bedloe's Island, New York Harbour. He said, that, on account of its lighter colour, more birds strike the pedestal of the statue than the statue itself. The statue was erected too late in 1886 for the migratory birds. It was first struck on May 19, 1887, then late in August, when the lights were said to be put out by birds. The first date at which birds struck the statue in 1889 was August 5, when fourteen were killed. A few others were killed during the month, and a considerable number in September and October. October 24 was the last date at which birds were killed. The whole number killed this year was 690, which was considerably less than in 1888 or 1887. He found that every cold wave in the early fall was followed by migratory birds flying against the statue. Of the dead birds picked up this year, 60 per cent. belonged to one species, the Maryland yellow-throats. The remaining 40 per cent. included a great variety.

AT the meeting of the Scientific Committee of the Royal Horticultural Society on December 10, Mr. Morris read a letter addressed to the Director, Royal Gardens, Kew, by Mr. R. W.

Blunfield:—"I see in the August number of the *Kew Bulletin*, an interesting account of the *Icerya purchasi*, and its depredations in South Africa, California, &c. During the past four years our gardens at Alexandria have been invaded by a coccus, which threatens now to destroy all our trees, and is causing the greatest alarm here. . . . It first appeared about four years ago, when I noticed it in quantities on the under side of the leaves of a banyan tree, but it has since spread with extraordinary rapidity, and one of our most beautiful gardens, full of tropical trees and shrubs, has been almost destroyed. A breeze sends the cottony bugs down in showers in all directions. It seems to attack almost any plant, but the leaves of the *Ficus rubiginosa*, and one or two other kinds of fig, seem too tough for it, and it will not touch them. It seems almost hopeless here for a few horticulturists to try to eradicate this formidable pest, while their indifferent neighbours are harbouring hotbeds of it, and there will have to be some strong measures taken by law to put it down." The insect in question had been referred to Mr. Douglas, and was said to be an undescribed species of Dactylopius. Spraying with kerosene emulsion was recommended, but no remedy was likely to be effectual that was not carried out universally.

The new number of the Journal of the Royal Horticultural Society contains a full and interesting report of the proceedings of the National Rose Conference held at the gardens of the Society at Chiswick on July 2 and 3. In the same number there are the following papers: on irises, by Prof. Michael Foster; the strawberry, by Mr. A. F. Barron; strawberries for market, by Mr. G. Bunyard; the origin of the florist's carnation, by Mr. S. Hibberd; peaches and nectarines, by Mr. T. F. Rivers; on conifers, by Mr. W. Coleman; on pears, by Mr. W. Wildsmith.

A German biography of the late Dr. E. G. F. Grisanowski, by Elpis Melena, has just been published (Hanover: Schmorl und von Seefeld). The book ought to be interesting to anti-vivisectionists, as Dr. Grisanowski was an enthusiastic advocate of their ideas, and much attention is given to the subject by his biographer.

The United States Department of Agriculture has issued the first and second of a series of illustrated papers on the North American fauna. They are by Dr. C. Hart Merriam. The first is a revision of the North American pocket mice, and includes descriptions of twelve new species and three new subspecies. The second paper contains descriptions of fourteen new species and one new genus of North American mammals.

The sixth edition of Mr. H. Bauerman's "Treatise on the Metallurgy of Iron" (London: Crosby Lockwood and Son) has been published. Mr. Bauerman explains that, as the progress in iron and steel manufacture during the seven years that have elapsed since the last issue of the volume has been mainly in the direction of perfecting the appliances and working details of the great processes introduced between 1858 and 1878, it has not been necessary to make any very great alteration in the principal part of the text. The additions required to bring the information up to date have been placed mostly as supplemental notes at the end. The statistical details have been revised and brought up to the latest dates for which returns are available.

In a recent paper on zoogeography, in *Humboldt*, Dr. Lampert states that a good many wolves are still captured in the east and west provinces of Germany, *e.g.* about fifty annually in Lorraine. In France, 701 wolves were destroyed in 1887; in Norway, only 15. It is estimated that in Russia the yearly loss in domestic animals through wolves is over £2,000,000, and the loss of game from the same cause, over £7,000,000. The German mole swarms, apparently, in the neighbourhood of Aschersleben, where 97,519 individuals were taken last year, and rewards amounting to £97 were paid. In great part of Germany, however (Upper and Lower Bavaria, East and West Prussia), it is not met with. Mecklenburg and Pomerania are its northern limits, at present. The beaver is nearly extinct in Germany, but a new settlement of thirty individuals was recently discovered at Regenwehrsberg, not far from Schönebeck, on the Elbe. A recent catalogue of diurnal birds of prey in Switzerland (by Drs. Studer and Fatio) gives thirty-two species. The disappearance of the golden vulture is here noteworthy. Early in this century it was met with in all parts of the Alpine chain; whereas now, only a very few individuals survive on the inaccessible heights of the Central Alps.

An interesting inquiry into prehistoric textiles has been recently made by Herr Buschan (*Arch. für Anthrop.*) He examined tissues with regard to the raw material used, to their distribution in prehistoric Germany, to their mode of production, and to their alteration by lying in the ground. With certain chemical reagents he was able to distinguish the various fibres, though much altered. The oldest tissues of Germany (as we now know it) come from the peat-finds of the northern bronze period. On the other hand, some articles of bone found in caves of Bavarian Franks, and evidently instruments for weaving or netting (bodkins, knitting needles, &c.), show that already in the Neolithic period textiles were made. The art of felting probably preceded that of weaving. Herr Buschan sums up his results as follows: (1) in the prehistoric times of Germany, wool (mostly sheep's) and flax were made into webs, but no hemp; (2) the use of wool preceded that of flax; (3) the wool used was always dark; (4) most of the stuffs were of the nature of huckaback (none smooth); (5) the textiles have, on the whole, changed but little in course of time. The author has some interesting observations on the oldest kinds of loom. The pile-builders on the Pfaffiker, Niederwyl, and Boden Lakes, were busy weavers; and they knew how to work flax fibres not only into coarse lace, fish nets, or mats, but into such finer articles as fringes, coverlets, embroidery, and hair-nets.

In a recent Consular Report from British North Borneo, an account is given of the explorations for gold which were made in the territories of the British North Borneo Company last year. The main obstacle had always been the difficulty of ascending the river, which is full of shallows and rapids, and of forwarding supplies of provisions, as the country is totally uninhabited, and does not afford supplies of any kind whatever. Striking into the forest at a point in Darvel Bay, which was judged to be nearest to the desired district, Mr. Skertchly crossed three sharp ridges of mountains, and at length struck the higher Segama, at a place some 250 miles inland from its mouth. The track is only 31 miles long, but great difficulty was experienced in bringing up provisions, as, owing to the rocky and mountainous nature of the ground, animals could not be used for transport, and everything had to be carried, at considerable expense, on men's backs. Payable gold was found soon after the Segama was reached, and the higher the river was ascended the more there was, but it was patchy and uncertain, and, so far, no reefs are reported, the gold being almost entirely in the river-bed. It is now certain, says the Consul, that payable gold exists, but whether the extent of country it is found in is large or small has yet to be ascertained, while the expense of conveying provisions to the gold-fields will require gold to be abundant to make it worth while working, unless an easier path is found. Mr. Skertchley was five months and a half in the forest without coming out once, and it was mainly owing to his foresight in arranging details, and his perseverance in carrying on the expedition, that success was due.

The Annual Report of the Conservator of Forests at Singapore refers at great length to the difficulty of dealing with a

grass called *lalang* (*Imperata cylindrica*, Gyr.), which is not only useless, but very injurious, both by reason of its inflammability, and because it prevents any cultivation of the land covered by it, except with a great deal of labour and expense. Wherever the land is burnt or having been under cultivation is suffered to run to waste, it is soon covered with *lalang*, whatever may have been the previous vegetation, except where the soil is sandy, or wet, or shaded by trees. The treatment of the soil by chemicals, such as salt, sulphate of iron, &c., apart from the heavy expense connected with it, is liable to have a very injurious effect, even for many years, on the plants with which the ground is afterwards afforested. The introduction of some more actively growing plant to combat and destroy the *lalang*, has been proposed, but this would be to destroy one noxious weed by another still more noxious. When trees are tall enough to throw a shade upon the ground, the *lalang* quickly disappears, nor can it penetrate even into forest glades if but a few trees bar its progress. It is suggested, therefore, that shade trees and bushes should be gradually planted.

OUR ASTRONOMICAL COLUMN.

Objects for the Spectroscope.

Sidereal Time at Greenwich at 10 p.m., December 26 = 4h. 22m. 20s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G. C. 839	—	—	4 15 32	+19 7
(2) 47 Eridani	5	Reddish-yellow.	4 28 54	− 8 25
(3) ε Tauri	4	Whitish-yellow.	4 22 12	+18 56
(4) μ Eridani	4	White.	4 40 0	− 3 27
(5) R Leporis	Var.	Red.	4 54 36	−14 56
(6) U Geminorum ...	Var.	Variable.	7 48 34	+22 17
(7) Neptune, Dec. 26..	—	Greenish.	4 2 21	+18 59
,, Jan. 2 ...	—	—	4 1 44	+18 57

Remarks.

(1) This is described in the General Catalogue as an exceedingly interesting object, but very faint and small; according to Hind it is variable. I have not been able to find any record of its spectrum. Continuous observations over a considerable period, even with small dispersion, may throw light upon the nature of the changes which take place.

(2) A star of Group II., in which Dunér records the bands 2–8. Bands 2 and 3 are the strongest, indicating that the star is well advanced in condensation towards Group III. As in similar stars, dark metallic lines and lines of hydrogen should receive special attention, as the stages at which these make their appearance have not yet been determined.

(3) Vogel classes this with stars of the solar type, and the usual differential observations are suggested. (For criteria, see p. 20.)

(4) According to Konkoly, this is a star of Group IV. The usual observations of the relative intensities of the hydrogen and metallic lines are required, so that the star may be placed in line with others on the temperature curve.

(5) This is a variable star of Group VI., but the range of variation is small (6·5–8·5). The origin of variability in stars of this group has not yet been satisfactorily explained, and there is no record of the spectroscopic changes which accompany the changes in magnitude. Further observations are therefore necessary, and it is suggested that variations in the intensities of the carbon flutings should be particularly noted. The star was at minimum on October 23.

(6) This variable reached its maximum on December 21, and, as the period is only 86 days, observations may be made from maximum to minimum, providing that sufficient optical power is employed. The magnitude ranges from about 9 at maximum to 14 at minimum. The colour is stated to vary from white at maximum to reddish at minimum. The spectrum has been described as continuous (probably near maximum), but the colour-changes indicate that considerable variations in the spectrum may also be expected.

(7) The spectrum of Neptune was first observed by Secchi, in 1869. He noted that there were three broad dark bands, which were nebulous at the edges, and that there was a remarkable absence of red light. Vogel gave a more detailed account of the spectrum in 1872 (*Bothkamp Beobachtungen*, 1872, p. 71). The bands then recorded were as follows:—

Wave-lengths.			Remarks.
597	...	...	End of spectrum.
565·7	...	...	End of a wide dark band.
556	...	...	Very feeble band.
540	...	...	Middle of the darkest band.
518	...	...	Faint band.
513	...	...	,,
507	...	...	,,
485·8	...	...	Middle of a dark band.
477	...	...	Middle of a wide dark band.

The whole spectrum is very similar to that of Uranus. The proximity of the edges of some of the dark bands to the bright flutings of carbon and manganese led Prof. Lockyer to suggest that in Uranus and Neptune we might have to deal with the radiation of those substances, the dark bands being produced by contrast. Acting on this suggestion, I made observations of Uranus with a 10-inch equatorial, and afterwards, in conjunction with Mr. Taylor, with Mr. Common's 5-foot reflector. Direct comparisons certainly showed coincidences of the flutings of carbon with luminous parts of the spectrum. No solar lines were visible, but Dr. Huggins has recently photographed the spectrum, and found nothing but solar lines. In a recent observation of Neptune, I thought the bright flutings were more evident than in Uranus, but I have not had an opportunity of making comparisons. Further observations with reference to the existence of bright flutings are suggested. A. Fowler.

Variable Star in Cluster G.C. 3636.—Prof. Pickering writes (*Astr. Nachr.*, 2941) that photographs are being taken at Wilson's Peak, Southern California, with a telescope of 13 inches aperture. Four photographs, with exposures of about one hour each, were taken of the above cluster, whose position for 1900 is R.A. 13h. 37m. 35s., Decl. + 28° 52′·9. A star about twenty seconds south of the centre of the cluster was found to be much brighter on May 21 and June 8, 1889, than on May 31 and June 17, 1889. Two maxima seem to be indicated by the photographs separated by an interval, during which the star becomes comparatively faint. Visual observations made at Cambridge Observatory since June appear to confirm this variability.

Changes in Lunar Craters.—A few observations made by Prof. Thury (*Astr. Nachr.*, 2940), of craters in the terraced ring of Plinius, indicate some striking changes. On November 1, Plinius presented the same aspect as that described in 1882 by MM. Elger, Gaudibert, and H. Klein. Two craters, cutting one another, appear in the middle of the ring, and it is thought that one of these was not visible in the middle of September. The central opening seems to have been enlarged, for on November 1 its diameter was estimated as at least one-third of the total crater, whereas in September the diameter of the opening was rather less than one-fourth of the total diameter.

The interpretation put by Prof. Thury upon these appearances is that in the centre of Plinius there are two small craters, the aspect of which is modified by the different amounts of snow and ice about them. Emissions of heated gas and vapour would affect considerably the state of the lunar surface, for if, in the beginning of an eruption, water-vapour were predominant, it would be immediately condensed around the crater, forming a circular field of snow, so that the apparent enlargement of the opening may be due to the melting of the snow surrounding it by the hot gases emitted.

ON THE FUTURE OF OUR TECHNICAL EDUCATION.

LAST week we referred to an address delivered by Sir Henry Roscoe at Goldsmiths' Hall on Tuesday, December 17, after the distribution of the prizes and certificates to the students of the City and Guilds of London Institute. He spoke as follows:—

In his admirable address delivered last year on a similar occasion to the present, Sir Lyon Playfair pointed out that one of the important objects for which the City Guilds were originally founded was to develop and restore arts and sciences,

and act as teachers to pupils. In the ancient charters the word "Universitas" is used for the modern designation of Guild. University simply means a teaching corporation, whether for professional or trade purposes. In both cases the teacher is termed a "master," and the pupil an "apprentice" from *apprendre*, to learn. The function of teaching by the Guilds was gradually lost. The master became the capitalist, the pupil the workman. The capitalist does not consider it part of his duty—quite the contrary—to teach the workman his craft, and thus the latter, though handy in one branch, never becomes a craftsman; intelligence is wanting, and the industry suffers when placed in competition with that for which the craftsman has been intelligently trained.

But now the Guilds have recovered their long lost ground, and by a natural process of evolution they are now engaged separately and collectively in nobly carrying out the work for which to a great extent they were originally constituted.

This new departure, or rather this recurrence to the ancient type, we know as technical education, and we define it as the instruction in those arts and sciences which underlie the practice of the industry or trade, this instruction being given in the technical school.

No attempt is there made to teach the trade or industry itself; this is done, and can only be done, in the factory or workshop. The school teaches how to make the best article; the workshop, how to make that article cheapest. The school ignores economical production, whilst this is the all-important factor in the workshop.

In my remarks this evening I propose to consider how the Guilds are now carrying on this work, and to point out the relation which that work bears to the general question of technical education in the country, which is now acknowledged on all hands to be one vitally affecting our industrial supremacy amongst the nations.

This acknowledgment has now received a national recognition in the passing of the Technical Instruction Act of last session of Parliament, and thus has materially changed the whole aspect of affairs. *Now* technical instruction, which has hitherto been sporadic may become systematic, for private effort has received national authorization, and sooner or later a complete scheme for technical instruction must be forthcoming.

The commencement of such a scheme has indeed already been made by the efforts of the City Guilds. Your Institute, with its various branches, is the nucleus of such a system, the importance of which will perhaps only be recognized when the history of this great educational movement comes to be written.

Starting from small beginnings, this work has already attained dimensions which exceed the most sanguine expectations of its founders.

The extension of your technological examinations has been so rapid that now no fewer than 12,000 students are receiving instruction in 500 registered classes in 113 towns in the Kingdom, whilst 6000 students passed the examinations last year.

Of the value of these examinations as stimulating a knowledge of the *rationale* of practical processes there can be no doubt. The age of empiricism is past, rule-of-thumb is dead, and a new rule, that of scientific training or organized common-sense, has taken its place.

These examinations serve to spread that scientific training amongst the masses of our population, and though they do not accomplish *all*, they accomplish much, and the classes if not all first-rate are still vastly better than none at all, and it is satisfactory to note that the employers of skilled labour are beginning to find out that the men thus trained are of greater value than those who have not had such training.

To quote one example of this among many, a pupil of the Manchester Textile School gained at the last examination the silver medal in honours. He was simply a "cotton operative," but since that time he has obtained the post of manager of 1170 looms under a large manufacturing firm, and the determining factor in his success over a great number of competitors was his possession of the silver medal first-class certificate in honours of this Institute.

But, after all, the attendance on these classes is only the beginning. A more thorough training is needed; for this the Institute has founded the admirable model "Intermediate" Technical School in Finsbury, where the course is a real preparation for entering the workshop, and thus the pupils begin industrial life under more favourable conditions than otherwise would have been the case.

It is much to be hoped that the Institute may not only be able to continue grants to this most useful school, but may see its way to plant other similar schools in various parts of the metropolis, which after all is the greatest industrial centre in the Kingdom.

But the Institute does not stand alone in carrying on this great work of raising up the true craftsman, and thus helping to keep down that danger to our overcrowded centres of population—the great army of unskilled labour. The Guilds are separately taking up the question, and if we may deplore the withdrawal of some from the general scheme, we may well commend their efforts in other directions. Witness the foundation by the Company in whose hall we are now assembled of a great technical and recreative institute at New Cross, which bids fair to become a centre of light and leading in a district dark and backward.

Again, look at what the Drapers' Company have done, and are doing, at the East End to place the People's Palace on a sound financial basis; or at the still greater work, if such things can be compared, which the Clothworkers' Company has done in Yorkshire and other districts to place upon sure scientific foundations the clothworker's craft.

Amongst these efforts to raise the industrial capabilities of our population we must not forget the scheme of the Charity Commissioners for the application of the property of the City of London charities. This arose out of an Act passed six years ago at the instance of my friend Mr. Bryce, which directed that the general funds of these charities should be applied to the benefit of the poorer part of the population.

No less a sum than £50,000 per annum is thus applicable, and the scheme lately put forward by the Commissioners for the appropriation of this sum is, on the whole, an admirable one, which may, if wisely worked, end in the creation of what may be termed a popular technical University for London. The value of such an organization as is thus proposed will be appreciated by those who have some knowledge of how these things are managed on the Continent, and in how chaotic a state is the whole of London education beyond the rank of the primary school.

All these efforts are truly "signs of the times;" they point to the recognition by the better endowed that not merely is it their duty, but their self-interest, to see that those who have the power know how to use it wisely, for it is on this that our national stability and progress depend.

But it is not enough simply to educate the craftsman; his employer needs it equally, if not more, and this task is, perhaps, a more difficult one, for as the Royal Commissioners on Technical Education report, "Englishmen have yet to learn that an extended and systematic education, up to and including original research, is a necessary preliminary to the fullest development of industry," and this necessity your Council have fully acknowledged, for, at the inauguration of your Central Institution, Lord Selborne said:—"It is, however, in the appreciation of, and in the facilities for higher technical instruction, that we in this country are most deficient, and it is to supply this want that the Central Institution has been established, . . . in which new and increased facilities will be afforded for the prosecution of original research, having for its object the more thorough training of the students and the elucidation of the theory of industrial processes."

I do not think that one could more emphatically or more clearly define the character of the work needed for the highest instruction of the future leaders of industry, than Lord Selborne has done in these words.

Now, the question arises, Is the Central Institution accomplishing the ends thus clearly marked out? It must be admitted that the supply of students has hardly been equal to the expectations formed by its friends at the outset. But if the work done is of a high class, and if those who come within its walls are there fitted for discharging the higher duties which modern industry requires, we may be satisfied, for the fact is that the demand for high-class technical instruction has yet to be created. Other difficulties beset this particular kind of teaching. One is that, as in many new institutions, the students enter ill-prepared, and thus the instruction is forced into elementary lines, and the time which can be given to higher work materially shortened.

A second, is that of hitting off the happy mean between the teaching of theory and that of practice, and in order that this essential may be accomplished, it is necessary that the teachers giving this higher technical instruction should be men who are well known and respected in their several professions, and not mere schoolmasters. In other words, that they shall know the

practice as well as the theory of the subjects they profess. Such men, as far as I am able to judge, your Council has found in the present able staff of professors.

Then again, in measuring the success of such a College, it must be remembered that it is intended for the *élite* of the industrial world, and that, as individual attention must be paid to each student in the laboratories and drawing-offices, the highest technical instruction of crowds is impossible.

Little seems hitherto to have been done in the way of training technical teachers, and for the obvious reason that the demand for such is very limited, whilst that for competent men to enter a more practical career is great.

But whether the College is training teachers, or those who are to carry out the lessons of such teachers into practice, does not matter. The object is to train men who can improve our present industries, and raise up new ones; and this may be accomplished by either or by both methods. Neither the one nor the other can, however, succeed unless the student of technology has a firm grasp of the scientific principles upon which his industry is based.

It is useless, and worse, to attempt to teach the applications to pupils to whom the science itself is an unknown quantity.

Hence arises the question, How and where can the preliminary science training be best given? and the answer to this raises many difficult and some delicate matters.

First, however, let me disabuse your minds of a notion which may become general, and, if so, harmful—namely, the new Metropolitan Polytechnic Institutions, as they are called, can ever do this highest and most important kind of education. Do not let us fancy that the establishment of these no doubt very valuable institutions is the ultimatum to be aimed at in technical education, or imagine that they can attempt to do what is done in Germany, France, or Switzerland by institutions bearing the same name. I look upon it as a misfortune that, by mere chance, the name of the old Institution in Regent Street, known to fame as the home of the diving-bell and of Prof. Pepper's Ghost, should have been retained for institutions which neither resemble it nor the high schools which form so marked a feature in the Continental educational system. These latter are in our country rather represented by the scientific departments of our Universities, and by those of the metropolitan and local University Colleges, by the Royal Normal School of Science, and by your own Central Institution. We cannot too clearly understand that whatever success attends the foundation of these Metropolitan Polytechnics—and no one more cordially wishes them success than I do—the work of the centres of the highest education still remains to be done; indeed, the greater the popularity of the lower institutions, the greater the need and scope for the higher ones.

The rapid growth in London of this idea of the importance of handicraft and recreative education is most remarkable, and for this stimulus we are almost wholly indebted to Mr. Quintin Hogg.

The effect of this movement upon your Institute has been severely felt, for it is clear that, whereas seven or eight years ago the enthusiasm of the City Companies was strongly in favour of the higher technical education in the Continental sense, it is now all for this newer and more popular, I will not say less useful, form of handicraft and recreative instruction.

It is a fact which may as well be clearly stated, that the Central Institution cannot do all it might do for want of a few thousands, and that the scheme of technological examinations is crippled by the loss of the support of those who at first nobly contributed towards these objects.

The Drapers prefer to support more popular institutions at the East End, and the Goldsmiths do likewise in regard to their own institution at New Cross, so that there is no doubt that the interest formerly felt in the general and collective work of the Institute is distinctly on the wane.

Well, ladies and gentlemen, a consideration of these patent facts leads one to the question, How are these things to go on? Are we never to have "law and order"—about which we have heard enough in other matters—introduced into affairs educational?

And in what I am about to say, let me premise that I merely express my own individual opinion as an independent observer, anxious only for the success of the good cause which we all have at heart. Then may I say that I am dead against a cut-and-dried system of Governmental education such as we see in other countries? I am all for stimulating and developing local effort to local requirements, and it is because I am fully aware of the dangers of centralization, and desire to promote adaptability to local needs, that I gave my hearty support to the Government Technical Instruction Bill as amended in the House of Commons, in which the power of the locality to work out its own educational salvation is fully safe-guarded.

But holding these views I see clearly that there are things which can only be satisfactorily accomplished by a central authority.

That our primary education can only be properly conducted on a national basis has been admitted for more than a quarter of a century; so it will be with the higher or secondary education, whether technical, commercial, or professional—we *must* have a system. As I have said, the foundation of your Institute was the beginning of such a system for technical instruction; but has it not already outstripped the bounds of your control? Can it be satisfactorily worked in the future on its present lines?

Let us look at the matter from an independent point of view. We have now three Government Departments charged with educational work—the Education Department for Elementary Instruction, the Science and Art Department, and the Charity Commissioners. One of the most important steps which could be taken to bring these under effective control is the appointment of a Minister of Education, of Cabinet rank, who would be in close touch with every part of our now discordant educational system. But that is not the immediate question before us.

This refers more especially to the desirability of consolidating the Science and Art Department. As you know, this controls and stimulates, in what I think we may allow to be a satisfactory manner, the teaching of elementary science and of art throughout the country. Would it not conduce to the benefit of the country, if the Guilds' technological examinations were to be undertaken by the Department, and thus placed on a national basis? Several of the subjects now included in the Directory of the Department, on which grants are made, are of a distinctly technical character, and therefore no objection can be raised that the other subjects now under the Guilds Institute cannot equally well be placed under the Department.

The benefits which would thus accrue are great and palpable, the two systems of examinations in pure and in applied science would then work side by side without friction or overlapping, and the extension of the technical examinations would be easy and certain.

If this were accomplished, I for one would strongly urge the removal of the system of payment on individual results—a method in all cases to be deprecated, but one which is especially unsuited for testing the value of technical instruction. This can be much more certainly effected by ascertaining the efficiency of the whole class, of the teacher, and of his appliances, by inspection or otherwise.

If once we get rid of this system of payment on individual results in one set of subjects, we may look forward to its ultimate extinction in the others, and no subject seems so suitable for making a beginning as that of technical instruction.

I would therefore suggest that the best means of securing the permanency and the extension of the very useful technological examinations which your Council—and all honour to them for it—have started, is to request the Government to take them over, thereby rendering the Science and Art Department more efficient, and enabling that Department to make the improvements and alterations in the system which it undoubtedly requires.

May I go one step further in these suggestions, and ask if this should be done, is it not a necessary corollary that the Central Institution should likewise become a Government Normal School for Applied Science? There is much to be said in favour of such a proposal.

The very situation, close to the Royal Normal School of Science, seems to forecast its ultimate destiny. Under separate management, no consistent or well-arranged scheme of common work is possible; brought under one direction, the essential alliance between pure and applied science, as regards teaching, becomes easy of attainment.

Students would pass and re-pass from the one school to the other, obtaining at the one the knowledge of the scientific principles, and, at the other, that of their applications.

Of the national advantages of such a fusion there can, I think, be little doubt. England would then be in possession of an institution which might, for completeness and efficiency, both as regards the *personnel* and the appliances, soon be made second to none on the Continent, and worthy of the greatest industrial nation in the world.

Your Institute would thus set itself free to extend its influence

in other directions, and could then concentrate its efforts on what is perhaps, after all, its most legitimate and most useful function—that of providing intermediate technical schools on the pattern of the Finsbury School, of which many are required in the metropolis.

The exact terms on which the Government would be prepared to take over this part of your work is a subject on which, of course, I cannot pretend to enter, but a satisfactory basis can, I do not doubt, easily be found.

Your Council would then feel that the great work which they have begun has been handed over in its full vigour to the nation, and that with the nation lies the responsibility of extending and perfecting the system which they have had the honour and the gratification of inaugurating.

I am aware that in making these suggestions, I have raised a somewhat burning question about which there may be difference of opinion, and my apology for this indiscretion, if one is needed, must be the importance of the subject, and the anxiety which we all feel that the technical education of our country shall be placed on a firm and enduring national basis.

A FIRST FORESHADOWING OF THE PERIODIC LAW.

IT is well known that the Newlands-Mendeleeff classification of the elements was preceded by the discoveries of certain numerical relations between the atomic weights of allied elements, due to Döbereiner, Dumas, and others; but what has been almost entirely ignored is the immense advance made by M. A. E. Béguyer de Chancourtois,[1] a French geologist of note, Professor at the École des Mines, who was the first to publish *a list of all the known elements in the order of their atomic weights.*

M. de Chancourtois embodied his results in two memoirs presented to the French Academy of Sciences in April 1862 and March 1863. These memoirs have never been printed *in extenso*,[2] but extracts from them, and additional notes relating to the subject, were published in the *Comptes rendus* for 1862 (liv. pp. 757, 840, and 967; lv. p. 600), 1863 (lvi. pp. 253 and 479), and 1866 (vol. lxiii. p. 24). The first note bears the date of April 7, 1862, so that there can be no doubt as to de Chancourtois's claim to priority in this important matter.[3]

I have in my possession a thin quarto pamphlet, by de Chancourtois, entitled "Vis Tellurique: Classement naturel des corps simples ou radicaux, obtenu au moyen d'un système de classification hélicoïdal et numérique" (Paris, Mallet-Bachelier,[4] 1863), which contains nearly all the extracts from the *Comptes rendus*, together with some additional matter. It contains, also, what is absolutely essential to the comprehension of de Chancourtois's ideas, the graphic representation of his system, which is not to be found in the *Comptes rendus.*

I propose to give here a translation of the first communication to the Academy, followed by certain explanatory comments and brief extracts from the other papers:—

"Geological studies in the field of research opened up by M. Elie de Beaumont in his note on volcanic and metalliferous intrusions (*émanations*) have led me, for the completion of a lithological memoir on which I am now engaged, to a natural classification of the simple bodies and radicles by a table in the form of a helix, founded on the use of numbers which I call *characteristic numbers* or *numerical characteristics.*

"My numbers, which are immediately deduced from the measure of the equivalents or other physical or chemical capacities of the different bodies, are, in the main, the proportional numbers given by the treatises on chemistry, these being reduced to half in the case of hydrogen, nitrogen, fluorine, chlorine, bromine, iodine, phosphorus, arsenic, lithium, potassium, sodium, and silver; in other words, I either divide the equivalents of these bodies by two in the system in which oxygen is taken as 100, or multiply by two the equivalents of the other bodies in the system in which hydrogen is taken as unity.

"On a cylinder with a circular base, I trace a helix which cuts the generating lines at an angle of 45°. I take the length of one turn of the helix as my unit of length, and starting from a fixed origin, I mark off on the helix lengths corresponding to the different *characteristic numbers* of the system in which the number for oxygen is taken as unity. The extremities of the lines thus marked off determine points on the cylinder which I call indifferently *characteristic points* or *geometrical characters*, and which I distinguish by the ordinary symbols for the different bodies. The same points will evidently be obtained if we take as the unit of length the $\frac{1}{16}$ of a turn of the helix, and mark off on the curve lengths corresponding to the numbers of the system in which hydrogen is represented by unity.

"The series of points thus determined constitutes the graphic representation of my classification, which may easily be traced on a plane surface by supposing the surface of the cylinder developed; by its aid I am enabled to enounce the fundamental theorem of my system: *The relations between the properties of different bodies are manifested by simple geometrical relations between the positions of their characteristic points.*

"For instance, oxygen, sulphur, selenium, tellurium, bismuth,[1] fall approximately on the same generating line, while magnesium, calcium, iron, strontium, uranium, and barium, fall on the opposite generating line. On either side of the first of these lines we find hydrogen and zinc on the one hand, bromine and iodine, copper and lead on the other; parallel to the second line we find lithium, sodium, potassium, manganese, &c.

"Simple relations of position on a cylindrical surface would be obviously defined by means of helices, of which the generating lines are only a particular case; hence, as a complement to the first theorem, we may add the following: *Each helix drawn through two characteristic points and passing through several other points or only near them, brings out relations of a certain kind between their properties; likenesses and differences being manifested by a certain numerical order in their succession, for example, immediate sequence or alternation at various periods.*

"In order to attain a greater degree of accuracy, it is necessary to discuss the results of different measurements with respect to the same body.

"One question is all-important in this discussion; it is to know if the divergencies which occur may have causes other than the error of experiment. I reply to this question in the affirmative.

"I think that here, as in all determinations of constants which we wish to compare, they must be reduced to the same conditions. This idea seems to me the indispensable complement to the notion of an absolute characteristic number. Once the existence of this absolute number or *numerical characteristic* guaranteed by the possibility of connecting it afresh with observed facts, certain limits of variation being allowed [*literally*, varying within certain limits], we promptly arrive at Prout's law, which presents itself as furnishing a means for reducing experimental observations to a comparable state by a series of trials, without this state being even a completely defined one, but, on the contrary, in order to be able to define it. The combination of this principle with the rules for alignment allow me to give the most striking form to my invention. I am thus led to formulate the table of integral numbers, which, as I must not omit to mention, exhibits under certain aspects the *résumé* of the work of M. Dumas on this subject.

"In the construction of this table I have had recourse to the determinations of specific heats, not only as a means of control, but also to find new numbers unattainable by the methods of chemical investigation. By adopting as the constant product of specific heat by atomic weight, the number which corresponds both to sulphur and to lead, I have deduced from the series of results given by M. Regnault, purely *thermic* quotients or numbers, which take their places on my alignments in the most felicitous way. I will only quote two examples: firstly, the number 44, obtained from the specific heat of the diamond, which finds its place on the generating line of the characteristic, 12, of carbon, by the side of the characteristic, 43, which corresponds to one of the equivalents generally accepted for silicon; and another

[1] Wurtz ("The Atomic Theory," p. 170) and Berthelot ("Les Origines de l'Alchimie," p. 302) give a bare mention of de Chancourtois's name. Mendeleeff, in his Faraday Lecture (Journ. Chem. Soc., October 1889), couples his name with those of Newlands and Strecker, and shows greater appreciation of his work.

[2] M. Friedel, the eminent Professor of Organic Chemistry at the Sorbonne, has kindly procured for me the information that the original manuscripts of these memoirs are preserved in the archives of the Institut; I hope to be able to examine them at some future period.

[3] Mr. Newlands' first paper, chiefly devoted to showing that the numerical differences between the atomic weights of allied elements are approximately multiples of 8 was published on February 7, 1863 (*Chemical News*, vol. vii. p. 70); his second paper, in which he arranges the elements in the order of their atomic weights, was published on July 30, 1864 (*Chemical News*, vol. x. p. 39). See J. A. R. Newlands "On the Discovery of the Periodic Law," &c. (Spon, 1884).

[4] Now Gauthier-Villars.

[1] This is probably a misprint, as bismuth does *not* fall on the same generating line in the table.

characteristic, 36, of silicon deduced from an equivalent proposed by M. Regnault, and which is very remarkable, from its coincidence with the characteristic of ammonium.

"By the discussion, which has shown me the advisability of accepting various results hitherto looked on as scarcely reconcilable, I have been led to conceive the possibility of reproducing the *series of natural numbers* in the series formed by the numerical characteristics of the real or supposed simple bodies supplemented by the characteristics of the compound radicles formed from gazolytic[1] elements, such as cyanogen, the ammoniums, &c., and doubtless also by the compound radicles formed from metallic elements, of which the alloys offer us an example. In this natural series, the bodies which are really simple, or at least irreducible by the ordinary means at our disposal, would be represented by the *prime numbers*. It will be at once seen that there are in my table at least twelve bodies, which, like sodium (23), have characteristics which are prime numbers. This is what led me to perceive this law, which, I believe, is destined, when established, to form one of the bases for the discovery of the law of molecular attraction. The predominance of the law of divisibility by 4 in the series of my table, a predominance which is also to be found in the elements of the theory of numbers, has confirmed me in the idea—an idea in itself really simple—that there is a perfect agreement between bodies, the elements of the material order, and numbers, the elements of the abstract order of things (*éléments de la variété matérielle, de la variété abstraite*). This goal once caught sight of, it will seem natural that I should have recourse to the theory of numbers to help me attain it. It will seem not less natural that I should also have recourse to higher geometry; for the series of relations it offers cannot fail to afford resources which may enable one to establish connections between the different numerical characteristics.

"My helicoidal system in this way leads me on towards abstract views of an extremely general nature; and on the other hand it should, it seems to me, find an application in the natural[2] sciences, as a method of classification throughout their whole domain, from the series of simple bodies which forms the prototype, to the opposite extreme of our natural divisions; in it will be found, I believe, the means of bringing into connection simultaneously, and by all their characters, the different terms of those parallel series, orders, families, genera, species, and races, in each natural kingdom, of which men of science have in vain tried to show the affiliation. In geology, as is evident, the application is implicit.

"Whatever may be the import of these considerations, and to return to the principal object of the present memoir, I think that the efficacy of the helicoidal system will be admitted as a means towards hastening the advent of the time when chemical phenomena shall be amenable to mathematical investigations.

"My table, by the distribution of bodies in simple or coupled series, by its indication of the existence of conjugate groups, &c., traces a plan for diverse categories of syntheses and analyses already executed or to be executed; it draws up very definite programmes for the execution of several researches which are exciting attention. Will not my series, for instance, essentially chromatic as they are, be a guide in researches on the spectrum? Will not the relations of the different rays of the spectrum prove to be derived directly from the law of numerical characteristics, or *vice versâ?* This idea, which occurred to me before we were taught the identification of the lines in the spectrum, and the admirable applications of this discovery, seems to me now even more than probable. Finally, looking upon it only as a concise representation of known facts, and reducing it to the points which offer no matter for discussion, the geometrical table of numerical characteristics affords a rapid method for teaching a large number of notions in physics, chemistry, mineralogy, and geology. I hope, therefore, that my natural classification of the simple bodies and radicles being capable of rendering manifold services, will need, like every object in habitual use, a name of easy application; hence, on account of its graphic representation and its origin, I give it the significant name of *telluric helix.*"

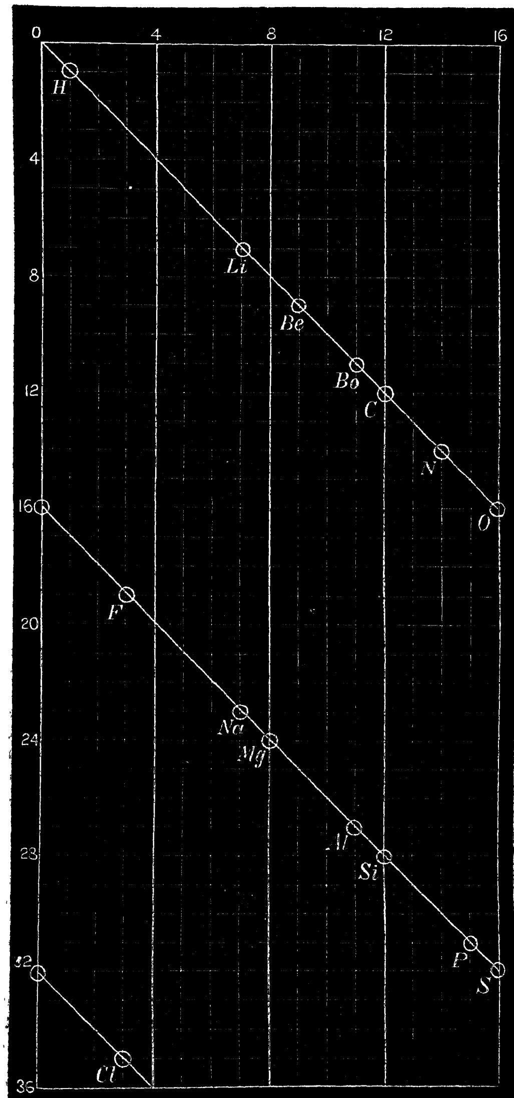
It will be well to point out immediately that M. de Chancourtois's system assigns to the *numerical characteristics* of the elements a general formula of the form ($n + 16n'$), where n' is necessarily an integer;[3] and his table thus brings out the fact that the differences between the atomic weights of allied bodies approximate in many cases to multiples of 16.[1]

Thus we get the parallel series of which our author speaks—

$$\overset{\text{Li}}{7} \ldots \overset{\text{Na}}{7 + 16 = 23} \ldots \overset{\text{K}}{7 + 2 \cdot 16 = 39} \ldots \overset{\text{Mn}}{7 + 3 \cdot 16 = 55} \quad \overset{\text{Rb}}{7 + 5 \cdot 16 = 87.}^{2}$$

$$\overset{\text{O}}{16} \ldots \overset{\text{S}}{16 + 16 = 32} \ldots \overset{\text{Se}}{16 + 4 \cdot 16 = 80} \ldots \overset{\text{Te}}{16 + 7 \cdot 16 = 128.}^{3}$$

As we glance at the first two turns of de Chancourtois's helix, we ask ourselves if the discovery of Newlands and Mendeleeff does not lie before us.



But the discovery of the "octaves" or "periods" cannot be ascribed to our author, although it seems almost impossible that chemists should not have perceived their existence on looking at his table.

[1] Metalloid.

[2] The term includes physical science.

[3] n is always represented in the author's table as integral, but he expressly states that he looks on this as by no means necessary. "The construction of the telluric helix rests on the use of proportional numbers derived from experiment. It would remain valid with fractional numbers, and often the helicoidal alignments would be even more easily applicable to these than to integers" (*Comptes rendus*, vol. liv. p. 842).

[1] This fact, now familiar, has again been noticed by your correspondent, Mr. A. M. Stapley, in the issue of November 21, 1889.

[2] The atomic weight of rubidium should be 85. We may notice that the author gives as probable also Cs = 135 = 7 + 8 . 16, which is thus placed on the same generating line.

[3] Certainly too high a value; according to Brauner, the exact atomic weight of tellurium remains to be determined.

Three important points, however, do exist in common between de Chancourtois's system and that of Mendeleeff:—

Firstly, all the known elements are arranged in the order of their combining weights.

Secondly, the combining weights chosen as best suited to bring out clearly the numerical relations existing between them are those adopted by Cannizzaro in 1858, a striking fact when we recollect that de Chancourtois wrote only in 1862, at a date long before these numbers had gained anything like general acceptance.

Lastly, an attempt is made to show that simple numerical relations exist, not only between the combining weights, but between all the measurable properties (*toutes les capacités physiques et chimiques*) of allied elements.

The reasons for the neglect of de Chancourtois's researches and the oblivion into which they have fallen are not far to seek. His style was heavy and at times obscure, and, moreover, his ideas were presented in a way most unattractive to chemists.

A geologist by profession, de Chancourtois had been powerfully impressed by the facts of isomorphism in the case of the feldspars and pyroxenes, which form such important constituents of the volcanic rocks he was studying; and he was thus led to seek for a system of classification which should bring out some simple relationship between the elements they contained.

To quote from his paper (*Comptes rendus*, vol. liv. p. 969): "The parallelism of the groups of manganese (7 + 3 . 16) and iron (8 + 3 . 16), of potassium (7 + 2 . 16) and calcium (8 + 2 . 16), of sodium (7 + 16) and magnesium (8 + 16), is the origin of my system"; and again, suggesting the expediency of adopting 55 (= 7 + 3 . 16) as a *characteristic* for aluminium, which would bring the element on the sodium and potassium generating line, "this would render perfect the parallelism between the elements of the feldspars and the pyroxenes, the starting-point of my system" (*Comptes rendus*, lvi. p. 1479).

Thus the correct idea of seeking for a relationship between the combining weights of isomorphous elements was marred by a somewhat imperfect comprehension of the facts of isomorphism. No chemist would certainly have tried to show any close relationship between aluminium on the one hand and the group of the alkalies on the other, notwithstanding their union in the feldspars and pyroxenes; and a suggestion of this kind served to cast discredit on de Chancourtois's really important views.

Notwithstanding his frequently eccentric ideas, de Chancourtois had the merit, so rare in an inventor of this stamp, of not considering his system as final. We cannot do better than let him speak for himself; and quote the conclusion of his last paper on the subject (*Comptes rendus*, lvi. p. 481):—"In presence of the rapid increase in the list of elements which engage the attention of chemists and physicists, it has become urgent to unite in one synthesis all the notions of chemical and physical capacities, of which the exposition would otherwise become an impossible task.

"It is, therefore, perhaps not unnecessary to recall the ideas of Pythagoras, or what I may better term the *Biblical truth* which dominates all the sciences, and of which I propose to make practical use by the following concrete example,[1] the first general conclusion of my essay:—

"THE PROPERTIES OF BODIES ARE THE PROPERTIES OF NUMBERS.

"It is easily perceived, that a helicoidal system of some kind or another, which is necessarily a graphic table of divisibility, offers the most convenient means for rendering manifest the relations between the two orders of ideas. It is evident, also, that the particular system which I have adopted brings into relief the relations of the most important and usual of the properties of matter, because the case of divisibility by 4, which is the basis of my plan, is the first which presents itself in arithmetical speculation after the case of divisibility by 2, to which there corresponds directly, as one perceives by a first glance at my table, the existence of the natural couples of elements, with consecutive odd and even characteristics.

"I hope, therefore, that the *telluric helix* will offer, until it is replaced by some more perfect invention, a practical framework, a convenient scale, on which to set down and compare all measurements of capacities, whatever the point of view which may be taken, whatever elasticity or variation, whatever interpretation may be given to the *numerical characteristics*, by which these capacities must always be represented.

[1] The French is *vulgarisation*, literally *popularization*.

"The development in a plane of the cylinder ruled into squares, with the circumference at the base divided into 16 equal parts, seems to me, in a word, to be a *stave* on which men of science, after the fashion of musicians, will note down the results of their experimental or speculative studies, either to co-ordinate their work, or to give a summary of it in the most concise and clear form to their colleagues and the public."

Lothar Meyer has noted down his classification in the form of a helix,[1] and Dr. Johnstone Stoney, F.R.S., has shown that the numerical values of the atomic weights may be expressed geometrically as functions of a series of integral numbers by points all lying approximately on a logarithmic spiral.

But no simple mathematical formula has so far been discovered to express the relationships of the atomic weights accurately—*i.e.* within the limits of experimental error, and de Chancourtois's predictions still remain but incompletely fulfilled.

I need not comment further on the remarkable breadth and originality of our author's views, taken as a whole. Strange to say, it was only a year or two before his death that he heard, through a colleague, of the immense development they had undergone; nor did he ever set up any claims to priority. But when we speak of the greatest generalization of modern chemistry, and recall the names of Newlands and Mendeleeff, it is only just that we should no longer forget their distinguished precursor, de Chancourtois.

P. J. HARTOG.

SCIENTIFIC SERIALS.

American Journal of Science, December.—The temperature of the moon, by S. P. Langley, with the assistance of F. W. Bery. With this memoir the authors complete the researches begun at the Allegheny Observatory in 1883 and continued during the next four years. The main outcome is that the mean temperature of the sunlit lunar surface is much lower than has been supposed, most probably not being greatly above 0° C.—The Lower Cretaceous of the South-West, and its relation to the underlying and overlying formations, by Charles A. White. The chalk formations constituting the so-called "Texas Section" are here referred to two natural divisions, which may be designated the Upper and Lower Cretaceous respectively, although not necessarily the exact equivalents of the corresponding European strata. Their fossil contents show that each represents an unbroken portion of Cretaceous time, while the palæontological contrast between the two indicates that there is a time hiatus between them. But this hiatus is no greater than exhibited in others of the mountain uplifts in the same region, and not so great as it is in some cases.—On the hinge of Pelecypods and its development, with an attempt toward a better subdivision of the group, by William H. Dall. Three fundamental types of hinges are described, and on these is based a new classification comprising the three orders of Anomalodesmacea with five sub-orders, Prionodesmacea with eight sub-orders, and Teleodesmacea with eleven or more sub-orders.—The magnetism of nickel and tungsten alloys, by John Trowbridge and Samuel Sheldon. The question is here discussed whether nickel and tungsten alloys magnetized to saturation increase in specific magnetism as different kinds of steel alloyed in small proportions with tungsten or wolfram are known to do. The tabulated results show that tungsten greatly increases the magnetic moment of nickel, if the alloy be forged and rolled, but has small influence if simply cast; nor do changes in the amount of tungsten appear to cause corresponding changes in the magnetic properties of the alloy.—Note on the measurement of the internal resistance of batteries, by B. O. Peirce and R. W. Willson. The authors' researches show that the value of the resistance of a cell obtained by the use of alternate currents is always smaller than that obtained by other methods, but the application of the method of alternate currents "fatigues" all but the so-called constant cells. In most cases there is a tendency in the internal resistance to *decrease* as the strength of the current which the cell is delivering increases.—Papers were contributed by Robert T. Hill and R. A. F. Penrose, Jun., on the relation of the uppermost Cretaceous beds of the Eastern and Southern United States, and on the Tertiary Cretaceous parting of Arkansas and Texas; by W. E. Hidden and

[1] "Die modernen Theorien der Chemie," iv. Auflage, p. 137; English translation, p. 118.

J. B. Mackintosh, on sundry yttria and thoria minerals from Llano County, Texas; and by O. C. Marsh, on the skull of the gigantic Ceratopsidæ.

THE *American Meteorological Journal* for November contains the first part of an article on "Theories of Storms, based on Redfield's Laws," by M. H. Faye, member of the French Institute. In support of his "whirlpool" theory, he urges that meteorologists have constructed a theory of storms on the basis of a single fact, viz. that storms which burst over a region cause a fall of the barometer there, and he points out that starting with the idea of an ascending column, exercising an aspiration below, a thing is invariably produced which neither turns nor progresses. Mr. A. L. Rotch contributes the first part of an article on "Meteorology at the Paris Exposition," dealing with the instruments exhibited in the French Section. Among the most interesting are (1) the actinometers exhibited by the Montsouris Observatory; (2) the Richard actinometer, which has bright and black bulbs *in vacuo*, connected with two thermometers, by which curves are traced giving at each instant the radiation from the sky, both at night and day; (3) the Richard anemographs, which have, instead of the usual Robinson cups, a fan wheel formed of six blades inclined at 45°, and fastened to a very light axis, one revolution of the wheel corresponding to one metre of wind. Parrigou-Lagrange's anemometer (NATURE, vol. xxxvii. p. 18), giving the vertical component of the wind, was also exhibited. M. Baudin showed some very fine standard thermometers, and Mr. Rotch describes various other instruments, such as hygrometers, aneroids, &c. Dr. F. Waldo continues his discussion of the "Distribution of Average Wind-velocities in the United States." The present article deals with the comparison of average wind-velocities with other elements, *e.g.* with barometric minima. Lieutenant Finley contributes State tornado charts for Arkansas, North Carolina, and Dakota.

THE numbers of the *Journal of Botany* for November and December are chiefly occupied with articles of special interest to students of British botany. Mr. Thiselton Dyer gives a very interesting biography of the late Mr. John Ball, F.R.S., first President of the Alpine Club, Under-Secretary of State for the Colonies under Lord Palmerston, an ardent explorer in all the four quarters of the globe, and a botanist of wide and varied knowledge. In the December number is a remarkable article on the disappearance of British plants, mainly through the depredations of collectors.

Rendiconti del Reale Istituto Lombardo, November 1.—Physical researches on the lakes of North Italy, by Prof. F. A. Forel. During a visit to this lacustrine region, last autumn, the author studied the waters of Lakes Maggiore, Como, Piano, and Lugano, with a view to determining their temperature, colour, and transparency, as compared with the analogous properties of Lakes Lucerne and Geneva. The results, which are here tabulated, show that the temperature is generally higher, and the colour deeper in the Italian than in the Swiss lakes, while the transparency is about the same, except in the shallow Lake Piano, where the temperature is lower and the transparency less than in any of these basins.—Meteorological observations made at the Brera Observatory during the month of September. These observations include records of temperature, barometric pressure, atmospheric moisture, rainfall, direction of the winds, and cloudiness.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, December 12.—"The Relation of Physiological Action to Atomic Weight." By Miss E. J. Johnston, University College, Dundee, and Thos. Carnelley, Professor of Chemistry in the University of Aberdeen. Communicated by Sir Henry Roscoe, F.R.S.

A. *As deduced from the Character of the Elements occurring naturally in Living Organisms.*—It is shown (*a*) that life is associated with a low atomic weight, so that elements with an atomic weight of 40 and under are required by the living organism, whereas those of an atomic weight greater than 40 are more or less inimical to life (compare Sestini, *Gazz. Chim. Ital.*, vol. 15, p. 107). (*b*) That the eight elements which enter most largely into the composition of the earth's crust, and which, therefore, are the most easily accessible to the living organism, are all included, with the exception of aluminium, in the fourteen elements which are required by the living organism.

A consideration of the exceptions (viz. Li, Be, B, Al, and Fe) to the first rule and of all the known facts bearing on the question leads to the conclusion that, "*The degree of necessity of an element to the living organism is a function of, first, its atomic weight, and, second, its accessibility to the organism.*" An element may be inaccessible to living organisms either because it is rare (*e.g.* Li and Be); or because, though moderately common, it has a very limited distribution (*e.g.* B); or because, though plentiful and widely distributed, it does not occur in nature in a form in which it can be assimilated (*e.g.* Al, on account of the insolubility of its native compounds).

That elements which are necessary to life must be readily accessible is self-evident, but that living organisms should require elements with low atomic weights, while elements with high atomic weights are inimical to life, is not so evident. This, however, may be due, in part at least, to the fact that the elements with low atomic weights are on the whole the most common elements (as shown by Gladstone, *Phil. Mag.* [5], vol. 4, p. 379; compare also Mendeljeff, *Zeit. f. Chem.* vol. 5, 1869, p. 405), and therefore the most accessible, so that *from the first the elements utilized in vital processes have been those which have been the most accessible, and therefore those with the lowest atomic weights.*

B. *As deduced from the Toxic Action of Compounds administered artificially.*—In view of the somewhat discordant results obtained by previous observers as to the relation between atomic weight and physiological action, the authors have reinvestigated the subject as carefully as possible. Their experiments have been made partly with fish (sticklebacks) and partly with aërial micro-organisms, the salt being administered by solution in the medium (water or Koch's jelly) in which the organism lived. The following conclusions are drawn from the results of about 800 experiments which the authors have made during the two years they have worked on this subject:—

1. *With corresponding compounds of elements belonging to the same sub-group, the toxic action*[1] *alters regularly* (i.e. *increases or diminishes*) *with the atomic weight.*

2. *In almost all cases this alteration takes place in such a way that the toxic power increases with the atomic weight.* (This is analogous to increase in toxic action in homologous series of carbon compounds.)

3. *Elements belonging to odd series* (Mendeljeff's classification) *are much more toxic than the corresponding elements of even series.*

4. *Other things being the same, the greater the ease of reducibility of an element from a state of combination to the free state the greater its toxic action.* (Applicable to compounds of odd as compared with those of elements of even series, and also to compounds of the elements of odd series belonging to the same group when compared with one another.)

5. *Other things being the same and the compounds comparable, the greater the heat of formation of a compound from its elements the smaller is its toxic power*; or, in other words, the greater the stability of a compound the smaller its toxic power. (Applicable to elements belonging to odd series; data for those belonging to even series are wanting or are too incomplete.)

There is a close connection between rules 3, 4, and 5.

6. *Lithium forms a very marked exception to all the above rules*, for notwithstanding its very low atomic weight, its difficult reducibility to the free state, the fact that it belongs to an even series, and the great stability of its compounds, as indicated by their relatively great heat of formation, its toxic power is, nevertheless comparatively very great. This exceptional character of lithium, however, is not limited to its physiological action only, but applies likewise to many of its purely chemical and physical properties. So much so, indeed, is this the case that its exceptional physiological character might have been foreseen.

7. *The toxic action of a series of comparable salts runs parallel with the solubility in such a way that as the solubility increases the toxic action either increases likewise or else diminishes.*

8. *When the quantity of salt present in Koch's jelly is less than the minimum dose required to prevent the development of micro-organisms, the number of colonies which develops increases as the amount of salt diminishes, but as a rule much more rapidly.*

[1] As represented in terms of either the minimum toxic weight of metal or of the minimum molecular toxic dose. The minimum molecular toxic dose = minimum toxic weight of salt ÷ molecular weight of the salt.

9. When Koch's jelly has been previously neutralized with sodium carbonate the minimum quantity of metallic salt required to prevent the development of aërial micro-organisms is scarcely altered in the case of KCl, NaCl, $MgCl_2$, and $HgCl_2$, but is slightly greater in that of $CaCl_2$, and much less in the case of KBr, KI, NaBr, NaI, $ZnCl_2$, and $CdCl_2$, than when the jelly has not been neutralized.

10. *Mercuric iodide*, notwithstanding its comparative insolubility, *has an exceptionally high antiseptic power*, which is $1\frac{1}{2}$ times as great as that of mercuric chloride per weight of salt, or $2\frac{1}{3}$ times as great per weight of metal, or 3 times as great per minimum molecular toxic dose.

Geological Society, November 20.—Mr. W. T. Blanford, F.R.S., President, in the chair.—The Secretary announced that a series of specimens from the line and the neighbourhood of the Main Reef, east and west of Johannesburg, Witwatersrand Gold Fields, had been presented to the Museum by Dr. H. Exton, and a letter from that gentleman in explanation of them was read. In this Dr. Exton stated that all but one of the mines represented were on the main reef of the district, which has a general direction east and west, its dip varying generally from 45° to 80°. South of the main reef, and parallel to it at a distance of 15–20 feet, is a narrow reef known to the miners as the "south leader," and generally much richer than the main reef. The gold-bearing deposits consist of conglomerates, specimens of which, and of a purplish-red rock which forms a jagged ridge at some distance north of and parallel to the so-called reef, were contained in the collection. The President considered the occurrence of the gold in large quantities in such a conglomerate was a remarkable and interesting case. The rock was an ancient-looking one, and the country appeared to have undergone much disturbance. Dr. Hinde remarked that in Nova Scotia beds of conglomerate of supposed Carboniferous age were formerly worked for gold, but the yield had not been very great.—The following communications were read:—On the occurrence of the striped hyæna in the Tertiary of the Val d'Arno, by R. Lydekker.—The catastrophe of Kantzorik, Armenia, by Mons. F. M. Corpi; communicated by W. H. Hudleston, F.R.S. Secretary. The village is 60 km. from Erzeroum, and 1600 metres above sea-level. Subterranean noises and the failure of the springs had given warning, and on August 2 last part of the "eastern mountain" burst open, when the village, with 136 of its inhabitants, was buried in a muddy mass. The author described the district as formed of Triassic, Jurassic, and Cretaceous strata, subsequently broken up and torn by granitic, trachytic, and basaltic rocks, which overlie the Secondary rocks, according to the nature of the dislocation. The flow was found to have a length from east to west of 7–8 km., with a width ranging from 100 to 300 metres, and the contents were estimated at 50,000,000 cubic metres. It appeared as a mass of blue-grey marly mud, which, after the escape of the gases, solidified at the top; the inequalities projected to the extent of 10 metres. The site of the village was marked by an elevation of the muddy mass, some of the *débris* of the houses having been carried forward. The lower part of the flow was still in a state of motion, and carried forward balls of marly matter. It was difficult to approach the source of this flow on account of the crevasses in the side of the mountain. An enormous breach served as the orifice for the issue of the mud, which emitted, it was said, a strong odour. The violent projection of this marly liquid and "incandescent" (?) mass had carried away a considerable portion of the flanks of the mountain, whose *débris* might be recognized on the surface of the flow by the difference of colour. Great falls were still taking place, throwing up a fine powder which rose into the air like bands of smoke. There were also fissures and depressions of the ground at other localities in the neighbourhood. The President, in commenting on the remarkable nature of the phenomenon, said it was not a volcanc eruption, but more of the nature of a mud-flow produced by a big landslip—possibly connected with the stoppage of the springs. Still it was on a very large scale, though clearly the effect of water and not of fire. Dr. Evans agreed with the President. It was difficult to reconcile the alleged incandescence with the other phenomena. Infiltration of water probably had something to do with the outburst. It was not even a mud volcano. The falling in of the mountain, he thought, might have been due to soft beds covered by harder material having oozed out. It would be interesting to know if there had been an increased rainfall prior to the occurrence. There was nothing of a truly volcanic nature mentioned in the paper. He should like to have further information about the incandescence. Mr. Dallas (the translator of the paper) said that the "redness" was reported by the people to the author. Rev. Edwin Hill thought that the mud-balls could in no way be explained by igneous agency. The photographs gave no indication of the presence of steam. As a landslip the amount was very great, and possibly the phenomenon might be something similar to the overflow of peat-bogs. Mr. Hudleston recalled the statement of the author regarding the geological constitution of the district, where masses of Secondary rocks are folded within igneous ones, probably of Tertiary age. It was likely, therefore, that some of the softer Secondary marls, pressed in more than one direction by harder rocks and soaked by water, might at last have given way. The immediate cause of the catastrophe could scarcely be indicated without a knowledge of the district. Such events occurred from time to time elsewhere. The Russian topographers, if his memory served him right, had described the bursting of a mountain-side with fatal results, in one of the valleys near Lake Issyk Kul. The smoke-like powder, resulting from the continued falls of rock, had often given rise to the notion of volcanic action. There could be no better instance of this than the case of Mount St. Elias, the highest mountain in North America. In geography-books this mountain has almost invariably been described as a volcano, and a portion has actually been designated as the crater. This illusion had been occasioned by the dust of rock-falls resembling smoke. We might well pardon the author for speculating on the probability of a return to volcanic activity in a region which bears so many traces of it as this part of Armenia.—On a new genus of Siliceous sponges from the Lower Calcareous Grit of Yorkshire, by Dr. G. J. Hinde.

December 4.—Mr. W. T. Blanford, F.R.S., President, in the chair.—The President stated that a circular letter had been received from the Secretary of the Committee on Geological Photographs, formed at the last meeting of the British Association for the Advancement of Science, to arrange for the collection, preservation, and systematic registration of photographs of geological interest in the United Kingdom, in which the aid and co-operation of geologists is earnestly requested. Copies of instructions, &c., drawn up in order to secure uniformity, are to be obtained on application to Mr. O. W. Jeffs, Secretary to the Committee, 12 Queen's Road, Rock Ferry, Cheshire, and one would be suspended on the Society's notice-board.—The following communications were read:—On remains of small Sauropodous Dinosaurs from the Wealden, by R. Lydekker.—On a peculiar horn-like Dinosaurian bone from the Wealden, by R. Lydekker. Among a series of vertebrate remains sent from the Dorsetshire County Museum to the British Museum, there is an imperfect, stout, short, cone-like bone from the Wealden of Brook, Isle of Wight. It appears to present a close resemblance to the horn-cores of the Dinosaur described by Prof. Marsh as *Ceratops*. The author did not regard the specimen as affording conclusive evidence of the existence in the Wealden of a large Dinosaur furnished with horn-like projections on the skull like those of the American *Ceratops*, but suggested that such might really prove to be its true nature.—The igneous constituents of the Triassic breccias and conglomerates of South Devon, by R. N. Worth. The reading of this paper was followed by a discussion, in which the President, Prof. Bonney, Dr. Geikie, Dr. Hicks, Mr. Hudleston, Prof. Hughes, and Prof. Judd, took part.—Notes on the glaciation of parts of the valleys of the Jhelam and Sind Rivers in the Himalaya Mountains of Kashmir, by Captain A. W. Stiffe. After referring to the previous writings of Messrs. Lydekker, Theobald, and Wynne, and Colonel Godwin-Austen, the author gave an account of his observations made during a visit to Kashmir in 1885, which appeared to him to indicate signs of former glaciation on a most enormous scale. A transverse valley from the south joins the Sind valley at the plain of Sonamurg, and contains glaciers on its west side. These, the author stated, filled the valley at no remote period, and extended across the main Sind valley, where horseshoe-shaped moraines, many hundred feet high, occurred, and dammed the river, forming a lake of which the Sonamurg plain was the result. The mountains which originated the above glaciers were described as being cut through by the Sind river, and the rocks of the gorge were observed to be striated, whilst rocks with a *moutonnée* appearance extended to a height of about 2000 feet. The whole of the Sind valley was stated to be characterized by a succession of moraines through which the river had cut gorges, whilst the

hillsides were seen to be comparatively rounded to heights of 2000 feet or more. The author had also formed the opinion that at Bâramulla the barrier of a former lake occupying the Kashmir valley was partly morainic, before reading Prof. Leith Adams's view of the glacial origin of some of the gravels of this point. The whole valley of the Jhelam from this point to Mozufferabad showed extensive glacial deposits, which had been modified by denudation and by the superposition of detrital fans, widely different in character from the glacial deposits. Below Rampoor the valley was thickly strewn with enormous granite blocks resting upon gneiss, and the author believed that they had been transported by ice. In conclusion, it was noted that the existing torrential stream had further excavated the valley since Glacial times, and, in places, to a considerable depth. Comments on this paper were offered by the President, Mr. Lydekker, General MacMahon, and Prof. Hughes.

Entomological Society, December 4.—The Right Hon. Lord Walsingham, F.R.S., President, in the chair.—Prof. Franz Klapálek, of Prague, was elected a Fellow.—Mr. W. L. Distant exhibited, on behalf of Mr. Lionel de Nicéville, a branch of a walnut tree on which was a mass of eggs laid by a butterfly belonging to the *Lycænidæ*. He also exhibited two specimens of this butterfly which Mr. de Nicéville had referred to a new genus and described as *Chætoprocta odata*. The species was said to occur only in the mountainous districts of North-West India, at elevations of from 5000' to 10,000 feet above the sea-level.—Dr. D. Sharp exhibited the eggs of *Piezosternum subulatum*, Thunb., a bug from South America. These eggs were taken from the interior of a specimen which had been allowed to putrefy before being mounted. Although the body of the parent had completely rotted away, the eggs were in a perfect state of preservation, and the cellular condition of the yelk was very conspicuous.—Mr. J. H. Leech exhibited a large number of Lepidoptera recently collected for him by Mr. Pratt in the neighbourhood of Ichang, Central China. The collection included about fifty-four new species of butterflies and thirty-five new species of moths. Captain Elwes observed that he noticed only two genera in this collection which did not occur at Sikkim, and that the similarity of the insect fauna of the two regions was very remarkable; about fifteen years ago, in a paper "On the Birds of Asia," he had called attention to the similarity of species inhabiting the mountain ranges of India, China, and Java. Mr. McLachlan, F.R.S., remarked that he had lately received a species of dragonfly from Simla which had previously only been recorded from Pekin. Mr. Distant said he had lately had a species of *Cicada* from Hong Kong, which had hitherto been supposed to be confined to Java.—Mr. W. H. B. Fletcher exhibited a preserved specimen of a variety of the larva of *Sphinx ligustri*, taken in a wood near Arundel, Sussex. Mr. W. White exhibited drawings of the larvæ of this species, and called especial attention to one of a variety that had been exhibited at a previous meeting by Lord Walsingham.—Mr. F. D. Godman, F.R.S., read a letter from Mr. Herbert Smith, containing an account of the Hymenoptera, Diptera, Hemiptera, and Coleoptera, he had recently collected in St. Vincent, where he was employed under the direction of a Committee of the Royal Society, appointed to investigate the natural history of the West Indies. A discussion followed, in which Dr. Sharp, Captain Elwes, Lord Walsingham, and Mr. McLachlan took part.—Captain Elwes read a letter from Mr. Doherty, in which the writer described his experiences in collecting insects in the Naga Hills, by means of light and "sugar." Colonel Swinhoe said that the attractive power of light depended very much on its intensity, and on the height of the light above the ground. By means of the electric light in Bombay he had collected more than 300 specimens of *Sphingidæ* in one night. Mr. J. J. Walker, R.N., stated that he had found the electric light very attractive to insects in Panama. Dr. Sharp, Mr. Leech, Captain Elwes, the Rev. Canon Fowler, and others continued the discussion.—Mr. de Nicéville communicated a paper entitled "Notes on a New Genus of *Lycænidæ*." —Mr. F. Merrifield read a paper entitled "Systematic Temperature Experiments on some Lepidoptera in all their Stages," and exhibited a number of specimens in illustration of his paper. The author stated that the darkness of colour and the markings in *Ennomos autumnaria* resulted from the pupæ being subjected to a very low temperature. In the case of *Selenia illustraria*, exposing the pupæ to a low temperature had not only affected the colour of the imagos, but had altered the markings in a striking manner. Lord Walsingham observed that it appeared that exposure to cold in the pupa-state produced darker colouring in the imago, and that forcing in that stage had an opposite effect; that insects subjected to glacial conditions probably derive some advantage from the development of dark or suffused colouring, and that this advantage was, in all probability, the more rapid absorption of heat. He said he believed that an hereditary tendency in favour of darker forms was established under glacial conditions, and that this would account for the prevalence of melanic forms in northern latitudes and at high elevations. Captain Elwes, Mr. Jenner Weir, and Dr. Sharp continued the discussion.

Linnean Society, December 5.—Mr. J. G. Baker, Vice-President, in the chair.—Mr. George Murray exhibited and made some remarks upon specimens of *Struvea macrophylla* and *S. plumosa*.—Mr. A. W. Bennett communicated some observations on a new and a little-known British fresh-water Algæ—*Schizothrix anglica* and *Sphæroplæa annulina*. It was pointed out that *Schizothrix* of Harvey's "Phycologia Britannica" is really an *Inactis*.—Mr. E. M. Holmes exhibited, as a new British marine Alga, a specimen of *Gracilaria divergens*, a rare native of the warmer portions of the Atlantic and the Mediterranean, which had been recently found at Brighton by Mr. J. Myles. The specimen exhibited possessed tetrasporic and cystocarpic fruits not described by Agardh.—Mr. Pascoe exhibited (with a view of eliciting information as to the *modus operandi*) a number of Crustacea and certain shells of the genus *Phorus* having various foreign substances attached to them. Commenting upon these specimens, Prof. Stewart gave an interesting account from personal observation of the way in which certain Crustacea collect and adorn themselves with fragments of shell, seaweed, &c., apparently as a protective covering.—Mr. T. Christy exhibited and made remarks on some "liquid-amber" or resin (*Attingia excelsa*) from Cochin China.—A paper was then read by Mr. George Massee on the life-history of a stipitate fresh-water Alga, illustrated by some excellent diagrams. A discussion followed, in which the chairman, Mr. Murray, and Mr. Bennett took part.—In the absence of the author, Mr. Harting detailed the chief points of interest in a paper by Mr. George Sim on the anatomy of the sand grouse (*Syrrhaptes paradoxus*), and the habits of this bird as observed on the sand hills of the coast of Aberdeenshire. A comparison was made of the sternum and the alimentary organs with the same parts in the pigeon and red grouse.

Chemical Society, December 5.—Dr. W. J. Russell, F.R.S., in the chair.—The following papers were read:—Compounds of phenanthraquinone with metallic salts, by Prof. F. R. Japp, F.R.S., and Mr. A. E. Turner. The authors have obtained several double compounds of phenanthraquinone with metallic salts, viz. $C_{14}H_8O_2$, $ZnCl_2$, crystallizing in dark, reddish-brown needles; $(C_{14}H_8O_2)_2$, $HgCl_2$, crystallizing in red, obliquely truncated prisms; and $(C_{14}H_8O_2)_2$, $Hg(CN)_2$, crystallizing also in red forms. They have prepared a similar compound from mercuric chloride and β-naphthaquinone, but could not obtain double compounds from benzoquinone, α-naphthaquinone, anthraquinone, diacetyl, or benzil. It would, therefore, appear that compounds of this class are derivable only from orthoquinones, and not from paraquinones or open-chain α-diketones. The intense colour of the double compounds indicates that in them the quinone preserves its distinctive character. In this respect they differ from the colourless compounds of the orthoquinones with sodium hydrogen sulphite, which, inasmuch as their formation involves reduction, are to be regarded as quinol derivatives.—Action of aldehydes and ammonia on α-diketones, by Mr. G. H. Wadsworth.—Phenylhexamethylene derivatives, by Dr. F. S. Kipping and Prof. W. H. Perkin.—Diphenylfurfuran, by Prof. W. H. Perkin and Dr. A. Schloesser.

Royal Microscopical Society, November 13.—Dr. C. T. Hudson, F.R.S., President, in the chair.—The Rev. Armstrong Hall exhibited a Bacillus from urine, which closely resembled *B. tuberculosis*.—Mr. Hardy exhibited and described a little apparatus which he had devised for the purpose of photographing an object under the microscope, without having to alter the position of the instrument in any way. He had originally made it in metal, but had found it too heavy; the one now before them was made of wood, and weighed about one ounce, the cost being nothing at all beyond the trouble of making it.—Mr. Watson exhibited and described a new pattern microscope for students (the "Edinburgh student's microscope"), and a student's petro-

logical microscope made upon the same lines; also, a small box for holding slides, for which a patent had been obtained by Mr. Moseley, its inventor. The slides were held in flat trays in the usual way, but they were so arranged that, upon opening the front of the box, the trays were drawn forward so as to form a series of layers overlapping sufficiently to expose the labels at the front end of each row, and enabling the position of any particular slide to be seen without the necessity of removing the trays in search of it.—Mr. Crisp exhibited apparatus by which it was proposed to convert a microscope into a microtome by placing the embedded substance in the lower end of the tube, and cutting sections by means of a blade fitted to move upon the stage plate.—Mr. J. Mayall, Jun., described the various microscopes and accessories which he had examined at the Paris Exhibition, pointing out that, whereas at former International Exhibitions most of the best makers in England, America, and other countries were exhibitors, on this occasion they had been rather conspicuous by their absence. The French opticians were fairly well represented as to numbers, but the instruments they exhibited were for the most part of the old, not to say antiquated, types. He had seen very little that was new in the matter of design.

Zoological Society, December 3.—Mr. Osbert Salvin, F.R.S., Vice-President, in the chair.—The Secretary read a report on the additions that had been made to the Society's Menagerie during the month of November 1889.—An extract was read from a letter received from the Rev. G. H. R. Fisk, concerning some specimens of *Bipalium kewense*, which he was keeping in captivity at Cape Town.—Mr. Henry Seebohm exhibited and made remarks upon some specimens of new or rare species of birds lately received from the Bonin Islands, North Pacific.—Mr. Sclater exhibited and made remarks on an egg of the crested screamer (*Chauna chavaria*), from the collection of Mr. J. J. Dalgleish.—Mr. F. E. Beddard read the first of a series of contributions to the anatomy of Picarian birds. The present communication treated of some points in the structure of the hornbills (*Bucerotidæ*), particularly of the syrinx, and of the muscular anatomy of these birds.—Mr. Beddard also read a paper upon the anatomy of Burmeister's cariama (*Chunga burmeisteri*), and pointed out the differences between this form and *Cariama cristata*.—Mr. G. W. Butler read a paper on the relations of the fat-bodies (subperitoneal and subcutaneous) of the Sauropsida. The author showed that a consideration of the subperitoneal fat-bodies appeared to throw light on the condition of the abdominal membranes in the monitors.—A communication was read from the Rev. H. S. Gorham, containing descriptions of new species of the Coleopterous family Erotylidæ from various localities.—A communication was read from Mr. L. Taczanowski, containing the description of a new warbler of the genus *Locustella* from Corea, which he proposed to call *Locustella pleskei*.—Mr. Oldfield Thomas pointed out the characters of a new mungoose, allied to *Herpestes albicaudatus*, which he proposed to call *H. grandis*. The type specimen (a skeleton) had been obtained by Mr. T. E. Buckley in South-East Africa.

STOCKHOLM.

Royal Academy of Sciences, December 11.—The Asco-ceratidæ and the Lituitidæ of the Upper Silurian formation of Gotland described, by Prof. G. Lindström.—Researches on the constitution of the spectra of emission of the chemical elements, by Dr. T. R. Rydberg.—On the observations at the Observatory of Upsala to determine the equinoctium of the spring 1889, by Dr. K. Bohlin and C. Schulz-Steinheil.—Definitive elements of the orbit of the comet 1840, by C. Schulz-Steinheil.—On the ores and minerals of the Gellivard district, especially the apatite, by Herr A. Sjögren.—The English edition of the atlas of fac-simile maps, by Prof. A. E. Nordenskiöld, exhibited by himself.—On the conductibility of snow, by Dr. S. Hjältström.—On the influence of the averting force of the telluric rotation on the movement of the air, by Dr. N. Ekholm.—A large collection of mosses from Japan, Korea, and East India, presented to the State Museum by Captain S. Ankarcrona, R.N., and determined by Dr. W. Brotherus, of Helsingfors, and by Dr. Carl Müller, in Halle, exhibited by Prof. Wittrock. On the recently-published first part of the second supplement to C. F. Nyman's "Conspectus floræ Europeæ," by Prof. Wittrock.—Echinologica, by Prof. S. Lovèn.—Some morphologic researches on the arteries of the brain of the Vertebrata, by Herr A. Klinkowström.—Derivatives of ortho-amido-benzyl alcohol, ii., by Dr. G. H. Söderbaum and Prof. Widman.—On distri azol combinations, by Dr. Bladin.—On naphtoe acids, by Dr. Ekstrand.—Derivatives of sulphate of ammonium, by Herr O. S. Hector.—Demonstration of some theories of Poincaré, by Herr de Brun.

DIARY OF SOCIETIES.

LONDON.

SATURDAY, DECEMBER 28.

ROYAL INSTITUTION, at 3.—Electricity (adapted to a Juvenile Auditory): Prof. A. W. Rücker, F.R.S.

TUESDAY, DECEMBER 31.

ROYAL INSTITUTION, at 3.—Electricity (adapted to a Juvenile Auditory): Prof. A. W. Rücker, F.R.S.

WEDNESDAY, JANUARY 1.

SOCIETY OF ARTS, at 7.

THURSDAY, JANUARY 2.

ROYAL INSTITUTION, at 3.—Electricity (adapted to a Juvenile Auditory):

FRIDAY, JANUARY 3.

GEOLOGISTS' ASSOCIATION, at 8.

SATURDAY, JANUARY 4.

ROYAL INSTITUTION, at 3.—Electricity (adapted to a Juvenile Auditory): Prof. A. W. Rücker, F.R.S.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

The Bala Volcanic Series of Caermarthenshire and Associated Rocks: A. Harker (Camb. University Press).—The Popular Works of Johann Gottlieb Fichte, 2 vols.; translated by Dr. W. Smith (Trübner).—Astronomy with an Opera-Glass: G. P. Serviss, 2nd edition (Appleton).—Logic Taught by Love: M. Boole (Edwards).—The Collected Mathematical Papers of Arthur Cayley, vol. ii. (Camb. University Press).—Aperçu des Travaux Géographiques en Russie: Baron N. Kaulbars (St. Pétersbourg).—Magnetic and other Physical Properties of Iron at a High Temperature: Dr. J. Hopkinson (Trübner).—On a Fossil Fish: M. Browne (Leicester).—Journal of the Chemical Society, December (Gurney and Jackson).—Brain, Part 47 (Macmillan).—Proceedings of the Geologists' Association, vol. xi. No. 5 (Stanford).—The Prevention of Measles: C. Candler (K. Paul).—Lectures on the Religion of the Semites: W. Robertson Smith (Edinburgh, Black).—Le Temps de Pose: A. de la Baume Pluvinel (Paris, Gauthier-Villars).—Manual de Phototypie: M. G. Bonnet (Paris, Gauthier-Villars).—The Proceedings of the Linnean Society of New South Wales, vol. iv. Part 2 (Sydney).—Internationales Archiv für Ethnographie, Band ii. Heft 5 (Trübner).

CONTENTS.

THURSDAY, JANUARY 2, 1890.

THE BERMUDA ISLANDS.

A Contribution to the Physical History and Zoology of the Somers Archipelago. With an Examination of the Structure of Coral Reefs. By Angelo Heilprin, Curator-in-Charge and Professor of Invertebrate Palæontology at the Academy of Natural Sciences of Philadelphia, &c. With additions by Prof. J. P. McMurrich, Mr. H. A. Pilsbry, Dr. George Marx, Dr. P. R. Uhler, and Mr. C. H. Bollman. (Philadelphia: Published by the Author, 1889.)

THIS work is mainly the outcome of researches concerning the physical history, geology, and zoology of the Bermudas, which were accomplished under the auspices of the Academy of Natural Sciences of Philadelphia in the summer of 1888. The author's principal object was to satisfy his own mind on certain points connected with the structure of coral reefs, and but little zoological work was contemplated. Fortunately, however, the collection of zoological material proved more extensive than was expected, and in this respect Prof. Heilprin was greatly assisted by the students who accompanied him.

After a pleasant chapter of "general impressions," the author gives the results of his examination of these islands, and then proceeds to make such a vigorous attack on the views advanced by Agassiz, Murray, and their followers, concerning the origin of coral islands, that those attacked may be pardoned if they regard him as an apostle of the old belief.

Coming from the pen of Prof. Heilprin, this volume will, however, be welcomed by both sides in the controversy, but he must expect from his opponents an energetic reply to some of his criticisms, and an unmistakable dissent from some of his conclusions. Thus when the author asserts that the existence of an atoll in the present position of the Bermudas is not demonstrable, and that we have yet to learn to what form of coral structure these islands belong, he is at variance with most other authorities on the subject; and it becomes at the same time a little difficult to follow him in his conclusion that the results of his researches go to sustain the atoll-theory of Darwin. However, laying this difficulty aside, and accepting the fact, fairly established in this volume, that these islands have undergone recent movements, first of upheaval and then of subsidence, we may ask: "Of what use is this double testimony to any theory, whether of upheaval or of subsidence, unless a direct connection is first established between the form of a reef and the character of the movement?" The direct testimony of a single atoll that can be proved to have grown in a stationary area will, unless this connection be established, far outweigh the presumptive evidence derived from a slight subsidence of every atoll in the Indian and Pacific Oceans.

Dr. Rein, in the instance of the Bermudas, was the leader of one of the early skirmishes in this controversy, and it was to his description of these islands that the opponents of the atoll-theory of Darwin pointed in support of their views. They miss, therefore, in this book, any special exposition on the author's part of the relation of his own views to those of Dr. Rein. They also will fail to see how Murray's explanation of the origin of the inner basins of the Bermudas by solution can be met merely by a statement of contrary conviction unsupported by experimental proof. Nor will they agree with Prof. Heilprin's assertion that the recent memoir of Agassiz on the Hawaiian Islands can scarcely be said to contribute materially towards the solving of the problem.

The author in this volume treats as absurd my attempt to show that a true conception of the relative dimensions of an atoll is necessary to understand the nature of the problem. I was aware that, if my meaning was not understood, I should lay myself open to some curious reflections, and therefore the point is further elucidated in my description of the Keeling Islands, in the *Scottish Geographical Magazine*. To Prof. Heilprin's inquiry as to how near are we brought to an understanding of the character of an atoll by a true conception of its relative dimensions, I would answer with the query, "How far are we misled from the truth by the woefully-distorted sections of atolls that are employed by lecturers and by the authors of text-books?" Let me cite a single instance—that of Darwin's section of the Great Chagos Bank, which gives that atoll (which is 76 miles in width and 40 to 50 fathoms deep) the relative dimensions of a soup-plate. Some go further, and draw, with a free hand, a deep, saucer-shaped section of such reefs. Illustrations of this kind practically beg the question at the start, if we are arguing in favour of the theory of subsidence. The mind is at once informed by the eye that there is a deep basin to be accounted for, whereas a section on a true scale would exhibit no appreciable depression. In the exaggeration of the relative depth of an atoll is concerned the very essence of the problem, and a side-note cannot remove the impression made by a false section on the mind. Our conception of the problem can scarcely be assisted by a section of an atoll representing in the lagoon greater oceanic depths than the *Challenger* ever plumbed.

Passing from these controversial matters to the zoological section of this volume, we find a very interesting chapter on the relationship of the Bermudian fauna. The number of known species of marine Mollusca has been increased from 80 to about 170, none of the eleven species peculiar to Bermuda having been described before this exploration. Strangely enough, though "overwhelmingly Antillean in character," the marine Mollusca include a Pacific element. The land mollusks have been increased from about twenty to thirty species, of which eight appear to be confined to these islands; but, in explaining the mode of transport of the non-peculiar species, the author scarcely seems to have laid sufficient importance on the transporting agencies of commerce. A remarkable fact noted in connection with the Bermudian crustaceans is the occurrence of three macrurans—*Palæmonella tenuipes*, *Palæmon affinis*, and *Penæus velutinus*—hitherto only recorded from the Pacific. Prof. Heilprin arrives at some interesting conclusions in this chapter, and perhaps the most important one is connected with the large proportion of peculiar forms amongst the land-shells, a circumstance which is pointed to as evidence not only of the antiquity of a portion of

the fauna, but also of its derivation from some pre-existing fauna in those islands. Much other zoological matter is to be found in this volume, though only a portion of the collections are here described. We are informed, however, that a great deal of systematic work still remains for the naturalist in the Bermudas, and Dr. Uhler, in respect of the insects, avers that much arduous collecting, particularly of the less conspicuous kinds, is still needed.

I do not know whether any argument for the considerable antiquity of the Bermudas from the character of the fauna has been advanced before. At all events, Prof. Heilprin's valuable suggestion opens up a line of inquiry in the case of coral islands generally, which might be pursued with profit. From investigations of the coral phenomena alone, I arrived at the conclusion that Keeling Atoll has a life-history of from 15,000 to 20,000 years, and that it is now in the last quarter of its existence. If this coral island is a type, then atolls must possess a high antiquity; and, taking our cue from Prof. Heilprin, we may ask whether, in the fauna and flora of a typical Pacific or Indian Ocean atoll, there is anything to suggest that they are derived from a pre-existing order of things. Confining ourselves to the flora, we find that oceanic atolls are mostly characterized by Hemsley as possessing no endemic element amongst their plants. Yet some of these large atolls must have once engirt, according to the theory of subsidence, a mountainous island possessing an upland flora, and, as in the case of the Fijis, not a few peculiar species. The islands formed on the encircling reef, just like the coral islands that often front the shore of a mountainous island in the Western Pacific, would possess, in addition to the common littoral plants, a number of plants derived from the slopes of the adjacent island. How comes it, then, that, if these large groups of oceanic atolls mark the disappearance of mountain-ranges, we find no sign of the vanished upland flora amongst the common littoral plants that are now brought by currents, winds, and sea-birds to every atoll? The Island of Tahiti could hardly disappear beneath the ocean without leaving a Tahitian impress on the flora of the surviving atoll. A similar reflection often occurred to me whilst on the Keeling Islands.

In conclusion, I would remark that partisanship in matters of scientific dispute cannot affect the value of this work by an American naturalist on one of the oldest of British possessions. The book is illustrated with several beautiful phototypes of general views in the islands, as well as of the æolian formations and of the coast scenery; and seventeen lithographic plates accompany the zoological descriptions. H. B. GUPPY.

THE USEFUL PLANTS OF AUSTRALIA.

The Useful Plants of Australia (including Tasmania). By J. H. Maiden, F.L.S., F.C.S., &c. (London: Trübner and Co. Sydney: Turner and Henderson. 1889.)

ALTHOUGH designed in the first instance as a hand-book to the specimens in the Technological Museum at Sydney, this work in its present form is really a concise text-book treating of "all Australian plants which, up to the present, are known to be of economic value, or injurious to man and domestic animals."

The literature of Australian economic botany may be said to date from the Great Exhibition of 1851. Owing, however, to the unsettled nomenclature of Australian plants previous to the publication of the great "Flora Australiensis," by Bentham and Mueller, the properties of the same plant were often found described under numerous botanical names. The publication of the "Flora," and the subsequent issue of Baron Mueller's "Census of Australian Plants" (with annual supplements), have now rendered species names easily accessible to workers in all parts of Australia, and the ground is well prepared for such a publication as that which lies before us. It is a bulky volume of 700 pages, well arranged, well got up, and furnished with an excellent index of botanical names, and also one of vernacular names. As Mr. Maiden reminds us, this is the first attempt made to grapple with the economical botany of Australia. He has wisely followed Baron Mueller in all essential details of classification, and due credit is given throughout the book to this learned and indefatigable worker, now, the greatest living authority on all that relates to Australian vegetable life. The arrangement of subjects has been adopted as that found most convenient in the Museum. This is not, perhaps, the best arrangement for a text-book, as it involves considerable repetition of names and synonyms under each section; but on that point we are not disposed to quarrel with the author. It opens, with human foods, and food adjuncts; and these are succeeded by forage plants, drugs, gums, resins and kinos, oils, perfumes, dyes, tans, timbers, fibres, and it closes with plants having miscellaneous uses not previously enumerated. A glance at the book shows very clearly, that if we except timbers, a description of which occupies about one-half the contents, the economic products of Australia are not of extraordinary importance. It is noticeable that the northern parts, where the flora is reinforced by representatives from the Malayan Archipelago and Southern Asia, yield most of the plants possessing medicinal properties. The genus Eucalyptus, comprising more than 130 species, yields excellent timber, kinos, and essential oils, and probably the chief economic products of Australia derived from native plants. Mr. Maiden has brought together practically all that is known about the industrial application of "gum"-trees, but we cannot now attempt to follow him.

Eucalyptus Gunnii (a large plant of which grows in the open air at Kew) yields a sweetish sap converted by settlers into an excellent cider. This, and manna, from *E. viminalis* and *E. dumosa* are probably the only food products derived from Eucalyptus trees. In the production of Eucalyptus oil (from *E. amygdalin* and *E. globulus*), Australia, it appears, has powerful competitors in Algeria and California, where gum-trees have been largely planted during the last twenty years. In the latter country, a large quantity is available as a by-product in the manufacture of anti-calcaire preparations for boilers.

The widely-spread Acacias of Australia, locally known as wattles, are hardly less useful than the gum-trees. Owing to the immense number destroyed for the sake of the bark used in tanning, the wattles in some districts are said to be threatened with extinction. Some whose leaves are eaten by stock are also becoming scarce. To counteract these influences, systematic attempts have been

made to plant wattles on a large scale. It is doubtful, however, whether, except in South Australia, such plantations will be ultimately successful. Gum arabic, of good quality, is yielded by various species of Acacia, but owing "to the great cost of unskilled labour in Australia, and the impossibility of utilizing the services of the aboriginals, it will never find its way into the world's market to any very great extent." Australian indigenous edible fruits, roots and leaves and stems, are apparently wisely left to the appreciation of "school-boys and aboriginals." Almost more important than food in a dry country is a constant supply of water. The aboriginal method of obtaining water from the fleshy roots of certain trees such as *Hakea leucoptera*, and from the stem of *Vitis hypoglauca*, is similar to that adopted in other countries, but Mr. Maiden has wisely given prominence to the fact, as the knowledge of it may be the means of saving the lives of many lost in the bush. Very few native Australian plants yield valuable fibres. The aboriginals appear to prepare their fishing-nets by chewing fibrous plants, and "this practice causes their teeth to be worn down to a dead level." In the same manner, we may add, the natives of Formosa prepare certain fibres for making clothes.

The best fodder grass of Australia is said to be *Anthistiria ciliata*, known as the "common kangaroo grass." There are several poison bushes (species of *Gastrolobium*, *Swainsonia*, and *Sarcostemma*) dangerous to stock so widely distributed as to render extensive tracts of country unoccupiable. These of late years have been reinforced by noxious weeds from other countries.

It is not to be supposed, however, that our knowledge of the economic uses of Australian plants is yet complete, and we are glad to learn that the author is actively engaged in observations that no doubt will be incorporated in a later edition. In the meantime, however, we cannot do better than commend this work as a most trustworthy guide in a handy form to the useful plants of Australia.

D. M.

MOUNT VESUVIUS.

Mount Vesuvius. A Descriptive, Historical, and Geological Account of the Volcano and its Surroundings. By J. Logan Lobley, F.G.S., &c. (London: Roper and Drowley, 1889.)

MANY people have been puzzled by the fact that there are so few English books on Vesuvius, especially of the descriptive type. The appearance of this work was looked forward to with ardent expectations, but it is doubtful whether it will fulfil them. Prof. Phillip's work was a remarkable one considering the short stay he made in Naples, but possessed those defects that all books must have which are written from little experience. Prof. Phillips wrote immediately after his visit. The first book of Prof. Lobley was prepared under similar circumstances, but apparently he has not re-examined the district for twenty years. Nearly every geologist on his visit to the type volcano of the world is attacked by a fever to write something about it —witness the 1300 or more books and articles in all languages referring to it—but a few months bring him safely through his complaint, and leave him satisfied that years of careful study on the spot will hardly qualify him to produce even a short description. This leads us to the main defects of the work, which spring from the author's want of personal observation, and the necessity of his obtaining information second-hand. Many recent authorities do not seem to have been consulted by Prof. Lobley. In consequence, he constantly makes statements that are incorrect or only partially accurate. Another fault to be found is the very incorrect and old-fashioned illustrations which would much bother a new-comer to the district with this work as a guide. Many of the crystal forms are incorrectly drawn, and in Plate xiv. dykes should not be represented as pipes branching out from the main chimney, but principally as radial sheets.

The accounts of the Phlegrean Fields, so far as they go, are very attractive, but lack that accuracy that a recent visit would have conferred. In describing Vesuvius, he mentions the library of vulcanology collected in the Naples section of the Italian Alpine Club, stating that 25,000 volumes are there preserved, which is more than three times the number. Neither will most people have had such a favourable experience of Vesuvian guides as Prof. Lobley. Yet altogether, the chapters on Vesuvius are the best part of the work, and are quite as much as a visitor with a couple of days to give to the mountain can comfortably absorb. The chapter on the geology of the volcano is clear and well written.

Unfortunately the book is spoiled—more perhaps than by anything else—by the author's views as to the causes of volcanic action. In the first place, the class of readers to whom the rest of the book appeals are not likely to possess sufficient physical and geological knowledge to be able to enter into the question, and to them chapter viii. is likely to prove a bore, and should they begin to peruse the book at this point, the effect will probably be that they will read no more. Even if it be supposed that the questions regarding the mechanics of the extrusion of igneous matter on the earth's surface are an easy matter of comprehension, the method of putting the subject into numbered paragraphs is much to be deprecated when the reader is not a specialist.

In the same way it is doubtful whether a description of rocks not occurring in the district is likely to be of use. Why mention the rare local rocks, "analcimite," "haüynophyre," "tholeite," &c., while "gabbro," "diorite," "syenite," are neglected?

The chapter on the minerals of Vesuvius is little more than a catalogue of every one that can possibly be raised to a species; some being obtained by dissolving saline crust in water, and allowing the solution to crystallize—a method that is hardly justifiable. Of far greater interest would have been a chapter on the general mode of occurrence, origin, &c., of the principal species, their characters being left to the systematic treatises on mineralogy.

The book is neatly got up and well-divided into separate chapters, so that the traveller, who will make most use of it, can easily turn up to a short account of any particular locality or subject. The language is clear, and not overburdened by petrological or other very learned words. Altogether, putting aside the above-mentioned blemishes, the work is likely to be of much use in leading travellers to observe for themselves one of the most interesting of geological phenomena.

OUR BOOK SHELF.

Index of British Plants, arranged according to the London Catalogue (Eighth Edition), including the Synonyms used by the Principal Authors, &c. By Robert Turnbull. Pp. 98. (London: George Bell and Son, 1889.)

THIS alphabetical synonymic list of British flowering-plants and vascular Cryptogamia is similar in general plan to that which was published about a year ago by Mr. Egerton-Warburton, which we noticed at the time of its issue (NATURE, vol. xl. p. 306). The author uses as a basis the last edition of the London Catalogue, and gives the synonyms of all the species that are described under different names in "English Botany," Bentham's "Handbook," Babington's "Manual," Hooker's "Student's Flora," "British Wild Flowers," Lindley's "Synopsis," Hooker and Arnott's "British Flora," Withering's "Arrangement," Notcutt's "Hand-book," and Hayward's "Pocket-book." The author has carried out his task very carefully, and has added an English name for each species, and given at the end a list of English names in alphabetical order. Two things lately have combined to cause considerable change in plant-names, the revision and redescription of the genera by Bentham and Hooker, and the increased attention which has been paid in tracing out priority by Mr. Daydon Jackson and Mr. Britten in England, and by Ascherson, Nyman, and many other writers on the Continent. We have noted a few slips in turning over the pages. For instance, there are only two native species of *Achillea*, not five—*decolorans*, *serrata*, and *tanacetifolia*, being manifest introductions. No wonder the author has not been able to refer some of the older bramble names to their London Catalogue synonyms. *Guntheri*, Bab., and *saltnum*, Foche, are both synonyms of the plant called *flexuosus* in the London Catalogue. The book will be found useful to many collecting botanists scattered up and down the country who have been puzzled to understand what was intended by many of the newly-introduced names. J. G. B.

Practical Observations on Agricultural Grasses and other Pasture Plants. By William Wilson, Jun. (London: Simpkin, Marshall, and Co., 1889.)

MR. WILSON tells us that "agriculturists have allowed themselves to run too much after a channel of indoor investigations." We do not know that this has been a fault in agriculturists, and are not convinced of the fact. Mr. Wilson appears to have omitted to acquire one important accomplishment in a writer on any subject—namely, the power of writing intelligibly. He tells us that "soil may be described as earthy matter on the surface of the globe"; that "climate has been described as a very complex matter, depending on a great variety of conditions"; but he does not say by whom it has been so lucidly "described." We are told that "sweet-scented vernal grass is one which most writers on grasses give a place as a useful grass, but not very definite as to what place it belongs, as it is not very readily eaten in some parts where there is a considerable quantity of it." Speaking of rough-stalked meadow-grass, he says:—"The Rev. J. Farquharson, F.R.S., mentions in his paper, which I have previously spoken of, as having cultivated it successfully on such soil, testifies as to the fondness of animals—both cattle and horses—for it, both as pasture and hay." Again, he informs us that "the fact has been pretty well borne out that a great fault has been to look at cultivation too much in the light of a matter which has been thoroughly investigated, when in reality it has little more than reached its infancy." Now, with all respect to Mr. Wilson, it appears to us to be mere cant to talk of the most ancient of all arts as having only reached its infancy. The style in which this little eighteen-penny book is written is poor and obscure, and the above quotations may be considered as fair samples of it. For instance, the eye falls by chance on the following passage (p. 70):—"The results of my observations have led me to the same conclusion as Mr. Sinclair—am of opinion that a mixture of it (*sic*) on dry soil would prove satisfactory, but should not be sown on clay moist soil." That this work should have reached a second edition is certainly strange, and appears to indicate that the agricultural palate is, as yet, particularly fresh. It must require a good deal of open-air exercise to enable a reader to digest Mr. Wilson's crudities.

W.

The State. Elements of Historical and Practical Politics By Woodrow Wilson, Ph.D., LL.D. (Boston, U.S.A.: Heath and Co., 1889.)

THIS work may be regarded partly as a text-book of political science adapted to the education of the young, partly as a repertory of what the writer calls "governmental facts," useful to readers of all ages. In the first part of his task Mr. Wilson has encountered great difficulties. He has no predecessors in whose steps to follow. Also the loose mass of facts and opinions which make up what is called political science does not admit of being compressed with safety. Again the class to whom Mr. Wilson offers a highly concentrated intellectual pabulum are little able to assimilate this species of nutriment even in its most digestible form. The young man, says Aristotle is not fit to be a student of political science. These difficulties appear to have been surmounted by Mr. Wilson better than might have been expected. He avoids the dogmatism to which short catechisms are liable. For instance in his section on the probable origin of government he does not rule that the earliest constitution of the family was patriarchal, or "matriarchal," as we believe it is now the fashion to say. While inclining to the former view he presents also the latter; and gives references by the aid of which the enquiry can be pursued. He stimulates curiosity and affords the means of gratifying it. The "evolution of government" is traced from the origin of the Aryan family through the changing types of Greek and Roman governments. This "institutional history" is somewhat dry; but the writer expects that the topical skeleton furnished by him will be clothed upon by the lessons of the intelligent teacher. Coming to modern times, we find a description of the principal pieces of political machinery which are now in use in the civilized world. This compilation seems to serve the purpose of a sort of magnified "Whittaker." If anyone who has not exhausted the subject of Home Rule wishes to refresh his memory as to the relations between Austria and Hungary or Sweden and Norway, he can here look out, as in a political dictionary, the main facts. We come nearest to the "practical politics" announced in the title in the chapter which discusses what are the proper objects of government. "This," says Mr. Wilson with much good sense, "is one of those difficult problems upon which it is possible for many sharply opposed views to be held apparently with almost equal weight of reason . . . It is a question which can be answered, if answered at all, only by aid of a broad and careful wisdom whose conclusions are based upon the widest possible inductions from the facts of political experience in all its phases." Mr. Wilson's solution of what Burke has called the "finest problem in legislation" is thus stated:—"It should be the end of government to *accomplish the objects of organized society*.... Not licence of interference on the part of government, only strength and adaptation of regulation. The regulation which I mean is not interference, it is the equalization of conditions, so far as is possible, in all branches of endeavour." Perhaps this teaching would have been more impressive if the writer, condescending to particulars, had discussed pretty fully any one question such as whether in any assigned country, the railways ought to be managed

by the state. Once more however we admit that the scope and limits of his work have imposed upon him almost insuperable difficulties.

Introductory Lessons in Quantitative Analysis. By John Mills and Barker North. (London: Chapman and Hall, 1889.)

THIS book of eighty-five pages is the first part of a larger work by the same authors, which will shortly be published. It is designed mainly for the use of "students in evening classes who have but little time to spare in acquiring such knowledge," and also to be of service for the Science and Art Department examination, as well as those of London University. The descriptions contained in the three chapters constituting the book, and which treat of preliminary operations, gravimetric analysis, and volumetric analysis, respectively, are meagre in the extreme, and lack many details essential to a primer. Slips and loose statements are numerous. For example, the student is led to infer that the ash of *any* of Schleicher and Schüll's filter-papers is negligible. Lead is estimated by means of "bichromate of potash," which is formulated as K_2CrO_4. On p. 62 the authors assert that "Normal solutions of *univalent* substances like iodine, silver nitrate, sodium chloride, &c., contain *their molecular* weight in grams in one litre." Whatever be the meaning attached to this, it is in no way confirmed by what follows on p. 63—namely, that "The *atomic* weight of iodine being 126·5, a normal solution would contain *this* number of grams in one litre."

The general scheme of work set out in the lessons is satisfactory, and if carefully elaborated might be useful. In its present condition, however, the effect of the book on the beginner cannot be other than confusing.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Note on a Probable Nervous Affection Observed in an Insect.

WHILST walking in the garden one bright September morning, my attention was called to a moth fluttering in a peculiar manner on the ground; it kept going round and round in a circle, running with its feet on the stones, its wings meanwhile being in rapid motion.

I captured the insect, which proved to be a quite fresh specimen of a male *Orgyia antiqua* (vapourer moth), of which there were many in the garden.

I replaced the insect without injury on the path, and watched it more closely.

The movements of the wings were irregular, convulsive, and very rapid in character; the feet and body were also in rapid movement, resulting in a circular motion of the whole insect from right to left—that is, in the same direction as the movements of the hands of a watch.

I again captured the insect, thinking that perhaps one of its antennæ might have been injured; but on careful examination with a hand lens, I could detect no lesion nor the presence of any parasite which might account for the condition.

I again placed the insect on the path, when it immediately began to rotate as before. It seemed unable to keep still, though evidently trying to do so.

Occasionally it would wedge itself in between two or more small stones, with its head downwards, and the under surface of its body upwards, its wings resting on the stones below; in this position it appeared to obtain some relief, as the movements were less continuous, though every breath of wind caused a convulsive twitching of the wings and body. On one occasion a leaf fell upon the insect whilst wedged in, causing a very violent convulsion of the whole insect, by which it was jerked quite out of its retreat, when the gyrating movements at once began again.

I tried stroking the antennæ with the point of a pencil, but this had no effect, nor could I obtain cessation of movement by stroking the body or the wings; on the contrary, when the insect was wedged in each touch caused a convulsion, varying with the intensity of the stimulus applied.

These movements continued without interruption for fully forty minutes, the insect gyrating in a space about a foot square. At the end of that time I placed it upon a piece of smooth paper, when the movements became more rapid and the gyrations less ample, it completing a turn in much less time than on the stones, owing, no doubt, to there being no projections on the paper to cause the insect to deviate.

I then placed it in a shallow cardboard box in the full sunlight, but protected from the wind. In this way the convulsive movements were less intense and less frequent; the insect, however, was often jerked over on to its back, then, after a struggle or two, would right itself, and begin to go round. When, however, it managed to press the top of its head against the side of the box, so that its antennæ were pressed between the head and the side of the box, all movement ceased till some external stimulus again set it in motion.

At the end of one hour the insect seemed quite exhausted, a strong stimulation being required to develop one convulsion.

On examination I found that it had worn away, in its movements, all its legs with the exception of the left hind leg, which was apparently pretty intact, and had broken both its wings on the right side, so that the greater part of them hung useless over its body.

After a few more violent convulsions, the upper wing of the right side was broken off, and the insect now began to revolve from left to right, owing, I suppose, to the movements of the left leg; the others being reduced to mere stumps would have little power of propelling the insect.

About twenty minutes later, during a convulsion, the right hind wing was broken off.

Shortly afterwards I noticed that the convulsive movements of the antennæ, which had been slight up to that time, were much increased; indeed, they were moving so rapidly as to have the appearance of two small black wings.

One hour and fifty-five minutes after I first noticed the insect all convulsions had ceased; no stimulus could excite any; the moth was dead.

Conclusion.—The insect, suffering from no apparent injury, and being attacked by no internal or external parasite, was, I believe, suffering from some nervous lesion. I was unfortunately unable to examine the insect microscopically to ascertain if the nervous centres exhibited any pathological characters.

E. W. CARLIER.

Does the Bulk of Ocean Water increase?

THE idea was, I think, suggested by myself, and has been referred to with approval by Mr. Jukes-Browne, that much of the water on the surface of the globe was originally occluded in the molten interior, and has been emitted by volcanic action in the course of ages. Mr. Mellard Reade argues against this, that the moon is covered with volcanic craters, and yet has no water on its surface, and that if the accumulation of surface water has followed volcanic action on the earth, it ought likewise to have done so on the moon. He concludes:—"At all events, it seems a reasonable question to ask why oceans should be supplied with water from the perspiring pores of mother earth, while her offspring, the moon, is so dry as to have absorbed into herself all evidence of any aqueous envelope that may have formerly existed."

It is a singular coincidence that one possible answer to this objection is suggested by a notice in the "Astronomical Column" of the same number of NATURE which contains Mr. Reade's letter. Therein Prof. Thury attributes apparent changes in the aspect of a lunar crater to the melting of snow or ice around it. Neither is he the only selenologist who thinks that those crater-rings consist more or less of frozen water. If they do so, then there *is* water on the moon, although in a solid state. On the other hand, Proctor, in his work on the moon, says that her

surface is more nearly black than white, which seems to render the existence of snow fields upon it less probable, unless they are covered with volcanic dust, as the end of a glacier usually is with rock *débris*.

But even if we take Mr. Reade's view, it is still conceivable that steam may have been the explosive agent in the moon's volcanoes, while her internal temperature was very high, and that the resulting water may have been subsequently absorbed after the body became cool, because the water would occupy less space within the interstices, which this theory of imbibition postulates, than the equivalent vapour did, when the temperature was high. The case of the earth would not be a parallel one, because it has not yet cooled.

Although not myself a selenologist, I have a suspicion that very little is known about the constitution of the moon; and that it is not even certain that its enormous craters are all of them really volcanic. It has been admitted by Prof. Darwin, in discussing the subject with Mr. Nolan, that on his view of the genesis of the moon it must have originally existed as a "flock of meteorites." These falling in during the later stages of the building up of its mass would have produced pits on a viscous surface, much like some of the craters.

At any rate it seems unsafe to rely upon arguments respecting the condition of the earth's interior, of which we know little, drawn from that of the moon's body, of which we know less.

Harlton, Cambridge. O. FISHER.

Exact Thermometry.

THE interesting experiments of Dr. Sydney Young, recorded in NATURE of December 19 (p. 152), seem to leave no doubt that the main part of the permanent ascent of the zero-point of a mecurial thermometer, after prolonged heating to a high temperature, is not due to compression of the bulb—rendered more plastic by the high temperature—by the external atmospheric pressure. Researches on the effects of stress on the physical properties of matter have convinced me that the molecules, not only of glass, but of all solids which have been heated to a temperature at all near their melting-point, are, immediately after cooling, in a state of constraint, and that this state can be more or less abolished by repeatedly heating the solid to a temperature not exceeding a certain limit, and then allowing it to cool again (it is not only the heating but the cooling also that is efficacious). It appears that the shifting backwards and forwards of the molecules, produced by this treatment, enables them to settle more readily into positions in which the elasticity is greatest and the potential energy is least.

This "accommodation" of the molecules, as Prof. G. Wiedemann and others have called it, is, as one might suppose, attended with alterations of the dimensions and other physical properties of solids, and is not confined to the release of molecular strain set up by thermal stress, but is extended to the strain set up by any stress whatever. As years roll on, the time of vibration of a metal pendulum gradually alters (and so, no doubt, do the lengths of our standard measures), the bulb of a thermometer diminishes in volume, a steel magnet parts with more or less of its magnetism, a coil of German-silver wire gains in electrical conductivity, &c. The changes in all these cases would probably be far less than they actually are if the temperature throughout the whole time could be maintained constant; but this last is not the case—heating and cooling goes on more or less every day. We may assist the effect of time by artificially increasing the range of temperature, but it would appear that we must not exceed a certain limit of temperature, which limit depends partly upon the nature of the substance and partly upon the stresses that are acting upon it at the time. Thus, the internal friction of a torsionally oscillating iron wire which has been previously well annealed may be enormously diminished by repeatedly raising the temperature to 100° C., keeping it there for several hours, and then allowing it to fall again. The amount of diminution of internal friction depends upon the nature of the wire, and on the load which there is at the end of it (if the load exceeds a certain amount, the friction is increased instead of diminished). In attempting to "accommodate" the molecules in this manner the heating must, at any rate in some cases, be prolonged for several hours, and the substance should then be allowed to remain cold for a still longer period.

I have not had much experience with glass, but I think it probable that the settling down of the zero-point of an ordinary thermometer into its ultimate position could be very materially facilitated by the heating and cooling process mentioned above.

HERBERT TOMLINSON.

36 Burghley Road, Highgate Road, December 23, 1889.

Self-luminous Clouds.

WITHOUT venturing to call in question the occasional occurrence of self-luminous clouds, I may be permitted to relate an observation which seems to reveal a possible source of error in the records of such phenomena.

On June 14, 1887, about 10.45 p.m., I witnessed an appearance over the north-north-west horizon which struck me as very remarkable. Amidst the strong glow of twilight a few fragments of cirrus cloud shone with a pure white light having so much the character of phosphorescence that it was difficult to believe the objects were not self-luminous. Looking out again an hour later, I found no trace of bright clouds, but in their place were small bands of cirrus showing dark and grey against the feeble twilight that remained. I could not but conclude that the clouds in both instances were the same or similar, lit up by the direct rays of the sun at the time of the first observation, and having lost his rays at the time of the second observation. Had they been self-luminous they should have become brighter instead of darker as the twilight faded.

It has been suggested to me that the bright clouds seen at 10.45 p.m. may have owed their brightness, not to the sun's rays falling on them at the time, but to a temporary phosphorescence, the result of exposure to the sun's rays in the day-time, and that this temporary quality had died out in the interval between the two observations.

I think this explanation is unnecessary for the following reasons. In the first place, it is certain that if a cirrus cloud were present in the atmosphere at a sufficient height to catch the sun's rays at 10.45 p.m. of a midsummer day, it would appear as a bright object amidst the surrounding gloom. And, secondly, there can be nothing incredible in the presence of a cirrus cloud at that height, when the persistence of twilight proves the presence of atmospheric particles of some kind at a greater elevation still.

GEORGE F. BURDER.

Clifton, December 19, 1889.

Duchayla's Proof.

I HAVE read with much interest the new proof given by Mr. W. E. Johnson of "the parallelogram of forces," in NATURE of December 19 (p. 153), and regard it as deserving a place among the best proofs that have been given.

I think, however, that, in his criticism of Duchayla's proof, Mr. Johnson runs to excess, when he says, "To base the fundamental principle of the equilibrium of a *particle* upon the transmissibility of force, and thus to introduce the conception of a *rigid body*, is certainly the reverse of logical procedure."

Duchayla's proof only requires us to suppose the transmission of force by *strings*. A particle is unthinkable. In presenting to a learner the conception of three equilibrating forces acting on a particle, we cannot do better than represent the forces by pulls in strings, and the particle itself by the knot where the three strings are tied together. All the steps of Duchayla's demonstration that the resultant force is directed along the diagonal of the parallelogram can be presented in tangible form, with the aid of strings. I do not think this is an illogical or unnatural procedure.

J. D. EVERETT.

Belfast, December 23, 1889.

The Satellite of Algol.

THE results of Vogel's photographs as to the satellite of Algol are of great interest to your astronomical readers. The observations made at Greenwich tended to the same result, but were unfortunately intermitted before anything approaching certainty was arrived at.

Regarding it as certain that the variations of Algol are due to the interposition of a satellite, the question of the slight change

in its period and the much larger change observed in the period of another variable of the same class in Cygnus becomes important. Besides the possibility of a third disturbing body it may be remarked that the existence of the solar corona and perhaps other appendages of the sun suggests that a resisting medium may exist in the entire space traversed by Algol and its satellite at each revolution. Also if the influence of gravitation is propagated in time (with whatever degree of velocity) the very rapid angular motion of a satellite which performs a complete revolution in less than three days (and in another variable of this class in twenty hours) could hardly fail to exhibit traces of this time-propagation. The attractive force, in fact, would never act in the line joining the centres of the principal star and satellite, and the deviation would probably be perceptible. I hope some mathematical astronomer will take up the problem, and show what the effects of each of these supposed causes would be. W. H. S. MONCK.

16 Earlsfort Terrace, Dublin, December 21, 1889.

Maltese Butterflies.

IN reading Mr. Wallace's "Darwinism" I am reminded by his observations on Island fauna (p. 106) of the impressions made upon me by the natural productions of Malta. My time was so fully occupied that I had little opportunity of exploring the country districts. I paid one visit to the extraordinary ruins of a Phœnician temple at Hagiar Kim, and one to the curious islet in St. Paul's Bay. On the latter I noticed several strange thistles and a beautiful flower—something like a large pink or purplish Tutsan. On the barren wastes round Hagiar Kim many familiar wild flowers grew, but all seemed shrunk and shrivelled as compared with those of Britain. The only unfamiliar one was called by the natives "the English flower." It was a tall trefoil with a drooping yellow trumpet-flower (not at all papilionaceous in form), and grew plentifully by the edges of the dustiest roads—unlike anything I know in England.

I lived for some time at the Imperial Hotel, at Sliema, which has a somewhat extensive garden, in which I used to spend about half an hour every morning. During April and May it was very lovely. The oleanders were then in their richest bloom; a shrub like a gigantic heliotrope, both in flower and leaf, was frequented by myriads of humming-bird moths; there were a few strawberry-plants, the fruit of which was delicious, although even smaller than that of our own wild kind; but most attractive to me were the clumps of valerian and scabious which were haunted, just as at home, by crowds of butterflies. These included blues, coppers, wood-ladies, painted-ladies, red-admirals, tortoise-shells, and swallow-tails. All of these were smaller than their English relatives are, and much less brilliant in colour. The swallow-tails were especially dwarfed in their proportions. I am puzzled to account for their presence in Malta, as there is nothing like a marsh or a fen in the whole island, whilst in England they are only to be found in the district of the meres. Can any of your readers throw light on this mystery? I saw several of the larger hawk-moths. They did not seem to suffer in size, but even they were dimmer in their colours.

Hoping to get a general idea of Maltese entomology, I visited the University Museum—only to find a few cases of insects in which every specimen had been devoured by mites! GEORGE FRASER.

Leighside, Tunbridge Wells, December 22, 1889.

A Preservative.

I HAVE been very much troubled in conducting classes in mammalian anatomy by the want of a preservative medium which would retain the natural colour and texture of tissues, would impart to them no offensive smell, would be inexpensive, and easily handled. Various experiments with freezing, alcoholic, glycerine, and other media have all proven failures, and this fall I turned to experimentation upon the simplest and cheapest of all chemical reagents—water and table-salt. My entire success with these was so satisfactory that I shall, at the risk of telling an old story, state the experiments here.

I tried preserving squirrels in three strengths of salt solution, one of 5 parts by weight of salt to 95 of water, a second of 10 per cent. salt, and a third of 15 per cent. All gave satisfaction, but the 10 per cent. seems best, because the weakest solution in which putrefaction could not take place. Specimens placed in five times their bulk of this solution retain the natural flexibility of all the tissues; the peculiar look of nerve-tendon and blood-vessel against muscle is retained; the tint of muscle is faded somewhat by the solution of hæmoglobin from the blood, but it is still distinctly reddish; there is no putrefactive odour; all of this after four weeks standing in the laboratory.

This is so simple a preservative that I wonder that it is not in common use. H. LESLIE OSBORN.

Hamline University, St. Paul, Minnesota, December 7, 1889.

The Evolution of Sex.

IT is a fact well known to pigeon fanciers that the two eggs laid by pigeons almost invariably produce male and female. But no attempt appears to have been made to ascertain which of the two eggs produces the male, and which the female. The second egg is laid about twenty-four hours after the first. I have kept pigeons for seven or eight years, and have only met with one or two instances of the young birds, produced from the two eggs, being of the same sex. Recently I have made several experiments to ascertain if any relation exists between the order in which the eggs are laid and the sexes of the young birds produced. The results show that the egg first laid produces the female, the second egg the male. It may, perhaps, be well to give the experiments.

(i) Egg 1 of pair A produced a female; egg 2 was bad.
(ii) Egg 1 of pair B produced a female; egg 2 a male.
(iii) Egg 1 of pair B produced a female; egg 2 a male.
(iv) Egg 2 of pair B produced a male; egg 1 was bad.
(v) Egg 1 of pair C produced a female; egg 2 was bad.
(vi) Egg 2 of pair D produced a male; egg 1 was broken.

These experiments were made on white fantail shakers. A large number of experiments must be made to prove if this relation does exist; should it be found correct, an examination of the eggs and of the ovary of the parent might throw some light upon the "evolution of sex." M. S. PEMBREY.

Oxford, December 14, 1889.

Fighting for the Belt.

A FIGHT has been going on in my verandah for the last half-hour between two young birds—minas—with four birds of last season looking on.

Now the fight is just over. I have watched it throughout, and am positive that one of the on-lookers walked often round the combatants without interfering; and that another on-looker came, when he (or she?) could, and attacked one of the fighters. I say "came when he could," because the other on-looker prevented him if possible—even fighting to that end. It seemed to me very much as if two youngsters from different nests were fighting for the belt, and the parents looking on—the one complacently at her offspring's success, the other angry and breaking the rules of the ring to help the weaker. F. C. CONSTABLE.

Karachi, December 1, 1889.

The British Museum Reading-Room.

THE proper ventilation of this spacious room is a problem, surely not insoluble, but still awaiting solution. Is it not a serious grievance that to make use of one of the finest libraries in existence, means, for many, injury to health? Bad headaches and other ills, due to the stuffy and impure atmosphere which collects about the desks, are a common experience; and I know men who have given up going to the place on that account. For readers who live by work which can only be done there (some of whom are women), the matter is especially grave. Officials, again, will tell you that they often feel thoroughly done out after their day's work, which in itself is not generally severe. It seems to me the atmosphere improves after the lamps are lit; possibly owing to the upward current of heated air. If this were verified, it might offer a clue to improvement. The whole matter calls for thorough scientific investigation; and I would suggest, as a preliminary step, that analysis be made of the air (say) on a Saturday afternoon, with regard not only to its gaseous constituents, but also to micro-organisms, which are no doubt plentiful. A. B. M.

"*AMONG CANNIBALS.*"[1]

IN the year 1880, Mr. Carl Lumholtz—as he explains in the preface to the work the title of which is given below—undertook an expedition to Australia, partly at the expense of the University of Christiania, with the object of making collections for the zoological and zootomical museums of the University, and of instituting researches into the customs and anthropology of the Australian aborigines. His travels occupied four years, and the first part of that time he spent in the south-eastern colonies, South Australia, Victoria, and New South Wales. From November 1880 to August 1881 he was in Central Queensland, and at the latter date he began his first journey of discovery, in the course of which he penetrated about 800 miles in Western Queensland—the results, he says, in no wise corresponding to the hardships he had to endure. He then went to Northern Queensland, where he spent fourteen months in constant travel and study, his head-quarters from August 1882 to July 1883 being in the valley of what he describes as "the short but comparatively broad and deep Herbert River," which flows into the Pacific at about 18° S. lat. From his base on this river he made expeditions in various directions, extending in some instances to nearly 100 miles, and he repeatedly came in contact with savages who had never before been visited by a white man.

It is to the period spent by him in the camps of the northern aborigines that Mr. Lumholtz chiefly devotes attention in the present volume, and it would hardly be possible to praise too highly the manner in which he has recorded his experiences. In every part of his narrative he displays a remarkable power of keen and accurate observation, and he presents his facts in a style at once so fresh and so simple that from beginning to end the reader's interest is maintained. Hitherto students of anthropology in Australia have derived their materials mainly from the southern part of the continent. Mr. Lumholtz may almost be said, therefore, to have broken new ground, and it is ground which it was well worth while to break, for the northern aborigines—from an anthropological point of view—are even more interesting than the southern tribes. They are decidedly at an earlier stage of development, and many of them have been only slightly and indirectly influenced by the ideas of European settlers.

If there are any survivors of the school of Rousseau, who attributed so many fine qualities to "the noble savage," it would be wholesome for them to study what Mr. Lumholtz has to tell about the savages of Northern Queensland. A more unlovely picture than his description of these poor people it would hardly be possible to imagine. He went to Australia full of sympathy with the natives; when he left it, he found that his interest in

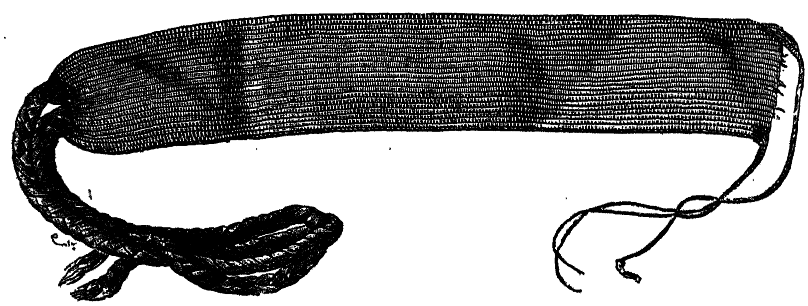


FIG. 1.—Brow-band from Central Queensland (⅓ size).

them was as deep as ever, but that his sympathy had nearly vanished. That they are cannibals is beyond doubt. Luckily, they do not take to white flesh; it has too salt a flavour for their taste. But native flesh, when they can get it, provides them with the meal they like best, and they are quite willing to talk freely about the parts which they consider the most delicious morsels. They are not only treacherous, but seem to have not the faintest idea that treachery is anything to be ashamed of. If anyone is kind to them, they at once mistake his motive: they fancy that his generosity springs from fear, and if this notion gets into their minds, it is time for their benefactor to look about him, for they will not scruple to kill him in order to obtain possession of his goods. Mr. Lumholtz found that, when accompanied by a party of natives, it was unsafe for him to walk in front; he had always to bring up the rear, and to keep every one well in view. At night, before going to sleep in his tent, he had to fire his gun as a reminder that he had the means of defending himself. For this weapon they had the most profound respect; also for his revolver, "the baby of the gun." The supreme ambition of the native is to have as many wives as possible, their number being regarded as a test of his wealth and importance. And he takes good care that they shall not earn his approval too easily. All the hard, disagreeable work has to be done by women, and when they excite the displeasure of their lords they may think themselves well off if they are not severely beaten.

In every way these savages are creatures of impulse. It is difficult for them to fix their attention on anything, and they can look ahead only a very short way. Fortunately for themselves, they have no intoxicating stimulants, but tobacco gives them intense delight, and it was chiefly by promising to reward them with small quantities of it that Mr. Lumholtz was able to secure their services. When they have a chance, they gorge themselves with food; and on a hot day they plunge like dogs into water they may happen to pass. At the approach of night they become timid, trembling at every sound they hear in the bush; but with sunrise all their fears are dispelled, and after they have become thoroughly awake—a rather slow process—they are ready for any pleasure that may come in their way. It is a happy moment for them when they discover a tree in which there is honey. This they eat with rapture; and Mr. Lumholtz says he has known cases in which they have lived upon it for three days in succession. If a savage finds such a tree, and is not able at once to take possession of its treasure, he marks the tree, and the mark will be respected by members of his own family or clan. There is, however, no conception corresponding to the idea of property, so far as anything claimed by strangers is concerned.

[1] "Among Cannibals: an Account of Four Years' Travel in Australia, and of Camp Life with the Aborigines of Queensland." By Carl Lumholtz, M.A. With Maps, Coloured Plates, and 122 Illustrations. (London: John Murray, 1889.) We are indebted to the kindness of the publisher for the use of the cuts reproduced in this article.

As the people live in small groups, they have, of course, the germs of social life; but more than this they can scarcely be said to possess. But they have aptitudes which have been naturally developed in the circumstances

FIG. 2.—Wallaby Hunt.

in which they spend their lives. They display extraordinary cleverness in climbing trees, and their sense of smell is so keen that it is invaluable to them when they are tracking wild animals. In various kinds of handiwork

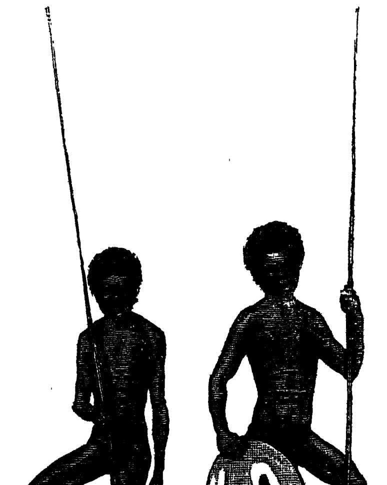


FIG. 3.—Peculiar position of natives resting.

they have considerable skill. Fig. 1 represents a brow-band of native workmanship ($\frac{1}{3}$ size). This specimen however, comes from Central Queensland. The Australians are generally supposed to throw the spear well, but Mr. Lumholtz never discovered any remarkable ability of this sort among the blacks of Herbert River. Fig. 2, represents a wallaby hunt, which he had an opportunity of seeing. He says:—

"Soon those who had remained behind spread themselves out, set fire to the grass simultaneously at different points, and then quickly joined the rest. The dry grass rapidly blazed up, tongues of fire licked the air, dense

FIG. 4.—Message stick, with interpretation of inscription.

clouds of smoke rose, and the whole landscape was soon enveloped as in a fog. I fastened up my horse and went into this semi-darkness, watching the blacks, who ran about like shadows, casting their spears after the animals that fled from the flames. But though many spears whizzed through the air, and though a large field was burned, not a single wallaby was slain."

Mr. Lumholtz often noticed natives resting in a most peculiar position, represented in Fig. 3. "They stood on one foot, and placed the sole of the other on the inside of the thigh, a little above the knee. The whole person was easily supported by a spear." This custom is said to

prevail among the inhabitants of the Soudan and the White Nile district.

A kind of sign language is occasionally used by the Australians. It consists of figures scratched on "a message stick" made of wood, about four to seven inches long, and one inch wide. Fig. 4 represents one of these sticks. It conveys a message from a black woman named Nowwanjung to her husband Carralinga, of the Woongo tribe. "Other message sticks," says Mr. Lumholtz, "are engraved with straight or circular lines in regular patterns as in embroidery; this has caused an entirely different view of their significance, which supposes them to be merely cards to identify the messenger. This view may be correct, but it is not corroborated by my experience on Herbert River."

Mr. Lumholtz secured a valuable collection of zoological specimens, and some of the best passages in his book are those relating to this part of his work. Fig. 5 represents

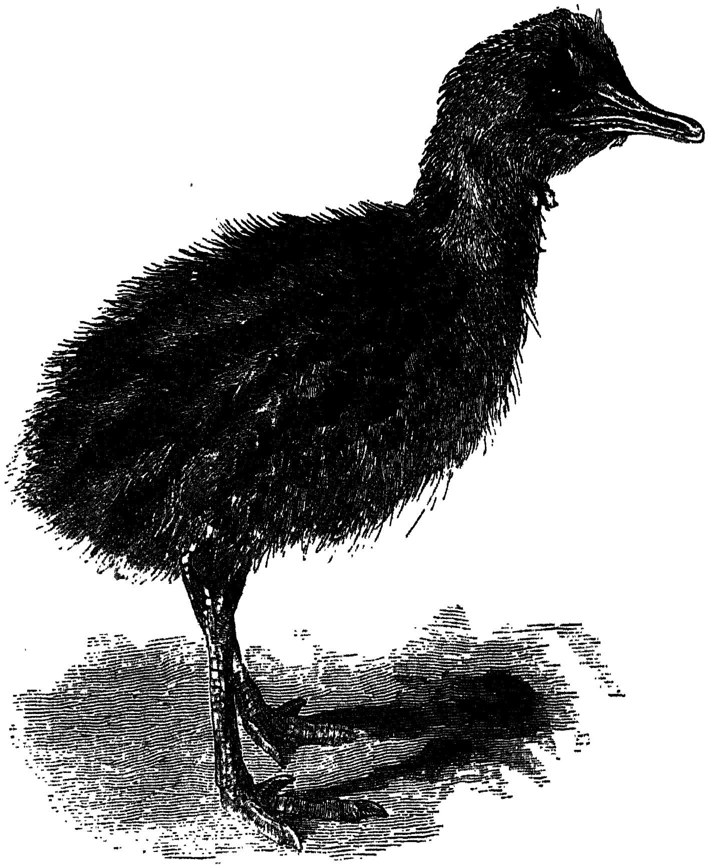


FIG. 5.—Young Cassowary.

a young cassowary, which the natives one day brought to him, with two eggs. He at once asked the natives to guide him to the nest, near which, in a bed of loose leaves, he placed the young bird, hoping to attract the old one. After the lapse of about ten minutes they suddenly heard the voice of the cassowary. This usually sounds like thunder, "but now, when calling its young, it reminded us of the lowing of a cow to its calf." Soon the beautiful blue and red neck of the bird became visible among the trees. The creature "stopped and scanned its surroundings carefully in the dense scrub, but a charge of No. 3 shot, fired from a distance of fifteen paces, laid it low." Six natives carried home the prize, which proved to be an unusually fine specimen of a male.

We cordially recommend this book to all who take an interest in anthropology and zoology, or in incidents of travel through unfamilar scenes. They will find in it much that cannot fail to give them genuine pleasure.

BRITISH EARTHQUAKES.

It is somewhat remarkable that the ordinary notion that Great Britain has a special immunity from serious earthquake phenomena, still very generally obtains credit. An explanation of this popular fallacy may perhaps be found in the simple fact that, on the average, few people living at any one time chance to have experienced any considerable shock; whilst in the case of those few—we except the many who were affected by the disastrous Essex earthquake five years ago—who have felt the sensation, as a momentary mental impression it has been soon forgotten. It should, however, by this time be more generally known and accepted that no part of the habitable globe is entirely exempt from seismic action, and that earth-tremors of considerable amplitude and intensity are by no means necessarily connected with volcanic disturbances, as was formerly supposed. When it is duly recognized that, at the lowest computation, 600 disconnected shocks are known to have taken place in this country during the present era, the popular belief respecting "our tight little island" may well be entirely shaken. This number includes many earthquakes of considerable magnitude, and the additional seismological

evidence of modern compilations furnishes the testimony that as many as six or eight minor shocks have occurred annually in recent years. In evidence of the prevalence of such phenomena in England, it should be also remembered that it was on this island that Prof. George Darwin first discovered the fact of the continuous microseismic vibration of the earth's crust.

The new edition of the late Mr. William Roper's excellent summary of the principal earthquakes that have been recorded in Great Britain and Ireland during this era, which has lately been issued,[1] bears witness both to the frequency of such phenomena, and, even more strikingly, to the great advance that has taken place within recent years in the study of seismology in Britain. The increased attention which has been devoted to the subject is doubtless partly due to the extensive shock which occurred in this country in 1884.

The famous Catalogue compiled by Robert Mallet will ever remain the cyclopædic work of reference upon which all subsequent earthquake catalogues will necessarily be based; and the name of Mallet, as the authority, naturally figures most extensively in Mr. Roper's list. Until recently, it may, indeed, be said that the work of Mallet, and of M. Alexis Perrey, of Dijon, stood almost alone as the historical register of seismic force in the world. Within the last few years, however, the valuable experimental work of Prof. Milne and others in Japan, and of numerous European and American seismologists, has been supplemented by several treatises devoted to British earthquakes alone. Some of these publications—as the detailed report of the great Essex earthquake, and Mr. E. Parfitt's Devonshire Catalogue—being issued in connection with particular areas, and by local scientific bodies, have had a restricted application; whilst others, as Prof. O'Reilly's catalogue, and the one just mentioned, have included the entire British Islands in their scope. It was the intention of the present writer, when engaged, in conjunction with Prof. Meldola, upon the Report of the East Anglian earthquake,[2] to furnish a full list of British earthquakes; but, from the quantity of material accumulated from very many sources, it was found that so extensive a catalogue grew entirely out of proportion to the purpose of a special monograph, and only those disturbances which had similarly caused structural damage were included in that memoir. These alone, however, number as many as sixty well-authenticated records, although Mr. Roper, in his catalogue, which, unfortunately, is very scanty in point of detail, omits fully 25 per cent. of these injurious shocks. But since his catalogue too modestly professes to include only "the more remarkable earthquakes," it is to be expected that numerous omissions might be noticed, and we could readily add to his list over two or three dozen records (both mediæval and modern) which fully equalled the average intensity of those he has included. In fact, while it may be said to form the most comprehensive list of British earthquakes that has yet been produced, it is incomplete, and it is much to be regretted that the compiler did not survive to finish his erudite undertaking, as is explained in a prefatory note by his son.

Mr. Roper has, in effect, unconsciously erred unduly on the side of moderation, since he includes most of the fabulous stories that belong to mediæval times, while he has omitted many important shocks. This recalls a somewhat strange incident in connection with the 1884 earthquake—namely, that more damage actually occurred in the out-of-the-way villages chiefly affected by the shock, than was ever reported in the London newspapers—a fact which does credit to the caution exercised by the daily press writers at the time. Too much, on the other hand, was made of the really slight but widely distributed shock which took place on May 30 in the present year, when no displacement of furniture nor stoppage of clocks then resulted; the experience being limited to the rattling of windows and the swaying of walls, as may be seen on referring to the summary which appeared in NATURE for June 6 (pp. 140–42).

Covering so considerable a period of history, and including so much subject-matter, Mr. Roper's work certainly deserved a more extended treatment than it has received. An introductory analytical chapter would have added considerably to the interest of such a catalogue, while a fuller elaboration and thorough editing would have advantageously extended the work beyond its unpretentious limit of fifty pages. The convenient method adopted by Mr. Roper of inserting a preliminary list of "principal authorities cited," is almost compulsory in such a work, for the purpose of establishing a code of abbreviations for subsequent use in the columns of the list; but the titles are generally given imperfectly or incorrectly, without the requisite details of publication, while the dates, where given, are not throughout those of the original, as they should be, but of later reprints. These and similar slight defects are inconvenient in an historical treatise, and we hope they may receive attention in the event of another edition of this interesting list being called for.

The total number of distinct earthquakes included in this catalogue—regarding the series of repeated shocks which sometimes take place within a brief period as a single record—amounts to 582, and an analysis of these records may be of interest here, as furnishing some slight indication of the chronological distribution of the chief seismic disturbances which have been accounted in British annals as having taken place within our area. They may, for convenience, be arranged as they occurred during each century, and term of 500 years: thus—

1st century	6	
2nd ,,	5	
3rd ,,	8	Total during the 1st 500 years 34
4th ,,	9	
5th ,,	6	
6th ,,	7	
7th ,,	6	
8th ,,	7	,, ,, 2nd ,, 28
9th ,,	3	
10th ,,	5	
11th ,,	27	
12th ,,	28	
13th ,,	26	,, ,, 3rd ,, 97
14th ,,	12	
15th ,,	4	
16th ,,	20	
17th ,,	36	,, ,, 4th ,, 423
18th ,,	132	
19th ,, (to 1889)	235	

It may perhaps be fairly assumed from this table that no true estimate of the actual number of shocks happening within each period can be arrived at, for the chief reason that the records are entirely subject to the irregularities of the few capable observers of the early centuries. It is to be observed that 423 shocks, or nearly 75 per cent. of the total number, have occurred since 1600, which may be considered as the period from which the more trustworthy accounts commenced. There is no reason whatever for supposing that the frequency of seismic shocks has increased since that period; and the evidence indicates little more than the activity of the observers, who appear to have fallen off considerably at times, as during the fourteenth and fifteenth centuries. This point is worth remarking, on account of the misleading statement that has been more than once made,

[1] "A List of the more Remarkable Earthquakes in Great Britain and Ireland during the Christian Era." Compiled by William Roper, F.S.S., F.R.Met.Soc. (Lancaster: Thos. Bell.)

[2] "Report on the East Anglian Earthquake of April 22, 1884." By Raphael Meldola, F.R.S., &c., and William White. (Essex Field Club Special Memoirs, vol. i.) (London: Macmillan and Co., 1885.)

that the twelfth century was specially subject to earthquakes.

Since the development of telegraphy, and the consequent rapid production of daily press news, the means of recording such phenomena with prompt accuracy has of course been greatly facilitated. This is very apparent when the number of shocks which have occurred within the present century is apportioned into decades of ten years. Thus—

In 1800-10	there were	9	shocks recorded.
,, 1811-20	,,	36	,,
,, 1821-30	,,	23	,,
,, 1831-40	,,	49	,,
,, 1841-50	,,	27	,,
,, 1851-60	,,	12	,,
,, 1861-70	,,	25	,,
,, 1871-80	,,	18	,,
,, 1881-88	,,	34	,,

Making a total number, between 1800-88, of 233 shocks.

Although it appears from this artificially divided list as if a low decade was followed, as a rule, by a high decade, the number being often doubled, no safe computation whatever can be inferred; and the more one considers the facts accumulated, the more one feels that there is no real evidence upon which the various conjectures respecting earthquake periodicity have been made. About a dozen only of the numerous Comrie shocks are included in the above figures, but even this number is sufficient to materially affect any such calculation, whilst very many other well-authenticated shocks, as already mentioned, are omitted in Mr. Roper's list. With regard to Comrie, in Perthshire, it may further be remarked that, during the month of October 1839, as many as sixty-six separate shocks are reported to have taken place; and during the years 1839-42, altogether upwards of 200 vibrations were experienced in that district (*vide* NATURE, vol. xxiii. pp. 117 and 170).

With regard to the trustworthiness of the earlier records, it may be generally assumed that some earth vibration did actually take place at the time stated, notwithstanding the exaggerations and extraneous notions that were mixed up with such phenomena in superstitious times. But whether the occurrence was in every case an earthquake in the proper sense of the term is open to doubt. It is, indeed, highly probable that such occurrences as that recorded under the date of June 7, 1750, and other more recent cases, were not earthquakes at all, but the effect of bursting bolides, similar to the phenomenon which was described very fully in *Symons's Meteorological Magazine* for December 1887. Others, again, appear to have been no more than extensive landslips, or other superficial rock displacements resulting from aërial denudation; while some others were probably only connected with violent storms, or the frost-cannonadings which are commonly produced on exposed chalk cliffs during the winter season.

The absurd statements that were made respecting some of the older occurrences are evidently either intentional or unintentional falsehoods; but many of them contain so much quaint humour that a few samples are well worth quoting. In the year 132 A.D. there was a terrible earthquake in England, when "men and cattle were swallowed up"; but this fashion in recording events had been set at least twenty-nine years earlier, for in the year 103, "a city is said to have been swallowed up." In 418 there was one that was "great and general; then famine, plague, hail, snow, cold, and meteors." In 505 one lasted for three hours. At about three o'clock on August 11, 1089, there was a terrible one in England, which caused great scarcity of fruit, and a late harvest; while twelve years later there was another that "terrified all England with a horrid spectacle, for buildings were lifted up and then again settled as before." Again, in 1177, near Darlington, "the earth swelled up to a great height from nine in the morning to the setting of the sun, and then with a loud noise sank down again"; there was another that took up all the day in 1110; while on September 11, 1275, a great earthquake was felt in Newcastle, with "dreadful thunder and lightning, blazing stars, and a comet, ... with the appearance of a great dragon, which terrified the people between the first and third hour of the day." This savours somewhat of the Chinese dragon fables, while some others almost match the deluge of Noah in their vast extent. In 974, for instance, "a great one shook the whole of England"; while earlier still, in 856, one occurred "over the greatest part of the known world." In 1133, "in manie parts of England an earthquake was felt so that it was thought that the earth would have sunke under the feete of men, with such a sound as was horrible to heare." In 1290, there was one felt in England that was described as being "nearly universal (!) in Europe"; while we are assured, with circumstantial evidence, that, in the year 1426, "on the even of St. Michael the Archangel, in the morning before day, betwixt the hours of one and two of the clocke, beganne a terrible earthquake, with lightning and thunder, which continued the space of two houres, and was universal through the world. The unreasonable beasts rored and drewe to the townes with hideous noise; also the fowls of the ayre likewise cried out."

Space does not permit of other equally curious accounts, as marvellous almost as the more primitive traditions of patriarchal times regarding the vindictive forces of Nature.

Whatever may be said about the accompaniments and absurd effects which have been ascribed to earthquake action, the majority of those shocks which are recorded as having caused damage to buildings may fairly be set down as facts, and although they may have occasionally been exaggerated, some of the details are generally authentically described.

A curious problem may be raised with regard to the effect of earthquakes upon river courses. That shoals have frequently been produced along marine coasts is well known, a striking case being that which happened early in January 1885, off Malta, to the extent of dangerously affecting navigation; but there are several accounts which agree in the assertion that the beds of such navigable streams as the Trent and the Thames have been temporarily raised by local earthquakes so as to permit of people "passing over dry-shod." What became of the river course during the operation is a problem that does not appear to have required solution. Yet sufficient circumstantial evidence has been produced, in connection with the shock in 1110 at Nottingham, and in 1158 at London, to almost warrant the idea that a certain amount of credençe may be given to the stories. Whether it may be inferred from such statements that a change in the bed of the rivers in question then took place is doubtful, as history yields us no information on the point.

As a general statement we may safely infer finally that earthquakes in Great Britain, including the microseismic disturbances which are now so frequently recorded, were as common in the past as in the present period of more scientific observation; though, fortunately, such calamitous results as attended the catastrophe in Essex within recent times continue to be rare. It is still a matter for regret, however, that no steps have yet been taken to establish seismographs in different parts of this country. Until this is done, the chance records of various individuals—whose impressions, being inevitably affected more or less by the personal equation, produce only doubtful data—must continue to take the place of precise observation.

WILLIAM WHITE.

EFFECT OF OIL ON DISTURBED WATER.

GENERALLY speaking, proverbs are the resultant expression of observed facts, but the efficacy of oil upon troubled waters would appear to be a proverb which, instead of being preceded by and founded upon trial and experiment, has rather led to the scientific demonstration and establishment of the truth it asserts. From the very earliest ages the effect of oil when poured upon disturbed water appears to have been widely known. Aristotle mentions it, and accounts for the phenomenon by assuming that the thin film of oleaginous matter into which oil resolves itself when poured upon water prevents the wind from obtaining a hold upon the water, and so checks the wave formations which are the usual results of wind at sea. Pliny, too, observes that among the officers of his fleet the soothing influence of oil was matter of common knowledge, and that the Assyrian divers were in the habit of sprinkling the surface water with oil when they wished to smooth down ripples, and so obtain a better light for prosecuting their work below. Coming down to more recent times, the custom of oiling the waves with a view to facilitate navigation would appear to have fallen into desuetude. Benjamin Franklin, however, seems to have been led, from observing the effect of pouring overboard some greasy water, to test its potency in a thoroughly scientific manner, when on a voyage across the Atlantic. Having experimented with great success upon the surface of a pond near London, he tested the effects of oil upon the sea itself. A stormy day was chosen, and from a boat, some half a mile from the beach at Portsmouth, oil was poured upon the sea. The experiment met with a very small share of success, for, while a greasy patch of water was discernible right to the shore, the surf continued to break upon the beach with unabated vigour. Subsequent and recent investigation has confirmed Franklin's finding, and proved that the greatest benefit derived from the use of oil is obtainable in deep water, where wave-motion is merely undulatory. When a shore-approaching wave ceases to find enough depth to impart to its neighbour its peculiar undulatory motion, it is no longer a wave pure and simple, but becomes an actual moving body of water which moves rapidly forward, until it breaks with great violence upon the shore; upon such waves as these, oil has little or no effect.

The knowledge of the influence of oil upon a rough sea has long been known to those engaged in the whale and seal fisheries, and its application is of common occurrence. When their vessels or boats are overtaken by a storm, they usually, by means of a drogue or sea anchor, make what is nautically termed a dead drift, *i.e.* they suffer themselves to be slowly drifted before the wind. In such circumstances as these, the application of oil to the waves insures that the area into which the boat drifts is one of calm, as the oil spreads more rapidly than the boat moves, and consequently prepares a smooth patch for the vessel to drift into. If the captain, however, prefers to run his vessel before the wind, then she ranges ahead of the oiled patch, and thus the effect of oiling the waves is very materially discounted.

The native Eskimo, when engaged in transporting his family from place to place, always insures a smooth passage for the *oomiak*, or women's boat, by trailing a punctured skin filled with oil from the stern of his *kayak*, which he propels at some considerable distance ahead of the boat containing his wife and children.

Within the last twenty years many well-authenticated instances have been placed on record as to the potency of oil as a water-soother, but unfortunately the value of such reports is very much diminished by the ship-masters neglecting to explain the relative position of their vessel in regard to the wind and sea. The British warship *Swiftsure*, when on a voyage from Honolulu to Esquimault, encountered a gale accompanied by tremendous seas. A bag, punctured with the point of a knife, was filled with oil and rigged out on the weather side of the vessel. This had such a marked effect, that the vessel rode bravely through the gale, and reached her destination in perfect safety. On October 8, 1880, a Mr. Fondacaro left Monte Video for Naples in a three-ton boat. He arrived at Malaga on February 4, 1881. On his voyage across the Atlantic, he had repeatedly to lay-to during stress of weather, and reports that he considered his safe arrival entirely due to his use of oil. A gallon of olive-oil would last him, when hove-to, for twenty-four hours. He gives it as his experience that oil does not diminish the size of the waves, but renders them comparatively harmless by preventing their breaking. There is a consensus of opinion among those who have tested the use of oil, that a small quantity is quite as efficacious as a larger one, a consumption of one pint per hour being sufficient. Small as this quantity is, the extreme expansibility of oil when floating upon the water renders it quite adequate. Thus a ship running 10 knots an hour will leave behind her a wake some 10 knots by 40 feet, covered with a thin film of oil.

The Dunkirk Chamber of Commerce, fully alive to the vast importance of the use of oil as materially conducing to safe navigation, have just reported on the results of some tests made at their direction among the French fishing fleet off Iceland. One master reports that by its use he was enabled to ride out successfully a prolonged and severe spell of bad weather, which compelled his *confrères* to run to port until the weather moderated. The Chamber rewarded him with 100 francs. Other captains who have reported in detail the result of their experiments, agree with him in stating that, for small vessels experiencing stress of weather in deep water, the use of oil cannot be too highly recommended.

Nor is the utility of oil confined alone to this branch of marine navigation. Advices just received from New York furnish some interesting particulars relative to the towage of the disabled steamship *Italia* of the Hamburg American Company. The *Italia* broke her shaft whilst proceeding from Havre to New York. In this condition she was taken in tow by the *Gellert*, of the same company. The towing hawsers—6-inch steel wire—were lengthened by heavy chain cables until the distance between the two vessels was increased to 1000 feet. Unfortunately, a heavy gale from the north-west caused a dangerous sea to arise, and it was feared that the *Italia* would have to be abandoned. As a last resort, a can of oil with a small hole in the bottom was set over the stern of the *Gellert*. The effect, according to the master, Captain Kampf, was magical. The seas broke over the bows of the *Italia* with much less fury, merely surging past in a heavy swell, while the tension on the cable was immediately relieved, and the *Gellert* was enabled, in spite of continued bad weather, to reach New York in safety, having towed her charge continuously for the distance of 750 miles. Possibly many cases of abandoned towages in bad weather might be averted did the masters of tugs but try the effect of a little oil prior to casting the vessel adrift.

The true part played by this oleaginous film in diminishing the disturbance of the sea seems to be that of a lubricant. Waves are formed by the friction of wind and water. Any force, therefore, that tends to lessen the friction reduces the violence of the waves. As far as is at present known, animal or the heavier vegetable oils form the best lubricant between the two elements. Mineral or fossil oils, which possess less viscosity and are less oleaginous in their mechanical properties, exert much less influence upon the water. This anti-frictional force of oil can hardly be over-estimated. The Atlantic waves have been calculated to exert an average pressure

during the winter months of 2086 pounds per square foot. During a heavy gale this pressure is increased to 6983 pounds; yet the thin oil blanket is sufficient, when applied under certain conditions, to enable a vessel to navigate through them in perfect safety, their oiled summits raising themselves in sullen grandeur, but never breaking aboard. What the exact coefficient of friction between air in motion and water is, and the proportion of its reduction by oil or other lubricants, are questions that open up a most interesting subject of inquiry, the resolution of which will prove highly beneficial to the whole nautical and mercantile world.

Numerous experiments have been made with a view to testing the utility of oil in smoothing the approaches to exposed harbours in rough weather. The tests undertaken at Peterhead have met with unqualified success. The *modus operandi* has been to lay leaden pipes along the bottom of the harbour, taking care to keep the pipes stationary by means of concrete. The pipe is provided with numerous roses for disseminating the oil. When rough weather comes on, oil is forced along the pipes, and it escapes into the water through the apertures provided, and then, its specific gravity being less than that of water, it rises to the surface and quickly renders the sea less turbulent and the passage into the harbour quite safe. Another method employed to render safe ingress into harbours in bad weather is that of firing out to sea an oil-carrying projectile. This consists of a heavy tin tube weighted with lead at one end. The tube is filled with two or three quarts of oil, and the aperture stopped. When the projectile is fired from a gun or mortar, it reverses, and, the time-fuse exploding, the powder blows out the plug, and the liberated oil falls into the sea. A series of experiments, conducted by a Committee appointed by the United States Life-saving Service to inquire into the practical utility of oil-carrying projectiles, goes to confirm the statement made above, viz. that the power of oil to subdue the force of the waves in shoal water, or to prevent the waves breaking in surf, is very small indeed. There is one point, however, upon which all authorities who have tested the use of oil at sea are agreed. As an adjunct to the equipment of ships' boats it is simply invaluable. Many a shipwrecked crew have been enabled to keep their frail craft afloat until land was reached or a rescue effected, solely by its use. Nothing is more common among the records of shipwrecks than to read of the small boats either being swamped while at the vessel's side, or capsizing through stress of weather. In January 1884 the *Cambria* emigrant ship was run into by the *Sultan* in the North Sea, and, out of 522 on board, 416 were drowned. Of the four starboard boats, no less than three capsized, and all their occupants perished. In the collision in the Channel between the *Forest* and *Avalanche*, two out of three boats which left the *Forest* were swamped, and all on board lost their lives. These are but two instances out of many where lives *might* have been saved by the use of a little oil.

The subject of saving endangered life at sea is one that always enlists the deepest sympathies of all sorts and conditions of men. The perusal of the "Annual Wreck Chart," published by the Board of Trade, or of the lamentable records of personal sorrows and destitution consequent upon the disasters around our coasts, suggests the possibility that the loss of life might be considerably reduced by a practical knowledge of the best methods of applying oil during storms at sea. We think that much might be done by its use to facilitate the launching of boats from distressed vessels, and their safe subsequent navigation. Harbours of refuge on exposed coasts might be established at a very small cost.

In one department alone of our maritime industry, deep-sea fishing, many lives might be saved. At present, the mortality among the carriers, *i.e.* those engaged in carrying in small boats the fish from the smacks to the steam despatch-boats, is very great. Their boats might be equipped, at a very low cost, with oil-tanks or oil-bags to be used when trans-shipments are being effected in heavy weather. Already the Governments of the United States and Germany have realized the vast importance of this subject, and have instituted an exhaustive series of experiments with the view of rendering compulsory the carrying of oil for use as a life-saving equipment. When that complex and overburdened instrument of government, the Board of Trade, was asked in Parliament to cause experiments to be made relative to the use of oil at sea, the reply was, that there were no funds available for the purpose; that the Board could not spend money or become investors in such schemes. The Consultative Committee appointed under the Life-saving Appliances Act of last year have, however, suggested oil-bags, among other equipments, to be carried by boats and rafts. At the International Maritime Conference at Washington, U.S., this subject has received the attention its importance merits. Further, the National Life-boat Institution and the National Sea Fisheries Protection Association have amalgamated their forces with a view to testing the efficacy of oil, but as yet the results of their investigations have not been published. While it is very gratifying to know that the man of science and the philanthropist are ready to explore the practical utility of this question, we cannot hope for any satisfying material results until the Board of Trade sees its way to take administrative action in the matter, and to deal in a fitting manner with a question that is so indissolubly connected with the interests of all classes of this great mercantile community.

RICHARD BEYNON.

RECENT OBSERVATIONS OF JUPITER.

OBSERVATIONS of Jupiter have been conducted under great difficulties during the past opposition in consequence of the low altitude of the planet. His elevation, even at meridian passage, has only been about 16°, as observed in this country, so that the study of his surface markings has been much interrupted by the bad definition which usually affects objects not far removed from the haze and vapours on the horizon. It is, however, important that planetary features, especially those which exhibit changes of form and motion, should be watched as persistently as circumstances allow, and with this purpose in view Jupiter has been submitted to telescopic scrutiny whenever the atmosphere offered facilities for such work during the past summer and autumn. Few opportunities occurred, however, during the latter season owing to the great prevalence of clouds, and on the several nights sufficiently clear for the purpose, the atmosphere was unsteady and the definition indifferent; thus the more delicate lineaments of the planet's surface could be rarely observed with satisfactory distinctness.

The great red spot was visible on the night of May 21, 1889, and it was estimated to be on the central meridian at 12h. 31m. Further views of the same object were secured in June, July, and later months. In appearance and form it presented much the same aspect as in preceding years. Its elliptical outline is still preserved, and there seems to have occurred no perceptible change in its size. It is somewhat faint relatively to the very conspicuous belts north of it, and it is only on a good night that it can be well recognized as a complete ellipse with a dusky interior. On the evening of September 12 last, I obtained an excellent view of it with my 10-inch reflector, power 252. The spot was central at 6h. 33m., and its following end was seen to be much the darkest. This has usually been the case, and I have often noticed a very small, black spot at this extremity. Another observation was effected on the early evening of November 26, when the spot crossed the planet's centre at 3h. 54m., but the

exact time was a little uncertain, the conditions being far from favourable. Possibly the spot may have effected its passage a little before this time, as from several views of the following end of this object at about 4h. 30m., I concluded my estimate might be a trifle late, but in any case the error would be small.

Comparing the observation on November 26 with that recorded on May 21, it will be found that in the interval of 188·64 days the red spot completed 456 rotations, and that its mean period was 9h. 55m. 40·15s. This is nearly identical with the rotation period I found for the same object in 1888, when it was 9h. 55m. 40·24s. (462 rotations), and in 1887, when the figures were 9h. 55m. 40·5s. It is evident from these several determinations that during the last three oppositions the motion of the spot has been very consistent and equable. There has been a slight acceleration perhaps in velocity, inducing the rotation period to become a little shorter, but the differences are so small that they may well be covered by the observational errors which cannot be altogether eliminated from work of this character, and particularly at a time when the object observed is unfavourably placed. In any case the red spot has rotated with more celerity during the last year or two than in 1886, when its mean period was 9h. 55m. 41·1s., to which it had gradually increased from 9h. 55m. 34·2s. in 1879–80. These variations of motion may be regularly effected in a cycle, and it will be very important if future observations can determine the exact period.

The white spots near the equator of Jupiter are still occasionally visible, but it has not been feasible to secure views of them of a sufficiently exact nature to deduce their rotations. In recent years the apparent velocity of these objects has been decreasing, for while in the autumn of 1880 their period was 9h. 50m. 6s., it was found, from many observations of similar markings by Mr. A. Stanley Williams, of Brighton, in 1887, that it had increased to 9h. 50m. 22·4s.

Since 1884 a number of white spots have been also observed on the northern borders of the great northern equatorial belt. The period of these is but very slightly less than that of the red spot. On September 12, I observed one of these situated in a longitude not far preceding the west end of the red spot, and it appeared to have divided the equatorial belt with a vein of bright material. There was another object of the same kind following the red spot, but in this case the continuity of the belt was not interrupted, the bright matter appearing as a slight indentation in its northern side. These markings are shown in a drawing of Jupiter made by Mr. Keder with the great Lick refractor, power 315, on September 5 last, but they are not delineated in quite the same characters as seen here. The drawing alluded to is perhaps the best and the most replete with detail of any I have ever seen of this planet, and it furnishes clear testimony that the defining properties of the 36-inch telescope are of the highest order.

The curiously curved belt immediately north of the red spot is still one of the most prominent features on the planet's disk. It forms the southern half of the great south equatorial belt which is double. Under the ends of the red spot it suddenly dips to the north and runs into the other half of the belt. In recent years the curved belt has been very dark and pronounced in the region contiguous to the following end of the red spot, and upon its crest there have been condensations of extremely dark matter. Under the preceding end of the spot this belt is, however, more delicate in tone, and it looks like a mere pencil shading.

During the few ensuing years these interesting features may be studied to greater effect, as the planet will assume a more northerly position, and rise above the vaporous undulations which have recently much interfered with observations of his surface.

W. F. DENNING.

NOTES.

DR. ARCHIBALD GEIKIE, F.R.S., has just received a diploma of membership of the Kaiserlich Leopoldinisch-Carolinisch Deutsche Akademie der Naturforscher, the oldest scientific Society of Germany.

SIR JOHN LUBBOCK'S name appears in the list of those who have received New Year's honours and appointments. He has been made a member of the Privy Council. A baronetcy has been conferred on William Scovell Savory, F.R.S., President of the Royal College of Surgeons.

THE Paris municipality proposes to do honour to the memory of Darwin by naming a new street after him.

A COMMITTEE has been formed in Paris for the purpose of preparing the way for the erection of a statue of the late M. Boussingault. His scientific researches were of so much service to industry, especially to agriculture, that the Committee ought to have little difficulty in obtaining the necessary funds.

THE death of Sir Henry Yule, which we regret to have to record, is a great loss to geographical science. He died on Monday, in his seventieth year. His masterpiece was his splendid edition of the "Book of Ser Marco Polo"—a work to the permanent value of which he added largely by his learned and luminous notes.

WE regret to announce the death, after an illness which lasted some months, of M. Eugène Deslongchamps, of the Château Mathieu, Calvados. He was formerly Professor of Zoology and Palæontology at the Faculty of Sciences at Caen, and a member of the committee of the "Palæontologie Française." He was the son of the celebrated French palæontologist, Prof. Eudes-Deslongchamps, and published several memoirs on the palæontological fauna of Normandy, ranging from Brachiopoda to the Crocodilia. His best known memoirs are the "Prodrome des Teléosauriens du Cavaldos" and "Les Brachiopodes des Terrains Jurassiques."

GERMAN papers announce the death of Dr. Karl Edward Venus, an eminent entomologist, and founder of the Entomological Society "Iris," at Dresden. He died on December 13.

THE Congress of Russian men of science and physicians is now holding its eighth meeting. Work began on December 28, and will go on until January 7.

THE general meeting of the Association for the Improvement of Geometrical Teaching will be held in the Botanical Theatre, University College, London, on Friday, January 17. At the morning sitting (11 a.m.) the reports of the Council and the Committees will be read, the new officers will be elected, and various candidates will be proposed for election as members of the Association. After an adjournment for luncheon at 1 p.m., members will reassemble for the afternoon sitting (2 p.m.), at which papers will be read by the Rev. Dr. C. Taylor, on "A New Treatment of the Hyperbola"; by Mr. G. Heppel, on "The Teaching of Trigonometry;" by Mr. E. M. Langley, on "Some Geometrical Theorems"; and by the President (Prof. Minchin), on "Statics and Geometry."

THE Annual Conference of the Principals of the University Colleges was held on Tuesday at the Durham College of Science, Newcastle-upon-Tyne, Principal Garnett occupying the chair. The Principals were subsequently entertained at dinner by the chairman. Several questions affecting the interests of the Colleges collectively were discussed at the meeting, and it was decided on the invitation of Principal Reichel that the next gathering should be held at University College, Bangor.

THE Paris Municipal Council has lately instituted two new scientific chairs in the Hotel de Ville. One of them is devoted

to the study of the history of religions. The other is a Chair of Biology, and has been entrusted to Prof. Pouchet, of the Natural History Museum, who delivers a course of general lectures on the fundamental ideas relating to zoology, anatomy, life, &c.

AT a meeting of the Senate of the University of Sydney, on November 4, 1889, a letter from Dr. Haswell was read, intimating his acceptance of the Senate's offer of the Challis Professorship of Biology, to take effect from March 1, 1890.

AT the annual meeting of the Manx Geological Society on December 28, in the Peel Grammar School, Dr. Haviland, the retiring President, referred with pleasure to the fact that early in the summer Mr. Robert Russell had been sent to prosecute the geological survey of the Isle of Man. Dr. Haviland was also able to congratulate Peel on the prospect of a system of technical education being established in Christian's School, under the auspices of the Cloth Workers' Company and Sir Owen Roberts.

MR. A. V. GARRATT, Secretary of the American National Electric Light Association, has sent to the members a circular letter, asking them to state briefly the hardest electrical problems they meet in their investigations or in the conduct of their electrical business. He asks them also to state what feature of their business is the least economical or efficient, and why, and where the greatest economy could be effected if the difficulty could be overcome. The answers to these queries will be digested, and the results submitted to Prof. Henry A. Rowland, of Johns Hopkins University. Prof. Rowland has consented to address the next Electric Light Convention at Kansas City in February, basing his remarks upon the problems suggested by the members, and pointing out the direction in which their solution must be sought.

M. VICTOR GIRAUD, the African explorer, has just published the narrative of his explorations in the African Lake Region from 1883 to 1889. The work contains many illustrations.

THE fourth volume of M. Grandeau's "Études Agronomiques," just issued, contains a review of British and American agriculture, as represented at the Paris Exhibition.

AN historical sketch of the geographical works relating to Russia has been compiled by Baron Kaulbars under the auspices of the Imperial Geographical Society of Russia, in which the author endeavours to show the respective parts played by the army and navy, with various scientific societies, in the exploration and representation of the Empire. Beginning with the map found by Dr. Michof in St. Mark's library, Venice, only five years ago, and dating back to 1525, he traces all the labours, geographic and geodetic, referring to Russia. The astronomer Struve figures well among the latter workers in the measurements of various meridian arcs and the determination of differences of longitude, whilst few can speak with more authority than Colonel Baron Kaulbars himself on the geographical portion. Hydrographical labours began with Peter the Great, and all similar undertakings completed by the Russian navy have been brought together; the bibliographical sketch commencing with the Baltic Sea, as being the most important in the history of the navy. In the chapter chronicling the works of scientific societies, accounts are given of the many explorations into Siberia and Arctic regions. A long and complete list of all maps due to Russian topographers is also given in historical sequence, together with the various scales used.

THE Report of the Kew Committee for the year ending October 31 last contains an interesting account of the experiments carried on at the Kew Observatory; the list of instruments verified, especially clinical thermometers, Navy telescopes and sextants, and of chronometers and watches rated, is a sufficient test of the value set upon the certificates given. The death of Mr. De la Rue, the Chairman of the Committee, will be much felt, as he was one of the most munificent benefactors of the Observatory, and it was at his suggestion that the first photoheliograph was constructed and brought into use there. The complete sets of magnetic, meteorological, and electrical instruments have been kept in perfect working order, and summaries of the results for the year's working are given in the appendices to the Report. Sketches of sun-spots have been made on 173 days, and the collection of solar negatives taken between 1858 and 1872 have been handed over to the Solar Physics Committee, with a view to their utilization. A good whirling machine has been erected, for the purpose of examining the accuracy of small anemometers and of the air-meters employed in measuring air-currents in mine-shafts, &c. In accordance with a resolution of the International Meteorological Committee, a thermometer of very low range has been constructed, to be used as a standard spirit thermometer for temperatures ranging from zero to about $-70°$ C.

MESSRS. SAMPSON LOW have issued, with Mr. Stanley's permission, a shilling volume, containing "The Story of Emin's Rescue as told in Stanley's Letters." It has been edited by Mr. Keltie, who contributes an introduction bringing the narrative of the Emin Pasha Relief Expedition up to the date at which the first of Mr. Stanley's letters was received. A map, showing Mr. Stanley's routes and discoveries, is included in the volume.

AT the meeting of the Photographic Society on December 10, Mr. G. M. Whipple read an interesting and valuable paper on photography in relation to meteorology. There are now 32 observatories—8 in this country, 7 in the colonies, and 17 abroad—in which photographic apparatus is used for meteorological observations.

AT the meeting of the French Meteorological Society of December 3, 1889, M. Wada gave an account of the cyclone which ravaged the southern and eastern part of Japan on September 11 and 12 last. The centre of the storm followed a course towards N. 35° E., progressing at a rate of 30 to 43 miles an hour, the velocity of the wind reaching 65 miles an hour. The barometer fell to 28·23 inches—a reading which is only known to have occurred once before in Japan. This storm raised an enormous wave, said to have been nearly 20 feet above high-water mark, and which carried away 3000 houses. M. Ritter explained his experiments upon the artificial production of clouds in liquids and gases. With regard to the clouds in the atmosphere, the author distinguishes two principal kinds—viz. (1) the "stratus" and semi-transparent mist, and (2) the ordinary forms, such as "cumulus," &c., and he deals with them from two points of view: the diffusion of vapour according to Dalton's law, and the transference of clouds by the movement of the air. He referred to the different results produced from these conditions, with regard to suspension in the atmosphere, &c. The details of the paper will be published in the *Annuaire* of the Society.

THE *Jaarboek* of the Royal Meteorological Institute of the Netherlands for 1888 is the fortieth of the series, and contains, in addition to the daily observations and summaries at various stations a summary of phenological observations for 1879–88, and observations at Parimaribo, Jeddah, and from the Upper Congo. The preface contains an explanation of the conventional signs used in this long series, and of the curious errors which have occurred from time to time; a reference to this volume is therefore necessary to anyone who wishes to make use of the

observations of previous years, as the errors are not all typographical; for instance, the wind is given during a year and eight months in kilometres per hour instead of ¼ kilometres. But, notwithstanding certain defects and peculiarities of methods, the Institute has been consistent in keeping to one and the same plan, from a period at which the publication of systematic observations was in its infancy.

THE trustees of the Missouri Botanical Garden, in accordance with the intention of its founder, have set a good example by establishing six scholarships for garden pupils, the object being to provide theoretical and practical instruction for young men desirous of becoming gardeners. The course of instruction will extend over six years, and will include thorough training in every department of work in which practical gardeners are interested.

FROM the latest Report of the School of Mines and Industries at Bendigo, Victoria, we are glad to learn that this institution continues to make steady progress. In 1883–84 it had 324 students. The number in 1888–89 was 799. This shows, as the Council fairly claim, that the efforts of the school to supply scientific and technical education to miners, engineers, assayers, architects, pharmacists, artisans, art students, and others are thoroughly appreciated in Australia. Some of the students hail from Queensland, South Australia, and other distant parts.

THE fifth part of the second volume of the *Internationales Archiv für Ethnographie* has been issued. It maintains in all respects the high level reached by previous numbers. Among the contributions are an article in German, by F. Grabowsky, on death, burial, and the funeral festival among the Dajaks; and one in English, by Prof. H. H. Giglioli, on a singular obsidian scraper used at present by some of the Galla tribes in southern Shoa.

AT a meeting of the Philosophical Institute of Canterbury, New Zealand, on October 3, Mr. H. O. Forbes, Director of the Canterbury Museum, Christchurch, described an extinct species of swan from osteological remains which he had discovered while excavating a cave recently exposed at Sumner, on the estuary of the Heathcote and Avon Rivers, a few miles distant from Christchurch. The cave had been entirely concealed by the falling in of the basaltic rock overhanging the entrance. This great heap of *débris* had been there since the arrival of the first settlers at Canterbury, and had been quarried from for twenty-five years for the making of roads, without any trace of a cave being exposed till about the beginning of September. When the cave was first entered, there were found on the surface a few Moa bones, and various Maori implements—a well-made paddle, an ornamental baler, numerous greenstone adzes, obsidian flake scrapers, shell-openers, and ornaments carefully polished. In some of the latter, small holes for suspending them round the neck were drilled in the most beautiful manner. It is difficult to conjecture how the Maoris had accomplished this when European workers in greenstone find it a laborious process even with, and impossible without, a diamond drill. Besides these greenstone objects, there was a great quantity of fishing paraphernalia—stone suckers, fish-hooks of all sizes made out of Moa and other bones—all carefully and elaborately fashioned. Some of the larger fish-hooks were carved out of bones which must have belonged to a Dinornis of great size. On the floor of the cave was also found a well-carved representation in wood of a dog, which seems to have formed the terminating ornament of a paddle-handle—evidence that the Maoris were well acquainted with this animal. The femur of the Maori rat and a portion of the skin covered with dense reddish fur in perfect preservation were also obtained. A quantity of human hair was scattered about, both on the floor and in the kitchen midden in front of the cave. This midden was composed chiefly of marine shells of many kinds, and of the remains of fires and feasts. One large lock of long hair—evidently a woman's—was discovered in the midden tied up with great care at both ends with plaited flax, and incased in a plaited flax pocket. Some very fine bone needles also were come upon, but little thicker than steel needles, with an eye exquisitely drilled. There were, besides Moa bones, those of many other species of birds, of dogs, of fish, of seals (both fur and hair), and sea elephants—all of which had been used for food, but no human bones. Of the ornithic remains, some apparently belong to species now extinct in New Zealand, and not yet described. The bones and egg-shells of the Moa show incontestably that the Maori and it were contemporaneous. The geological evidence would seem to indicate that this cave was of considerable antiquity, and was inhabited at intervals for a long period of time. Several fire-places occur interstratified with bands of silt, as if the cave had been inhabited and then flooded many times. Definite conclusions on the geological evidence have not yet been arrived at. The swan bones discovered consist of three complete coracoids, the proximal and distal portions of the humerus sufficient to complete the whole bone. They differ very little from those of the *Chenopis atrata* of Australia, except in their greater size. The new species has been named *Chenopis sumnerensis*. It is smaller, however, than a species of swan discovered—as a complete skeleton—many years ago in Otago, some 18 feet below the surface of the ground, when the foundation for a house was being dug in Dunedin. This Sumner cave has been closed since before the introduction of the *Chenopis atrata* into New Zealand. The extension, therefore, of the Cygnidæ to New Zealand is a very interesting fact in ornithology. A similar cave, but far distant from the present one, was excavated and examined by Sir Julius von Haast (Mr. Forbes's predecessor) many years ago. Of the bones found in it, the Moa remains were fully described by their discoverer, but none belonging to the smaller birds have as yet been described. These with the osteological collections disinterred from the Glenmark and Hamilton swamps, and from the Earnscleugh Cave, will form the subject of a future paper by Mr. Forbes before the Institute.

IN a previous paper before the Philosophical Institute of Canterbury, Mr. Forbes pointed out that the bone figured by Prof. Owen on plate ciii. of his "Extinct Birds of New Zealand" as the coracoid of the *Cnemiornis*, belongs with little doubt to *Aptornis*. The coracoid of *Cnemiornis*, of which there are numerous specimens in the Christchurch and Otago Museums, is of the typical anserine form, and closely resembles that of *Cereopsis*. The coraco-clavicular angle in Aptornis approached 130°.

THE following curious instance of inheritance of an acquired mental peculiarity is given by Pastor Handtmann, of Seedorf by Lenzen on the Elbe, in the *Korrespondenzblatt* of the German Anthropological Society. When acting as substitute for a few months in 1868, in the parish of Groben, in Brandenburg, he there met a farmer named Löwendorf, who, when he signed his name officially in connection with the school, always wrote his Christian name "Austug" instead of "August." Some years later, the writer was inspecting this school, and heard a little girl read "Leneb" for "Leben," "Naled" for "Nadel," and so on. On inquiry, he found her name was Löwendorf, and she was a daughter of this farmer. The father (then dead) had in talk with his neighbours occasioned much amusement by the peculiar habit, which appeared to be the result of a fall from the upper story of a barn, some time before the birth of this girl. She wrote, as well as spoke, in the peculiar way referred to.

PROF. LEUMANN is of opinion (*Phil. Studien*) that the influence of blood circulation and breathing, on mind-life, has been too little

considered. He notices the parallelism between pulse acceleration and passion, the rush of ideas in fever, and so on. The differences of pulse and breathing in different persons are no less significant, and should be regarded in all psychometric determinations. The author noticed in boys of a Strasburg gymnasium, that in scanning verse, the number of feet spoken in a minute rose with the pulse-frequency. Even in one person, experimented on from midday till evening, the dependence of normal reading of metrical compositions on pulse-frequency was proved; the rhythmic intervals in scanning corresponded to the pulse-intervals. Leumann supposes that to be the most general and normal song-metre, whose feet correspond to the pulsations, and its lines to respiration. And, in fact, the Indo-Germanic original metre consists of four times four trochees, an arrangement agreeing with that view; from it arose the Nibelungen strophe and the hexameter.

IN the Legislative Council of India recently, Mr. R. J. Crosthwaite in introducing the amended Land Revenue (Central Provinces) Bill, said that many objections had been raised, chiefly by the Malguzars' Association of Nagpore, to the powers given by the Bill to the Chief Commissioner to make rules for the management of forests. To show that such powers were necessary, Mr. Crosthwaite instanced two cases of the wanton destruction of forests which is so common in India. In 1885 the Deputy Commissioner of Nagpore reported that the malguzar of Munsar had given a contract for the cutting and removal of the wood in the forest land of his mahal. The villagers had rights in this forest-land, and those rights were interfered with by the cutting of the wood; but, in spite of the Chief Commissioner, the malguzar continued the cutting, and the hills were completely stripped of all timber and brushwood. In another case a zemindar had sold the right to collect resin from his forest. The resin is obtained by girdling the trees, and it was found that in about four square miles of particularly fine forest every sab tree was killed outright. That is, four square miles of forest were destroyed to produce about 1200 rupees. Sir Charles Elliott, speaking on the same occasion, said that if some such provision as that now proposed had existed in the past, the forest clearances round Simla and along the southern slopes of the Himalayas abutting the Punjab plain could never have taken place.

MESSRS. DULAU AND CO. have issued a catalogue of works on chemistry and physics.

IN some copies of NATURE, last week, the following sentence appeared in the first paragraph of the Duke of Argyll's letter on "Acquired Characters and Congenital Variation": "But it implies the denial of 'congenital' causes." It ought to have been: "But it implies no denial of 'congenital' causes."

THE additions to the Zoological Society's Gardens during the past two weeks include a Malbrouck Monkey (*Cercopithecus cynosurus* ♂) from South Africa, presented by Mr. William F. Hughes; a Lesser White-nosed Monkey (*Cercopithecus petaurista*) from West Africa, presented by Mr. Lawson N. Peregrine; two Viscachas (*Lagostomus trichodactylus* ♂ ♀) from the Argentine Republic, presented by Mr. Thomas Taylor; two Crimson-winged Parrakeets (*Aprosmictus erythropterus* ♂ ♀) from Australia, presented by Mrs. G. Byng-Payne; a Bonnet Monkey (*Macacus sinicus* ♀) from India, presented by Mr. James Entwistle; a Malabar Parrakeet (*Palæornis columboides*) from Southern India, presented by Mr. J. E. Godfrey; three Common Bluebirds (*Sialia wilsoni*) from North America, presented by Commander W. M. Latham, R.N., F.Z.S.; a Black Wallaby (*Halmaturus walabatus* ♂) from New South Wales, two Black and White Geese (*Anseranas melanoleuca*) from Australia, a Ring-tailed Coati (*Nasua rufa*) from South America, deposited.

OUR ASTRONOMICAL COLUMN.

OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at Greenwich at 10 p.m., January 2 = 4h. 49m. 56s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G. C. 1157	—	—	5 27 52	+21 57
(2) 5 Orionis	5	Yellowish-red.	4 47 39	+ 2 20
(3) ι Aurigæ	3	Orange.	4 49 48	+33 0
(4) η Aurigæ	4	White.	4 58 48	+41 5
(5) 51 Schj.	6	Very red.	4 59 43	+ 1 2
(6) S Geminorum ...	Var.	Yellowish-red.	7 36 26	+23 43
(7) S Persei	Var.	Yellowish-red.	2 14 59	+58 5

Remarks.

(1) Described as "very bright, very large, very gradually brighter in the middle; barely resolvable." The spectrum was observed at Harvard College in 1869. The continuous spectrum extended from about λ 450 to 607. Two bright lines appear to have been observed, less refrangible than those of other nebulæ, but no reliable measures were made, owing to errors in the micrometer (Harvard College Observations, vol. xiii. part i. p. 64). Further observations are required, as all departures from the ordinary spectrum of bright lines are especially interesting in connection with the question of the variation of spectrum with temperature. Comparisons with the carbon flutings seen in the flame of a spirit-lamp, and the brightest flutings of manganese and lead, conveniently obtained by burning the chlorides in the flame, are suggested.

(2) In this star of Group II. the bands are very weak, only 2, 3, 7, 8 being well seen. The star falls in species 3 of the subdivision of the group, the manganese fluting (band 4) being absent because it is masked by the fluting of carbon near λ 564, and 5 and 6 being absent because the temperature is low. The carbon flutings appear to be brightest in the earlier species, and it seems probable that band 9 is also present but has been overlooked. This band is the dark space lying between the bright fluting of carbon 468–474 and the end of the continuous spectrum. Comparisons with the spectrum of the spirit-lamp flame, with special reference to the presence of the carbon fluting 468–474 are suggested. Dunér's mean value for the end of the band in other stars is λ 476.

(3) This is classed by Gothard with stars of the solar type. The usual observations are suggested.

(4) Gothard describes the spectrum of this star as Group IV., but is somewhat doubtful about it. It is probably either a late star of Group III. or Group V., as in either case the hydrogen lines would be moderately thick.

(5) This is a good example of stars of Group VI., in which Dunér records the bands 2, 3, 4, 5, 6, 9, and 10. The last three are carbon absorption flutings, and the only point to be noted in connection with these is the intensity of band 6 (near λ 564), relatively to the other bands. The first four are secondary bands, possibly produced by vapours similar to those which produce the telluric bands in the solar spectrum. Other absorptions may also be looked for.

(6) This is another variable of which no spectrum has been recorded. The range of variation is from about 8·5 at maximum to < 13 at minimum, and the period is 294 days. The maximum occurs on January 2.

(7) This is a variable star of Group II., of the same type as those in which Espin has found bright lines of hydrogen at maximum. The number and character of the bands and the presence or absence of bright lines should be noted. The intensity of the bright carbon flutings and their fading away, if any, as the maximum (January 7) is passed should also be noted. The magnitude at maximum is stated by Gore as 7·6 and that at minimum as < 9·7.

A. FOWLER.

DR. PETERS'S STAR CATALOGUE.—The case of Dr. Peters against Mr. Borst, with reference to the possession of the Clinton catalogue, containing over 30,000 stars arranged in the order of their right ascension, has been definitely settled. It will be remembered that Mr. Borst claimed the catalogue on the grounds that most of the computations had been made by him outside of his labours at the Observatory, and not under the direction of Dr. Peters, who, however, devised the work, and regarded it all

along as his own, since it included his observations extending over very many years. The court held, firstly, that the manuscript could not belong to Hamilton College, of which Dr. Peters is Professor, nor to Litchfield Observatory, of which he is Director, but to the authors and to them alone; and secondly, that the whole of the manuscript, numbering 3572 pages, held by Mr. Borst, had been wrongfully detained, and would have to be delivered to Dr. Peters, with compensation for the detention.

LONGITUDE OF MOUNT HAMILTON.—A telegraphic determination of the longitude of Mount Hamilton has been made by the United States Coast and Geodetic Survey, and the result found for the transit house meridian (Fauth transit instrument) of the Lick Observatory is—

8h. 6m. 34·807s., or 121° 38′ 42″·10 W. of Greenwich,

with an estimated probable error ± 0·1s. or 1″·5.

COMET BORELLY, *g* 1889 (DECEMBER 12).—The following elements and ephemeris have been computed for this comet by Drs. Zelbr and Froebe (*Astr. Nach.*, 2943):—

T = 1890 January 27·7438 Berlin Mean Time.

$$\left.\begin{array}{l}\pi = 211^\circ\ 4'\ 23''\\ \Omega = 16\ 59\ 17\\ \iota = 59\ 56\ 56\end{array}\right\}\text{Mean Eq. 1889·0.}$$

$$\log q = 9{\cdot}45755$$

$$\Delta\lambda \cos\beta = -\ 4''{\cdot}1$$

$$\Delta\beta = +\ 10{\cdot}7$$

Ephemeris for Berlin Midnight.

1889-90.		R.A. h. m. s.		Decl. ° ′		Brightness.
Jan. 4	...	18 31 40	...	+ 21 36·2	...	3·68
8	...	35 45	...	15 22·9	...	5·02
12	...	40 25	...	8 20·5	...	7·06
16	...	46 40	...	+ 0 19·7	...	10·22
20	...	56 31	...	− 8 42·1	...	14·80

The brightness at discovery has been taken as unity.

COMET BROOKS, *d* 1889 (JULY 6).—The following ephemeris is in continuation of that previously given (NATURE, vol. xli. p. 115):—

1890.		R.A. h. m. s.		Decl. ° ′		Brightness.
Jan. 4	...	0 45 54	...	+ 7 52·6	..	0·6
8	...	52 5	...	8 37·6	..	0·5
12	...	58 25	...	9 22·7	..	0·5
16	...	1 4 53	...	10 7·8	..	0·5
20	...	11 29	...	10 52·7	...	0·4
24	...	18 12	...	11 37·4	...	0·4
28	...	25 1	...	12 21·9	...	0·4

Brightness at discovery = 1.

THE SOLAR ECLIPSE.—Intelligence has been received by Mr. Turner, Secretary of the Eclipse Committee, from Mr. Taylor, stationed at Loanda, announcing that he has obtained no observations.

ACCUMULATIONS OF CAPITAL IN THE UNITED KINGDOM IN 1875–85.

AT a meeting of the Royal Statistical Society on December 17, Mr. Robert Giffen read a paper on accumulations of capital in the United Kingdom. He began by stating that he proposed to continue and expand the paper which he read to the Society ten years ago, on "Recent Accumulations of Capital in the United Kingdom," which dealt specially with the increase of capital between 1865 and 1875. He would now deal with the accumulations between 1875 and 1885, another ten years' period, and 1885 also being practically the present time, there being very little change in the income-tax assessments since 1885, though it appeared likely enough there would be considerable changes in a year or two. His notes had extended so much, as really to become a book, which would be published immediately by Messrs. George Bell and Sons, under the title of "The Growth of Capital," and the paper he now proposed to read consisted of extracts from that book. It must be understood that the computations were necessarily very rough and approximate only, and only designed, in the absence of better figures, to throw light on the growth of societies in wealth, and on the relations of different societies in that respect, with reference to such questions as the relative burden of taxation and national debts, the rate of saving in communities at different times, and the like. Exact figures were impossible, but approximate figures were still useful. The method he followed was to take the income-tax returns, capitalise the different descriptions of income from property there mentioned at so many years' purchase, and make an estimate for property of other kinds not coming into the income-tax returns. Formerly, in comparing 1865 and 1875, he had capitalised at the same number of years' purchase in each year, but between 1875 and 1885 there were changes in capital value irrespective of changes in income which it was important to take notice of, at least as between different descriptions of property, though the results in the aggregate would not be much different from what they are if no change in the number of years' purchase were made. In 1885, then, the total valuation of the property of the United Kingdom, according to the method followed in the paper, came to 10,000 millions sterling in round figures, equal to about £270 per head. The principal items were: Lands, 1691 millions; houses, £1,927,000; railways in United Kingdom, 932 millions; miscellaneous public companies in Schedule D, 696 millions; trades and professions in Schedule D, 542 millions: farmers' profits, &c., in Schedule B, 522 millions; public funds (excluding home funds), 528 millions; gasworks, 126 millions; waterworks, 65 millions; canals, docks, &c., 71 millions; mines and ironworks, 39 millions. These were all based on the method of capitalising income in the income-tax returns, and the principal item of other property, for which an estimate was made in a different way, was that of movable property not yielding income, *e.g.* furniture of houses, works of art, &c., which was taken at about half the value of houses, or 960 millions. Comparing these figures with those of 1875, when the valuation was 8500 millions, the apparent increase was 1500 millions, or about 17½ per cent.; but there were important changes in detail, lands having declined considerably, mines and ironworks having also declined, and there being a great increase in houses and some other items. It appeared also that the increase in the decade 1875–85 was considerably less than in the previous decade dealt with in the former paper. In 1865–75, in fact, the increase was from about 6100 millions to 8500 millions, or no less than 2400 millions, and 40 per cent. in ten years, and 240 millions per annum; whereas in 1875–85 the increase was only 1500 millions, or 17½ per cent. in ten years, and only 150 millions per annum. The difference in the rate of growth was ascribed very largely to a difference in the rate of growth of money values only, reasons being given for the belief that in real prosperity, in the multiplication of useful things, and not merely money values, the improvement in the later period was not less than in the first. The distribution of this great property between England, Scotland, and Ireland, could not be exactly shown, part of the income belonging to the community of the United Kingdom in a way which did not permit of a distinction being made; but upon a rough estimate it appeared that England was considered to have 8617 millions, or 86 per cent. of the total; Scotland, 973 millions, or 9·7 per cent.; and Ireland, 447 millions, or 4·3 per cent. These figures worked out about £308, £243, and £93 per head respectively, as compared with the average of £270 for the United Kingdom. The small relative amount of property in Ireland was commented upon, and the difference between it and Great Britain was ascribed very largely to the political agitation in Ireland, which depreciated property, and the excess of population on the land, which had the same effect; these two causes together making a difference of 200 millions in the apparent capital of Ireland. Measured by property, Ireland was enormously over-represented in the Imperial Parliament. Looking at the subject historically, they found that there had been an enormous and continuous advance in the course of the past three centuries, during which at different times there had been contemporary estimates on the subject. In 1600 the property estimate was for England only 100 millions, or £22 per head; 1680, 250 millions, or £46 per head; 1690, 320 millions, or £58 per head; 1720, 370 millions, or £57 per head; 1750, 500 millions, or £71 per head; and in 1800, 1500 millions, or £167 per head. The estimate for Great Britain in the latter year being about one-eighth more in the aggregate than for England only, and £160 per head. Since 1800 there are figures for the United Kingdom, and these show: 1812, 2700 millions, or £160 per head; 1822, 2500 millions, or £120 per head (a reduction largely due to fall of prices); 1833, 3600

millions, or £144 per head; 1845, 4000 millions, or £143 per head; 1865, 6000 millions, or £200 per head; 1875, 8500 millions, or £260 per head; and finally, the present figures of 10,000 millions, or £270 per head. There was in fact a steady increase, with the exception of the interval between 1812 and 1822, when there was a heavy fall of prices, and this increase, it was believed, represented almost all through a real increase in things, money prices at any rate being at a lower rate now than at the beginning of the century. There had also been a remarkable change all through in the proportions of different descriptions of property. Lands, at the commencement constitute about 60 per cent. of the total; at the beginning of the century they are still about 40 per cent.; at the present time they are 17 per cent. only. Houses, on the other hand, are about 15 per cent. of the total at the beginning, and 19 per cent. at the present time, an increasing percentage of an ever-increasing total; but the main increase after all is in descriptions of property which are neither lands nor houses. After referring to the accumulations of capital in foreign countries, Mr. Giffen concluded by giving illustrations of the mode of using such figures, showing the difference of the burden of taxation and national debts in England, France, and the United States; the preponderance of England in the United Kingdom as compared with England, Scotland, and Ireland; the rapid growth of the United States in recent years as compared with the United Kingdom, and especially as compared with France (the national debt in the United States, from amounting twenty years ago to a sum equal to a fifth of the total property, having come to be only equal to a thirtieth of the property); and the small proportion of the annual savings of the country which comes into the public market for investment, as compared with the savings invested privately as they are made. In passing, a reference was made to the talk of the vast expenditure on military armaments, and the burden they impose on certain communities; and it was suggested that, heavy as the burdens are, yet the vast amount of property relatively indicated that the point of exhaustion was more remote than was commonly supposed. In conclusion, the hope was expressed that the discussion of recent years would lead in time to the production of better figures, especially with regard to the growth of different descriptions of property. Were trouble taken, results might be arrived at which would be of value to the Government practically, as well as to economists in their discussions. The progress of revenue was intimately connected with the progress of national resources, and the progress of money revenue with the progress of the money expression of those resources. The resources themselves, and the money values, must be studied by Chancellors of the Exchequer with almost equal anxiety, and they should both, at any rate, be studied together. Periodical complete valuations of property were in this view as indispensable as the census of population itself.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

University College, Liverpool.—The Sheridan Muspratt Chemical Scholarship, of the value of £50 per annum for two years, has been awarded to Mr. J. T. Conroy, who has been a student in the chemical laboratories during the past two years. Mr. Conroy has recently taken the degree of B.Sc., with honours in chemistry, at the University of London. The Scholarship, which is the gift of Mrs. Sheridan Muspratt, is intended to enable the holder to continue work in the higher branches of chemistry. The Sheridan Muspratt Exhibition of £25 has been awarded to Mr. A. Carey, of Widnes, who has been a student of the College during the last two and a half years, and is now in the final stage of preparation in the honours school of chemistry of Victoria University.

SCIENTIFIC SERIALS.

Rendiconti del Reale Istituto Lombardo, November.—On the antidotes of the virus of tetanus, and on its prophylactic surgical treatment, by Prof. G. Sormani. In continuation of his previous paper on this subject, the author here describes some further experiments with alcohol, chloroform, and various preparations of camphor, chloral, and iodine. He finds that camphor and camphorated alcohol produce no effect on the virus, and that chloroform and hydrated chloral have a more or less attenuating action, checking the development of the artificially cultivated microbe, or even in some cases rendering it absolutely sterile, while camphorated chloral has a decidedly neutralizing effect on the virus. Other experiments show that when tetanus is once developed in the system iodoform is powerless to arrest its progress, but is most efficacious in neutralizing the virus of the injured part. The whole series of experiments fully confirms the author's previous conclusion that iodoform is the specific *disinfectant* of the microbe of tetanus.

Bulletin de l'Académie Royale de Belgique, October 12.—Jupiter's north equatorial band, by M. F. Terby. The author describes in detail the structure of this remarkable phenomenon which he has been carefully studying for the last three years with a Grubb 8-inch telescope.—Determination of the invariant functions or forms comprising several series of variants, by M. Jacques Deruyts. In continuation of his previous communications, the author here extends to forms with several series of variants the results already made known for forms with a series of *n* variables.—M. C. Vanlair describes the symptoms and treatment of a new case of bothriocephaly in Belgium, due to the presence of *Bothriocephalus latus* in the patient.

SOCIETIES AND ACADEMIES.

London.

Royal Society, December 5, 1889.—"Researches on the Chemistry of the Camphoric Acids." By J. E. Marsh.

An account is given of some experiments leading to the production, in any desired quantity, of a new camphoric acid, and to the mutual conversion of one acid into the other; as well as to a method of quantitatively separating the two acids when mixed. The space at our disposal does not permit us to enter into any details of the experiments, nor into the theoretical considerations involved. For this, reference must be made to the original paper.

December 19, 1889.—"On the Steam Calorimeter." By J. Joly, M.A. Communicated by G. F. Fitzgerald, F.R.S., F.T.C.D.

The theory of the method of condensation has been previously given by the author in the Proceedings of the Royal Society, vol. 41, p. 352.

Since the publication of that paper a much more extended knowledge of the capabilities of the method has been acquired, which has led to the construction of new forms of the apparatus, simple in construction and easily applied. Two of these are described and illustrated, one of which is new in principle, being a differential form of the calorimeter. The accuracy of observation attained by this latter form is so considerable that it has been found possible to estimate directly the specific heats of the gases at constant volume to a close degree of accuracy.

An error incidental to the use of the method arising from the radiation of the substance, when surrounded by steam, to the walls of the calorimeter, is inquired into. It is shown that this affects the accuracy of the result to a very small degree, and is capable of easy estimation and elimination.

Further confirmation of the accuracy of the method is afforded in a comparison of experiments made in different forms of the steam calorimeter.

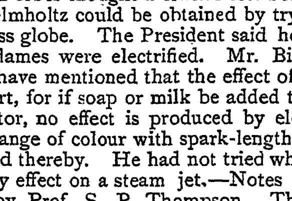
Various tables of constants are given to facilitate the use of the method, and the results of experiments on the density of saturated steam at atmospheric pressures, made directly in the calorimeter, are included. These are concordant with the deductions of Zeuner, based on Regnault's observations on the properties of steam, and were undertaken in the hope of affording reliable data on which to calculate the displacement effect on the apparent weight of the substance transferred from air to steam.

The communication is intended to provide a full account of the mode of application of the steam calorimeter.

Royal Meteorological Society, December 18, 1889.—Dr. W. Marcet, F.R.S., President, in the chair.—The following papers were read:—Report of the Wind Force Committee on the factor of the Kew pattern Robinson anemometer. This has been drawn up by Mr. W. H. Dines, who has made

large number of experiments with various anemometers on the whirling machine at Hersham. Twelve of these were made with the friction of the Kew anemometer artificially increased, seven with a variable velocity, and fourteen with the plane of the cups inclined at an angle to the direction of motion. In discussing the results the following points are taken into consideration, viz. the possibility of the existence of induced eddies, the effect of the increased friction due to the centrifugal force and gyroscopic action, and the action of the natural wind. The conclusion that the instrument is greatly affected by the variability of the wind to which it is exposed seems to be irresistible, and if so, the exact value of the factor must depend upon the nature of the wind as well as upon the mean velocity. There is evidence to show that during a gale the variations of velocity are sometimes of great extent and frequency, and there can be but little doubt that in such a case the factor is less than 2·15. The one point which does seem clear is, that for anemometers of the Kew pattern the value 3 is far too high, and consequently that the registered wind velocities are considerably in excess of the true amount.—On testing anemometers, by Mr. W. H. Dines. The author describes the various methods employed in the testing of anemometers, points out the difficulties that have to be encountered, and explains how they can be overcome.—On the rainfall of the Riviera, by Mr. G. J. Symons, F.R.S. The author has collected all the available information respecting rainfall in this district, which is very scanty. He believes that the total annual fall along the Riviera from Cannes to San Remo is about 31 inches, and that any difference between the several towns has yet to be proved.—Report on the phenological observations for 1889, by Mr. E. Mawley. This is a discussion of observations on the flowering of plants, the appearance of insects, the song and nesting of birds, &c. Taken as a whole, 1889 was an unusually gay and bountiful year.

Physical Society, Dec. 6, 1889.—Prof. Reinold, President, in the chair.—The following communications were read:—On the electrification of a steam jet, by Shelford Bidwell, F.R.S. The author showed that the opacity of steam issuing from a nozzle is greatly increased by bringing electrified points near it, and that its colour is changed to orange-brown. Electrified balls and disks when placed in the steam produce similar effects, and when these are connected with an influence machine at work, the decoloration of the jet rapidly responds to each spark. On examining the absorption spectrum of the unelectrified jet, little or no selective absorption was detected, but on electrification, the violet disappeared, the blue and green were diminished, and the orange and red remained unchanged. From these results the author concludes that electrification causes an increase in the size of the water particles in the steam, from something small compared with the wave-length of light, to about 1/50000 of an inch in diameter. Allied phenomena with water jets have been observed by Lord Rayleigh, who found that a straggling water jet is rendered much more coherent by bringing a rubbed stick of sealing wax near it. These observations are of considerable meteorological interest, for the steam jet phenomena go far towards explaining the cause of the intense darkness of thunderclouds, and of the lurid yellow light with which that darkness is frequently tempered. After making his experiments the author learnt that similar observations had recently been made by the late Robert Helmholtz, who viewed the steam jets by reflected light against a dark background. On electrification the jets became much better defined, and presented diffraction colours. Luminous flames also produced similar effects, and Mr. Bidwell has found that glowing touch paper is equally efficient. Helmholtz conjectures that the sudden condensation may be due to molecular tremors or shock imparted by the electrification upsetting the unstable equilibrium of the supersaturated vapour, just as a supersaturated saline solution is suddenly crystallized when disturbed. Another hypothesis suggests that condensation is caused by the introduction of solid matter into the jet by the exciting cause, thus producing nuclei upon which the vapour may condense. On reading Helmholtz's paper, the author tried the effect of gas-flames on water jets, and found that when luminous they influenced the jet considerably, whereas non-luminous flames had no appreciable effect. He also found that luminous flames are positively electrified, and demonstrated this before the meeting. Prof. Rücker thought the surface tension of the films surrounding the water jets might be lowered by the presence of a burning substance, and that the smoke from the touch paper used in some of the experiments on steam jets would introduce solid particles and facilitate condensation. Mr. Richardson inquired whether a red-hot iron had any effect. Dr. Fison said he had made experiments on the electrification of flame, and found that potentials varying from + 2 volts to − 1½ volts could be obtained in the region within and surrounding a Bunsen flame. Prof. S. P. Thompson commented on the contrast between Mr. Bidwell's experiments and those of Dr. Lodge on the dissipation of fogs by electricity, and also asked whether the colour of the jet depended on the length of the spark produced by the machine. Prof. Forbes thought a crucial test between the two hypotheses of Helmholtz could be obtained by trying the experiment in a germless globe. The President said he had recently noticed that gas flames were electrified. Mr. Bidwell in reply said he ought to have mentioned that the effect of flames on jets may be due to dirt, for if soap or milk be added to the water in the steam generator, no effect is produced by electrification or flame. As to change of colour with spark-length, little (if any) variation is caused thereby. He had not tried whether a red-hot iron produced any effect on a steam jet.—Notes on geometrical optics, Part 2, by Prof. S. P. Thompson. Three notes were presented, the first of which dealt with the geometrical use of "focal circles" in problems relating to lenses and mirrors, and to single refracting surfaces. By "focal circles" the author means the circles having the principal foci as centres, and whose radii are equal to the focal lengths. By their use the point conjugate to any point on the principal axis is readily determined. One construction for a mirror is to draw a tangent to the focal circle from a point P on the axis; the foot of the perpendicular to the axis drawn through the point of contact gives the point conjugate to P. When applied to a thin lens, a tangent is drawn as above to one focal circle, and the line joining the point of contact with the centre of the lens is produced to meet the other focal circle; a perpendicular to the axis from the remote point of intersection gives the conjugate point. Modifications applicable to thick lenses and single refracting surfaces were also given. In his second note the author treated similar problems by the aid of squares drawn on the principal focal distances, the constructions being remarkably simple, as will be seen from the figure, in



which M_1 M_2 represent the principal planes of a thick lens, F_1, F_2, its principal foci, and P and Q are conjugate points. The line B C is drawn parallel to P A. In the third note, the paths of rays through prisms are determined by the aid of imaginary planes representing the apparent position of the plane bisecting the dihedral angle of the prism when viewed through its two faces. Just as in problems on thick lenses in which the part between the principal planes may be supposed removed, so when dealing with prisms, the part between the imaginary planes above referred to may be supposed non-existent. In another method of treatment, the apparent positions of points outside the prism when viewed from inside the prism are made use of, and their application to illustrate dispersion was pointed out. Mr. C. V. Boys asked whether the latter construction could be used to show why the slit of a spectroscope appears curved.—On the behaviour of steel under mechanical stress, by Mr. C. H. Carus-Wilson. This is an inquiry into the properties of steel as illustrated by the *stress-strain* curves given in automatic diagrams from testing machines, and by magnetic changes which take place during testing. After pointing out that the permanent elongation of a bar under longitudinal stress consists of a sliding combined with an increase of volume, the author showed that the "yield" is caused by the limit of elastic resistance (ρ) parallel to one particular direction in the bar (generally at 45° to the axis) being less than along any other direction. When this lower limit is reached, sliding takes place in this direction until the hardening of the bar caused thereby raises the limit of elastic resistance (in the direction referred to) to that of the rest of the bar, after which the stress must be increased to produce further permanent set. From considerations based on the stress-

strain curves of the same material when hardened to different degrees by heating and immersion, &c., it was concluded that the increase of (ρ) during "yield" is the same for all the specimens, and that the "yield" is a measure of the "hardness." The question of discontinuity of the curves about the "yield point" was next discussed, and evidence to the contrary given by specimens which show conclusively that the yield does not take place simultaneously at all parts of the bar, but travels along the bar as a strain wave. In these specimens the load had been removed before the wave had traversed the whole length; and the line between the strained and unstrained portions could be easily recognized. As additional evidence of continuity, the close analogy between the *stress-strain* curves of steel of various degrees of hardness, and the isothermals of condensible gases at different temperatures when near their point of liquefaction, was pointed out; the apparent discontinuity in the latter probably being due to the change from gas to liquid taking place piecemeal throughout the substance (see Prof. J. Thomson, Proc. Roy. Soc., 71, No. 130). In seeking for an explanation of the hardening of steel by permanent strain, the author was led to believe this due to the displacement of the atoms within the molecules of the substance. To test this hypothesis, experiments on magnetization by stretching a bar in a magnetic field were made; these show that the magnetization increases with the stress up to the "yield point," and is wholly permanent when approaching that point. On comparing his results with Joule's experiments on the elongation of loaded wires produced by magnetization, the author infers that there are two kinds of elongation—firstly, that produced by relative motion of the molecules, and secondly, an elongation resulting from a straining of the molecules themselves. To this latter straining the hardening by permanent strain is attributed, and this view seems compatible with the results of Osmond's researches on the hardening of steel.—Mr. F. C. Hawe's paper was postponed.

Mathematical Society, Dec. 12, 1889.—Mr. J. J. Walker, F.R.S., President, in the chair.—The following papers were read:—On the radial vibrations of a cylindrical elastic shell, by A. B. Basset, F.R.S.—Note on the 51840 group, Dr. G. G. Morrice.—The President then vacated the chair, which was taken by Mr. E. B. Elliott, Vice-President.—Complex multiplication moduli of elliptic functions for the determinants -53 and -61, by Prof. G. B. Mathews (communicated by Prof. Greenhill, F.R.S.).—On the flexure of an elastic plate, by Prof. H. Lamb, F.R.S.—Notes on a plane cubic and a conic, by R. A. Roberts (communicated by the Secretary).—Dr. Larmor and Mr. Curran Sharp made brief communications.

EDINBURGH.

Royal Society, December 16, 1889.—Sir Arthur Mitchell, Vice-President, in the chair.—Dr. Thomas Muir read a note on Cayley's demonstration of Pascal's theorem. He has succeeded in simplifying the proof.—Dr. Muir also read a paper on self-conjugate permutations, and one on a rapidly converging series for the extraction of the square root.—Prof. Tait read a note on some quaternion integrals, and also a note on the glissette of a hyperbola. When a given ellipse slides on rectangular axes, any point in its plane traces out a definite curve, and the same curve can be similarly obtained as the trace of a definite point in the plane of a certain hyperbola sliding between axes in general inclined to the former.—Dr. Woodhead communicated a paper, written by Dr. Herbert Ashdown, on certain substances, formed in the urine, which reduce the oxide of copper upon boiling in the presence of an alkali. Dr. Ashdown was led to search for these substances in the human subject as the result of observations made upon lower animals.—Dr. G. E. Cartwright Wood discussed enzyme action in the lower organisms.—Dr. Woodhead communicated a paper, by Mr. Frank E. Beddard, on the structure of a genus of Oligochætæ belonging to the Limnicoline section.

PARIS.

Academy of Sciences, December 16, 1889.—M. Hermite in the chair.—Note on the eclipse of December 22, by M. J. Janssen. The arrangements are described which were made at the Observatory of Meudon for observing this event.—On the effects of a new hydraulic engine used for irrigation purposes, by M. Anatole de Caligny. The general disposition of this apparatus was fully described in the *Comptes rendus*, November 19, 1887. The present note has reference to an improvement introduced for the purpose of remedying a serious defect in the original design. It has now the advantage of giving as good results as any of the systems in general use, while superior to them in simplicity and economy.—On the production of films of ice on the surface of the alburnum of certain species of plants, by M. D. Clos. Early in December, after a hard frost, when the glass fell to $-6°$ C. at night, *Verbesina virginica*, *Helianthus orgyalis*, and several other plants exhibited the same phenomenon of glaciation at the Toulouse Botanical Garden as was observed and described by Dunal at Montpellier in 1848. An explanation is here given of the phenomenon, which occurred on a much larger scale on the present than on the previous occasion.—Observations of Borrelly's new comet (*g* 1889), made at the Paris Observatory with the equatorial of the west tower, by M. G. Bigourdan. The observations were taken on December 15, when the comet presented the appearance of a nebulosity indistinctly round, of 2′ diameter, slightly more brilliant in the central region, but without notable condensation. In its expanse were clearly visible two stellar points, and the presence of several others suspected.—On the series $\sum\frac{1}{k^2}$, $\sum\frac{1}{k^3}$, by M. André Markoff. From the nature of these series the author establishes a formula which yields the equation—

$$1 + \frac{1}{2^3} + \frac{1}{3^3} + \frac{1}{4^3} + \ldots = 1{\cdot}202\,056\,903\,159\,594\,285\,40,$$

correct to 20 decimals. M. Markoff's paper forms a sequel to Stirling's memoir "De Summatione et Interpolatione Serierum Infinitarum."—On magnetic potential energy and the measurement of the coefficients of magnetization, by M. Gouy. The mechanical action of magnets on isotropous substances, diamagnetic or feebly magnetic isotropous bodies, has often been utilized for measuring or comparing the coefficients of magnetization assumed to be constants. On this hypothesis has been established the expression of the potential energy which serves to calculate the mechanical action in question. Here M. Gouy proposes to supply a somewhat more complete theory by regarding these coefficients, not as constants, but as variable with the magnetizing force, and utilizing the experimental data for measuring the variations.—On the colour and spectrum of fluorine, by M. Henri Moissan. The colour of fluorine as here determined is a greenish-yellow, much fainter than that of chlorine under like conditions, and inclining more to the yellow tint. Thirteen rays have been determined in the red region of the spectrum. With hydrofluoric acid several bands have been obtained in the yellow and violet, but very wide and not sufficiently distinct to fix their position with accuracy.—Action of ammonia on the combinations of the cyanide with the chlorides of mercury, by M. Raoul Varet. The paper deals severally with the action of ammonia on the cyanochloride of mercury; the action of absolute ammoniacal alcohol; the action of ammoniac gas; the cyanochloride of mercury and zinc; and the cyanochloride of mercury and copper.—On an adulteration of the essence of French turpentine, by M. A. Aignan. This fraud, which consists in the addition of a small quantity of the oil of resin, is not easily detected, but may be discovered by studying the rotatory power of the liquid, as is here shown.—Papers were submitted by M. Besson, on the temperature of solidification of the chlorides of tin and arsenic, and on their faculty of absorbing chlorine at a low temperature; by M. Seyewitz, on the synthesis of dioxidiphenylamine and of a red-brown colouring substance; by M. Pierre Mercier, on a general method of colouring photographic proofs with the salts of silver, platinum, and the metals of the platinum group; and by MM. G. Pouchet and Biétrix, on the egg and first development of the alose, a fish allied to the sardine.

December 23.—M. Hermite in the chair.—On the discovery of a fossil ape, by M. Albert Gaudry. On presenting to the Academy the skull of an ape recently discovered by Dr. Donnezan at Serrat d'en Vaquer, M. Gaudry remarked that, except those from Pikermi in Greece, these are the only cranial remains of a fossil Simian hitherto brought to light. Many other fossils have been found in the same place, which evidently contains large accumulations, especially of extinct vertebrate animals.—Observations of the comet discovered by M. Borrelly at the Observatory of Marseilles, on December 12, by M. Stephan. The observations are for December 12, 13, and 14, during which period the comet steadily increased in brightness, and assumed more distinct outlines. On the 12th it was

obscured for a few minutes by a star of the tenth or eleventh magnitude.—Determination of the difference of longitude between Paris and Leyden, by M. Bassot. This international operation, executed by MM. Van de Sande Bakhuyzen and Bassot, presents a special geodetic interest, Leyden being the northernmost station of the meridian of Sedan which now passes through Belgium far into the Netherlands. From the observations the difference of longitude between Paris and Leyden appears to be 8m. 35·602s., with probable error ± 0·011s., which, reduced to the official meridians, gives 8m. 35·213s.—On the degree of accuracy attained by thermometers in the measurement of temperatures, by M. Ch. Ed. Guillaume. On presenting to the Academy his "Traité pratique de la Thermométrie de précision," the author took occasion to reply to M. Renou's recent remarks on the accuracy of the mercury thermometer. Reviewing the whole question, and comparing the opinions and experiences of the most distinguished physicists during late years, M. Guillaume considers it placed beyond doubt that mercury thermometers with glass of varying qualities yield varying results. But these differences, formerly supposed to be fortuitous, are now known to be systematic, so that any number of instruments giving identical results may be constructed by a judicious selection of glass and careful manipulation.—On β-inosite, by M. Maquenne. In a previous note (*Comptes rendus*, vol. cix. p. 812) the author showed that pinite may be decomposed into a molecule of methyl iodide and a molecule of a new sugar called by him β-inosite. The analysis of these two bodies leading to identical results, he inferred that they were isomerous, presenting relations of the same order as those existing between the two known hexachlorides of benzine. This hypothesis has been fully confirmed by his further study of β-inosite, communicated in the present memoir.—On a new class of diacetones, by MM. A. Béhal and V. Auger. The authors have already shown that the chlorides of malonyl, methylmalonyl, and ethylmalonyl react on the aromatic carburets, yielding diacetones, β,R—CO—CHX—CO—R. They have also determined the formation of compounds having the characteristic property of yielding with the alkalies and alkaline carbonates blood-red solutions. A further series of researches has now enabled them to prepare several of these compounds in large quantities, and thus study their constitution as here described. The best results were yielded by metaxylene and the chloride of ethylmalonyl.—Optical properties of the polychroic aureolas present in certain minerals, by M. A. Michel Lévy. This curious phenomenon is traced mainly to the presence of small crystals of zircon widely disseminated throughout granitic and other rocks. In some cases it may also be due to the presence of dumortierite and allanite. These aureolas offer an interesting example of a simultaneous modification of birefraction and polychroism, a modification, however, which is not permanent, or at least which may disappear, without involving any change in the properties of the mineral itself.—Analysis of the Migheï meteorite, by M. Stanislas Meunier. This meteorite, which fell on June 9, 1889, at Migheï, in Russia, yielded besides the usual constituents, a new element, which M. Meunier has not yet succeeded in identifying.—Papers were contributed by M. Y. Wada, on the earthquake of July 28 at Kiushu Island, Japan; by M. Ch. Contejean, on the circulation of the blood in mammals at the moment of birth; by M. Ferré, on the semeiologic and pathologic study of rabies; and by Messrs. Woodhead and Cartwright Wood, on the antidotic action exercised by the pyocyanic liquids on the development of the anthracite disease.

BERLIN.

Meteorological Society, Dec. 3, 1889.—Dr. Vettin, President, in the chair.—Dr. Kremser spoke on the frequency of occurrence of mist, a subject whose investigation he had recently undertaken. Up to the present time the material derived from observation is extremely scanty, as shown by the extremely divergent mean values obtained for different places in close proximity to each other, as, for instance, Hamburg and Altona, or even different parts of the one city, Berlin. It seems scarcely possible to attribute the differences to local conditions in all cases, for the mean annual values resulting from the observations of different observers in one and the same place show an equally striking discordancy. This is undoubtedly due to the want of suitable units for estimating and measuring mists. From the above it follows that it is impossible to determine any secular changes on the basis of existing observations, although the yearly variations may be. By comparisons based on a long series of observations, it appeared that a series extending over ten years suffices to give a reliable monthly mean. From this it appears that at most stations the maximal amount of mist occurs in the months of November and December, the maximum occurring in November in the eastern provinces of Prussia, and falling progressively later the further the stations lie towards the west. On the coasts of the North Sea and on the adjacent islands the maximum is observed in January, while it occurs on mountains as early as September and October. At the latter stations the minimum is met with as early as May, and is progressively later (June and July) at the other stations according to the lateness of the maximum. On the islands, as, for instance, Heligoland, the minimum does not occur before September or October. As a general rule, 70 per cent. falls in autumn and winter, 20 per cent. in spring, and 10 per cent. in summer. The amplitude of the yearly differences is greatest on the plains and least on mountains. The number of days on which mist occurs is greatest at mountain stations, amounting on the average to 200 per annum, falling in the low lands to as few as 40 or less. The material at hand for determining the variations in the amount of mist per diem was extremely scanty; still it was possible to make out that, in winter, mist is most frequent in the morning, diminishing considerably towards midday, and being in the evening at times as frequent as at midday, at times somewhat more frequent. In summer, mist is observed only in the morning, and then disappears completely. In the discussion which followed the above communication it was pointed out how essential it is to distinguish between clouds and mist, as also many other factors, such as the frequency of purely local mists, the absence of wind, the difficulty of determining the density of mists, the differences of altitude, &c.—Dr. Sprung spoke on some new self-recording apparatus of various kinds made by Richard of Paris, and described fully his actinometer and anemocinometer.

Physical Society, Dec. 6, 1889.—Prof. Kundt, President, in the chair.—Prof. Planck spoke on the development of electricity and heat in dilute electrolytic solutions. From the experiments of Kohlrausch and Hittorf, and the theoretical considerations of Van t' Hoff, Arrhenius, and Nernst, all that takes place in dilute electrolytic solutions during the passage of a current is very accurately known, especially in the cases where the solution is very dilute and the electrolyte is very uniformly distributed in it. It has become possible to subject the occurrences in electrolytic solutions to mathematical investigation, owing to the existing conceptions of the osmotic pressure in such solutions, of the more or less complete dissociation of the electrolyte when in dilute solution, of the applicability of the gaseous laws to such solutions, and owing to the experimental determination of the rate at which the *ions* travel. The speaker had submitted the general case, in which the solution is not quite uniform, to a mathematical analysis, and deduced the formulæ which represent that which is taking place in each unit of volume of the highly diluted solutions in which dissociation is complete. These formulæ correspond exactly to those arrived at by Nernst for the development of electricity. Up to the present time the thermal phenomena in dilute electrolytic solutions have not been fully dealt with. The speaker showed that heat is the most important form of energy existing in the solution. It is only possible to arrive at a complete understanding of the heat production if, when drawing parallels between dilute solutions and gases, a further step is taken, and it is assumed that just as gases become warmer by compression and colder by a fall of pressure, so also heat is developed in electrolytic solutions when the *ions* are increased in number, and disappears when they are diminished per unit of volume. Hence the mere diffusive processes in an electrolytic solution whose composition is not uniform must develop an osmotic heat, which makes its appearance, and can be calculated in the absence of any electrical current. This osmotic heat must be taken into account, along with the two already known sources of heat production, during the passage of an electric current through a solution, before it is possible to calculate all the relationships of energy in a dilute, non-uniform, electrolytic solution during the passage of a current through it.—The President exhibited the air-pump constructed by Otto von Guericke in 1675, which had recently been acquired by the Physical Society. This pump is still in a thoroughly workable condition, with the exception of the glass vessel, which has been renewed. The pressure in this receiver could be reduced to 20 mm. of mercury, by means of the pump. The

celebrated Magdeburg hemispheres have also come into the possession of the Society, and were exhibited at the same time; they are perfect except in the want of the leather packing.

AMSTERDAM.

Royal Academy of Sciences, November 30, 1889.—Dr. Hoek read a paper on the Zuyder Zee herring, showing that it belongs to a race of spring herrings (herrings spawning in spring) closely related to the spring herrings of the Baltic, as described by Heincke. But whereas, in the Baltic, two races of herrings—an autumn or winter herring, and a spring herring—can be distinguished, all the herrings which enter the Zuyder Zee—both those which enter it in autumn and those which are caught in spring—belong to one variety: they all spawn in the spring months only; they are reproduced only in water that is rather brackish (nearly fresh); and their fry is very small in comparison with that of open-sea herrings. Considering that the Zuyder Zee herring is a variety which has sprung from the open North Sea herring, it furnishes a striking instance of the formation of a variety under changed conditions in the course of a few centuries.—Prof. van de Sanden Bakhuyzen gave an account of the meeting of the Committee for the Construction of the Photographic Map of the Heavens, held at Paris in September last, and spoke about the share of the Dutch astronomers in that undertaking.

DIARY OF SOCIETIES.

LONDON.

THURSDAY, JANUARY 2.

ROYAL INSTITUTION, at 3.—Electricity (adapted to a Juvenile Auditory): Prof. A. W. Rücker, F.R.S.

FRIDAY, JANUARY 3.

GEOLOGISTS' ASSOCIATION, at 8.—On the Fossil Fishes of the English Lower Oolites (illustrated by Specimens from the Collection of Thos. Jesson): A. Smith Woodward.—A Short Account of the Excursion to the Volcanic Regions of Southern Italy (illustrated by Photographic Views): Dr. H. J. Johnston Lavis.

SATURDAY, JANUARY 4.

ROYAL INSTITUTION, at 3.—Electricity (adapted to a Juvenile Auditory): Prof. A. W. Rücker, F.R.S.

SUNDAY, JANUARY 5.

SUNDAY LECTURE SOCIETY, at 4.—Ballooning in the Service of Science (with Oxyhydrogen Lantern Illustrations): Eric S. Bruce.

MONDAY, JANUARY 6.

VICTORIA INSTITUTE, at 8.—Iceland: Rev. Dr. F. A. Walker.
SOCIETY OF CHEMICAL INDUSTRY, at 8.—Peroxide of Hydrogen, its Preservation and Commercial Uses: C. T. Kingzett.
ARISTOTELIAN SOCIETY, at 8.—Practical Certainty the Highest Certainty: R. E. Mitcheson.

TUESDAY, JANUARY 7.

ANTHROPOLOGICAL INSTITUTE, at 8.30.
ROYAL INSTITUTION, at 3.—Electricity (adapted to a Juvenile Auditory): Prof. A. W. Rücker, F.R.S.

WEDNESDAY, JANUARY 8.

GEOLOGICAL SOCIETY, at 8.—On some British Jurassic Fish-remains referable to the Genera Eurycormus and Hypsocormus: A. Smith Woodward.—On the Pebidian Volcanic Series of St. Davids: Prof. C. Lloyd Morgan. The Variolitic Rocks of Mount Genèvre: Grenville A. J. Cole and J. W. Gregory.
ROYAL MICROSCOPICAL SOCIETY, at 8.—On the Variations of the Female Reproductive Organs, especially the Vestibule, in Different Species of Uropoda: A. D. Michael.
SOCIETY OF ARTS, at 7.

THURSDAY, JANUARY 9.

ROYAL SOCIETY, at 4.30.
MATHEMATICAL SOCIETY, at 8.—On the Deformation of an Elastic Shell: Prof. H. Lamb, F.R.S.—On the Relation between the Logical Theory of Classes and the Geometrical Theory of Points: A. B. Kempe, F.R.S.
ROYAL INSTITUTION, at 3.—Electricity (adapted to a Juvenile Auditory): Prof. A. W. Rücker, F.R.S.

FRIDAY, JANUARY 10.

ROYAL ASTRONOMICAL SOCIETY, at 8.
INSTITUTION OF CIVIL ENGINEERS, at 7.30.—The] Irrigation Works on the Cauvery Delta; Alfred Chatterton.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Challenger Report; Physics and Chemistry, vol. ii. (Eyre and Spottiswoode).—Manuel de l'Analyse des Vins: E. Barillot (Paris, Gauthier-Villars).—Traité de Photographie par les Procédés Pelliculaires, tome premier et second: G. Balagny (Paris, Gauthier-Villars).—Leçons sur la Théorie Mathématique de l'Électricité: J. Bertrand (Paris, Gauthier-Villars).—Sundevall's Tentamen, translated by F. Nicholson (Porter).—The Nests and Eggs of Indian Birds, vol. i., 2nd edition: A. O. Hume. edited by E. W. Oates (Porter).—The Cosmic Law of Thermal Repulsion (New York, Wiley).—Old Age: Dr. G. H. Humphry (Cambridge, Macmillan and Bowes).—A Hand-book of Quantitative Analysis: J. Mills and B. North (Chapman and Hall).—Alternate Elementary Physics: J. Mills (Chapman and Hall).—Solutions to the Questions set at the May Examinations of the Science and Art Department, 1881 to 1886; Pure Mathematics, Stages 1 and 2: T. T. Rankin (Chapman and Hall).—Perspective Charts for Use in Class-teaching: H. A. James (Chapman and Hall).—Theoretische Mechanik Starrer Systeme: Sir R. S. Ball, herausgeben von H. Gravelius (Berlin, Reimer).—Prodromus of the Zoology of Victoria, decade xix.: F. McCoy (Trübner).—The Garden's Story, 2nd edition: G. H. Ellwanger (Appleton).—New Light from Old Eclipses: W. M. Page (St. Louis).—A Trip through the Eastern Caucasus: Hon. John Abercromby (Stanford).—A Manual of Palæontology, 2 vols., 3rd edition: H. A. Nicholson and R. Lydekker (Blackwood).—A Thousand Miles on an Elephant in the Shan States: H. S. Hallett (Blackwood).—Descriptions of Eight New Species of Fossils, &c.: J. F. Whiteaves (Montreal).—Victoria Water Supply, Third Annual General Report (Melbourne).—Studies from the Biological Laboratory, Johns Hopkins University, vol 4, No. 5 (Baltimore).—Journal of the Asiatic Society of Bengal, vol. 58, Part 2, Nos. 1 and 2 (Calcutta).—Journal of the Anthropological Institute, November 1889 (Trübner).—Journal of the Royal Microscopical Society, December (Williams and Norgate).—Proceedings of the Royal Society of Queensland, 1889, vol. 6, Part 5 (Brisbane).—Zahl und Vertheilung der Markhaltigen Fasern im Froschrückenmark, No. 9 (Leipzig, Hirzel).—Notes from the Leyden Museum, vol. xi., No. 4 (Leyden, Brill).—The Quarterly Journal of Microscopical Science, December (Churchill).

CONTENTS.

THURSDAY, JANUARY 9, 1890.

THE ZOOLOGICAL RESULTS OF THE "CHALLENGER" EXPEDITION.

Report on the Scientific Results of the Voyage of H.M.S. "Challenger" during the Years 1873–76, *under the command of Captain George S. Nares, R.N., F.R.S., and the late Captain Frank T. Thomson, R.N.* Prepared under the superintendence of the late Sir C. Wyville Thomson, Knt., F.R.S., &c., Director of the Civilian Staff on board, and now of John Murray, LL.D., Ph.D., &c., one of the Naturalists of the Expedition. Zoology—Vols. XXXI. and XXXII. (Published by Order of Her Majesty's Government. London: Printed for Her Majesty's Stationery Office, and sold by Eyre and Spottiswoode, 1889.)

WITH these recently published volumes, the series of Reports on the zoological results of the *Challenger* Expedition, comes to a close. Volume XXXI. contains three Reports, the first of which is on the "Alcyonaria," by Profs. E. Perceval Wright and Th. Studer. It would appear that on the first distribution of the zoological treasures of the Expedition, the Alcyonaria were given to Prof. von Kölliker to describe, and the first part of his Report on the Pennatulidæ, forms the Second Report published in 1880. From a note of the editor, we learn that Prof. Kölliker being unwilling to undertake the remainder of the group, the fixed forms were committed to Dr. E. P. Wright for description. After the appearance of the "Narrative of the Expedition" in which a few of the more remarkable of the new species were described by this author, Prof. Studer consented to join Dr. Wright in preparing the Report, and all the details were worked out in unison.

The Report opens with a brief introduction, in which an attempt is made to present a more or less complete list of the orders, families, and genera, of the recent Alcyonaria; short diagnoses and references to the bibliography are given. While this introduction might with advantage have been greatly expanded, yet we think its value will be appreciated by all those working at this group. This is followed by the description of the genera and species in the *Challenger* collection. In the earlier pages an attempt has been made to include brief notices of all the known forms, but it was soon found that this would occupy too much space, as the forms from large portions of the Indian Ocean and the very rich Alcyonarian fauna of the western shores of North America were not represented in the collection.

One hundred and eighty-nine species are described as found during the voyage of the *Challenger* and of this number no less than one hundred and thirty-three are described as new. Of the more interesting of these, the following may be mentioned, *Callozostron mirabilis*, a most extraordinary species taken in the Antarctic Sea, in the most southerly dredging made during the voyage. While there can be no doubt as to its affinities yet this form presents many puzzling features. Another remarkable species from the Fiji's, *Calypterinus allmani*, although it has a rigid axis, in the arrangement of its polyps shows some relationship to the previously mentioned species A great number of new species are added to a genus quite recently described by Verrill, and which is made the type of the family Dasygorgidæ. The new genus Acanthoisis, which is nearly related to the well known genus Isis, exhibits an unique condition of its axis, which consists of alternate horny and calcareous joints, the latter being very beautifully grooved and spined. *Keroeides koreni*, with a sclerogorgic axis, from Japan, is also a curious species, with massive spicules.

Under the heading of "Geographical Distribution," a brief history is given of the distribution of the species of most of the well established genera; while this subject is necessarily very incomplete, yet it would seem as if the West Indian Islands, the Californian shores of America, the Australian seas and especially those of Japan were the chief centres of the group. But it cannot be overlooked that the record is very imperfect and that the recent researches of Danielssen have proved that immense numbers of species exist in the seas of Norway.

This Report extends to 386 pages and is illustrated by 49 lithographic plates, the figures in which have been drawn by Mr. George West, Jun., and Mr. Armbruster of Berne.

The second Report is by Dr. Günther, on the pelagic fishes, and comprises an account of the specimens which were obtained in the open ocean by means, chiefly, of the surface net.

The specimens were as numerous as those of either the shore or deep-sea fishes, described in the author's first and second Reports on the *Challenger* fishes, and by far the greater number were of small size; some, indeed, had been taken at so early a stage in their development as to make it impossible to refer them to their family or even order. The pelagic fish fauna, as defined by the author, consists, first, of the truly pelagic fish—those which habitually live on the surface of the ocean, accidentally and rarely approaching the shore; the majority breed in the open sea and pass through all their phases of growth without coming into the vicinity of land; numerous representatives of these were in the collections. Secondly, there are a number of fishes inhabiting the depth of the ocean, from a hundred fathoms downwards, which seem periodically to ascend to the surface, possibly in connection with their propagation; most of these are found at the surface, only during the early stages of their growth, but they connect the truly surface fishes with the deep-sea fishes, and were fairly well represented in the collection. Thirdly, the pelagic fauna receives a very considerable contingent from the littoral fauna; some shore fishes, when in a young state, are, while floating on the surface, driven to sea to great distances by currents and winds; many such immature forms were found. And, lastly, fully developed specimens of littoral species sometimes stray or are accidentally driven out to the open sea, and several such were in the collection.

Sixty-seven species are indicated, and several new genera and species are described. A new species of Branchiostoma is described from the Pacific; it was either from the surface or from a depth of 1000 fathoms; the perfect condition of its delicate fin-fringe seemed to militate against the latter idea, and yet it would be even more extraordinary to find a lancelet living at the surface

of the open sea. This Report extends to 47 pages, and has six plates.

The third Report is by Arthur W. Waters, and is entitled a "Supplementary Report on the Polyzoa.' From every point of view we regret that these "notes the time for the preparation of which has been limited by Mr. Murray," have been published as part of the present series of Reports.

If the Reports on the *Challenger* Polyzoa by the late George Busk, which form Parts XXX. and L. of the zoological series, had been defective, say, for example, that a number of new or rare species had escaped description, then it would have been useful and perhaps excusable to have had a supplemental Report issued, noting such; but out of the 41 pages of which this Supplementary Report consists, not more than one and a half are devoted to the record of the three new species described, while the rest is simply a series of criticisms on the late Mr. Busk's work.

The very heading of the Report contains an implied piece of criticism, "The term Polyzoa is used for sake of uniformity." Into the argument *pro* and *con* for the use, of this term it is not needful for us here to enter, but remembering what Mr. Busk had written to justify its use, this uncalled-for remark might have been omitted. We read:—

"Shortly after the death of Mr. George Busk, who prepared the Report on the *Challenger* Polyzoa, I had, through the kindness of his daughter, Miss Busk, an opportunity of examining some of the duplicate specimens, and I desire to thank her for sending me those which, from published criticism, were most interesting to me. I have also to thank Mr. John Murray, the director of the *Challenger* publications, for allowing me to examine the whole of the duplicate material in Edinburgh. I communicated to Mr. Murray some valuable results arising from an examination of sections of the *Challenger* specimens prepared by a method similar to that employed in the examination of fossil Polyzoa, and at his request I have drawn up the following supplementary notes on the *Challenger* species."

We have been careful to quote the author's own account of his work, which would have formed an interesting communication to any of our scientific Societies, but which seems to us to be quite out of place where it is now published. There is probably not one of the eighty-two Reports published on the zoological results of the *Challenger* Expedition that could not be added to and emended, and no one would wish that they should escape every just criticism, but this is quite a different thing from employing the funds placed by the Treasury for the publication of these Reports on the printing and illustrating of critical notes on the already published ones. This supplementary Report is illustrated by three plates from drawings of the author.

In the editorial notes to Vol. XXXII. we are told:—

"This volume concludes the zoological series of Reports on the scientific results of the Expedition, with the possible exception of a few supplementary notes to some of the memoirs and Prof. Huxley's Report on the genus Spirula, which may appear as an appendix to the concluding summary volume."

We must content ourselves with protesting against the publication of any further "supplementary notes" on the Reports unless these are contributed by the several authors thereof. As to a "concluding summary volume," opinions may differ as to the advisability of publishing a summary of the thirty-two volumes in the same series as the original volumes. For the scientific worker such a summary would be quite useless, for any such would have recourse to the full details. For the general reader, anxious to know something of the facts stored away, beyond his reach, in these many ponderous volumes, a summary would no doubt be of interest, and, if fairly well executed, of value, but the size and cost of a volume like those already published in this series would place such far beyond the buying powers of most people, and to us it would seem a waste of public money to undertake so unnecessary a labour. If, indeed, the Treasury would publish, in a convenient handy volume, a carefully prepared sketch of the cruise of the *Challenger*, with a few chapters added giving a summary of the additions to biological knowledge, which were the immediate results of the Expedition, such a volume would be acceptable to the general public, and would let them know more than they at present do of the most important voyage of discovery of this century.

The first Report in Volume XXXII. is on the Antipatharia by George Brook, and we believe it to be one of the most praiseworthy of all the Reports; the time at the disposal of the author was of necessity very short, and perhaps no group of marine animals had been so little attended to. Our Museums no doubt possessed numerous specimens, but these being in the great majority of cases, only the dried skeletons, presented little upon which to work, there were therefore many and serious drawbacks to a determination of the species or to a knowledge of their anatomy. In spite of all this Mr. Brook has succeeded in making this Report an excellent contribution to our knowledge of the classification, distribution, and anatomy of the group. There was one fortunate circumstance about the *Challenger* specimens, most of them had the polyps well preserved, so that their structure could be fairly well made out. Making the most of the material at his disposal, the author has attempted a partial revision of the group, and has placed the classification for the first time on a natural basis. The study of the fine collections made by Pourtales and during the voyages of the *Blake*, would have greatly assisted Mr. Brook's labours, but as in the case of the Alcyonaria, the specimens were not available.

Nearly all the forms collected by the *Challenger* were new, which is to be largely accounted for, by the fact that almost all the collections were made in localities from which no Antipatharia had been previously recorded. The collection is remarkably deficient in littoral forms, but a number of species are now for the first time described from great depths. In this monograph not only are all the *Challenger* species described but a number of new species in the British Museum are also described, so that the Report forms quite a monograph of the group.

The Report opens with a bibliography, not a very extensive one, and one which up to the time of Pallas, possesses little interest. Botanists like Bauhin, Tournefort, and Breynius are among the pre-Linnæan writers who refer to these corals, and it is worthy of note that the last mentioned of these authors, describes and gives an

excellent figure of a species of Antipathes, in his "Prodomus fasciculi rariorum plantarum anno 1679 in hortis celeberrimis Hollandiæ, etc., observatarum." He calls it *Abies maritima*, and mentions it as a fossil plant; thus beginning his Prodomus with a form which was not a plant, and which certainly never grew in any of the Dutch gardens. After the bibliography there is a critical review of the literature; it is pleasing to find the author doing justice to Esper's "beautiful work 'Die Pflanzenthiere,'" and without wishing to enter on any technical criticism in a general notice like this, we may mention, in reference to a remark that "Esper does not describe *Antipathes ericoides*, but gives a figure of it," that in the second volume of his work, p. 150, he tells us that the name *Antipathes myriophylla* should replace the name of *Antipathes ericoides* engraved on the plate, and having a delamarck's[1] copy of the "Fortsetzungen der Pflanzenthiere" open before us, we may add that nearly all the references to Part ii. of this work in Mr. Brook's Report should be to Part i. Part ii. contains only 48 pages, and *Antipathes virgata*, Esper, is the only species of the genus described in it. In justice to Esper it may be also mentioned that he corrects his mistake of describing a decorticated gorgonid as *A. flabellum* (*vide* "Pflanzenth. forts.," ii. Th. p. 33).

The general morphology is next treated of, a general outline of the structure of the various genera, more especially with regard to the forms of the zooids and the number of and relative development of the mesenteries; this is the first detailed outline of the kind yet published on the morphology of the group, and it is illustrated by woodcuts. The classification and description of the genera and species follow; then notes on the geographical and bathymetrical distribution. Four species were taken at depths of between 2000 and 3000 fathoms.

A chapter on the anatomy concludes the Report, but we must content ourselves with quoting only the last few words of this most valuable contribution:—

"The Antipathinæ approach the Cerianthidæ more closely than the Hexactiniæ in structure, particularly in the following points: the arrangement of the mesenteries; the relatively thin mesoglœa, which is entirely devoid of stellate connective tissue cells; the presence of an ectodermal muscular layer in the stomodæum and body wall; and the rudimentary condition of the musculature of the mesenteries."

This Report extends to 222 pages, and has an atlas of 15 plates.

The second Report in this volume is by Prof. Th. Studer, M.D. Bern, being a "Supplementary Report on the Alcyonaria." We quote the short preface:—

"After the main Report on the *Challenger* Alcyonaria was in the press, several further specimens were found. These were in part new species, of which however, it was no longer possible to insert a description in the text. I am under great obligations to Dr. John Murray, the editor of the *Challenger* Reports, for allowing me to publish in the form of a supplement an account of these new species with the necessary illustrations. At the same time I have seized the opportunity to insert further illustrations of such forms as Dr. Wright and myself had only been able to describe in the Report, as *Telesto trichostemma* and *Siphonogorgia kollikeri*. This supplement extends the list of the *Challenger* collection by three new species of the genus Siphonogorgia, three Muriceidæ, an Indian representative of the genus Bebryce (which before had been known only from the Mediterranean), and one of the Plexauridæ."

It seems surprising that as a matter of courtesy, quite apart from other considerations, either the editor of these Reports or the author of this supplementary one, could have brought out this 81st Part of the *Challenger* Reports, without any communication with or participation therein, by Prof. Wright, to whom the preparation of the Report of the fixed Alcyonaria was originally committed.

With personal matters the reader has no right to be troubled, but he may well inquire why, when the Report itself was published in 1889 as the joint work of two Reporters, who narrate in their preface how pleasantly they worked in unison, there should appear in the same year this supplementary Report, written by but one of the two, and why he should acknowledge "his great obligations to Dr. Murray for enabling him to describe seven new species, under his own name," which had been found not by himself, but had been transmitted to him by his co-reporter as new forms early in 1888. The dates of the reception of the manuscript of this supplement prove that it could have been easily added to the appendix to the Report.

This supplementary Report adds eight, not seven as stated in the preface as quoted above, to the species collected during the cruise of the *Challenger*. The "Indian representative of the genus Bebryce" belongs to the Muriceidæ; but the interesting *Sarakka crassa*, Dan., belonging to the Alcyonidæ must be added to the list. Seven new species are described and figured, in addition to the last mentioned species, and figures are given of *Siphonogorgia kollikeri* and *Telesto trichostemma* which were described in the original Report. To the fourteen pages of the Report is added a list of the Alcyonaria (Pennatulacea excepted) obtained during the voyage, arranged according to the order of the stations at which they occurred; this comparatively useless record occupies ten pages, and is followed by a four page account of the bathymetrical range of the species, which takes no account of the record of the ranges as given in the original Report, which omits references to some of the *Challenger* forms and alludes to a large number of genera not found by the *Challenger*.

The six plates have been well drawn by Armbruster of Berne.

The third Report and the last of the series is by Prof. Ernst Haeckel, on the deep-sea Keratosa.

It will be remarked that this is not a "supplementary" Report to the Report on the Keratosa by Dr. Poléjaeff published in 1884, and it may be mentioned that the forms herein described appear to be of a very doubtful nature, "several spongiologists (among them some well known authorities) had denied their sponge nature and declared that these peculiar objects were either Rhizopods or other Protozoa. Other naturalists on the contrary who were closely acquainted with the Rhizopods, could not acknowledge their Rhizopod nature, neither could they make out the class to which they belonged." Possibly Prof. Haeckel was even one of these later for he tells us that "A closer comparative examination of these doubtful

[1] So Lamarck has written his name on the title-pages.

organisms of the deep sea has led me to the conviction that they are true sponges, for the most part modified in a peculiar manner by the symbiosis with a commensal organism which is very probably in most cases (if not in all) a Hydropolyp stock."

Four families and eleven genera of these strange forms are described, and the species are well illustrated. With some few of them we may have had a previous acquaintance, but these turn up here with quite new faces; for, "to avoid further confusion," the author "proposes to employ the term Haliphysema for that monothalamous Foraminifer in the sense of Mobius, Brady, and most recent authors"; while "for the true Physemaria, however," which he described in 1876 "as *Haliphysema primordialis*, &c., it will be best to adopt the term Prophysema," and he thinks that "it may be that the body-wall (in these Physemaria) is perforated by numerous microscopical pores, and that these were closed temporarily and accidentally during the few hours I was examining them; in this case they are Ammoconidæ," that is, belong to the first family of these deep-sea Keratosa.

In the truly extraordinary forms placed in the fourth family of Stannomidæ, containing specimens taken from depths of between 2425 and 2925 fathoms, we find present a fibrillar spongin skeleton, composed of thin, simple or branched spongin fibrillæ, never anastomosing or reticulated and also symbiotic Hydroids. Haeckel thinks that these "fibrillæ" throw some light on the peculiar filaments met with in the Hircinidæ, and that in both instances these fibres are not independent organisms, but are produced by the sponges, in which they occur, and should be regarded, as "monaxial Keratose spicules."

In concluding this notice of one of the most remarkable of the series of animal forms found during the expedition of the *Challenger*, we feel compelled to protest against the style of the author's criticisms on Poléjaeff's previously published Reports on the Keratosa. It is very easy to write that "the whole systematic work of Poléjaeff turns in a large *circulus vitrosus*," &c., &c., but is it fair or just for one Reporter to thus, at the expense of Her Majesty's Treasury, write of a fellow Reporter? Such sentences must have been overlooked by the editor.

This Report extends to ninety-two pages, and is accompanied by an atlas of eight coloured plates.

THE VERTEBRATES OF LEICESTERSHIRE AND RUTLAND.

The Vertebrate Animals of Leicestershire and Rutland. By Montagu Browne. Pp. 223, illustrated. (Birmingham and Leicester, 1889.)

As we are informed in the preface, the volume before us is the first complete work treating of the vertebrate fauna of the two counties mentioned in the title, which has hitherto appeared, although scattered notes and a few lists have been published by several writers. The author, who, from his position as Curator of the Town Museum at Leicester, has exceptional opportunities for a work of this nature, can certainly claim that the result of his labours does not err on the side of incompleteness. Thus this volume is not only a record of all the existing species of vertebrates which have been observed within the limits of the counties in question, but likewise includes the fossil forms hitherto described from the same area. The recent and extinct forms are, indeed, arranged together in a systematic manner, without any difference of type or other indication to distinguish at a glance the fauna of the present from that of the past; and it is certainly rather startling, at first sight, to find in a fauna of an English Midland county the dormouse immediately followed by elephants and rhinoceroses. Now, although we are not on the side of those who regard the sciences of zoology and palæontology as separated by a wide gulf, yet we venture to think that in this instance the author would have been better advised had he given his synopsis of extinct types in a separate portion of the volume, after having first dealt with the existing species. Faunas are, indeed, to a very large extent, features of one particular epoch; and when we have those of two or more distinct epochs mixed up together, we tend to lose sight of the peculiar features of each one. The ordinary student of the local distribution of existing English mammals will find that the introduction of a number of extinct types, of which he knows nothing, tends to distract his attention from the observations regarding the local distribution of the living forms. Fortunately, indeed, this objection does not apply to the birds, in which no extinct forms are recorded.

The very natural tendency on the part of the author to make as much as possible of his subject, probably accounts for the introduction of some groups or species which might have been better omitted, or, at all events, passed over with a brief foot-note. Thus, in the first place, the introduction of the family *Hominidæ* could have been very well spared, at all events in the systematic arrangement. Then, again, the devoting of nearly two pages to the order Cetacea seems to be very unnecessary, seeing that the only ground for the introduction of this order into the fauna of Leicestershire is that the bones of whales are sometimes used as gate-posts, or in one instance as an ornament to a carriage-drive! The author's remark in the latter instance that he records "these, lest, in the event of their getting loose and being subsequently dug up, they should be mistaken for bones of an extinct elephant," reads as though intended for a caustic sarcasm against palæontologists. As another instance, we may mention the case of the avocet (p. 150), introduced on the ground that a gentleman fishing at the junction of the Soar with the Trent, at the extreme northern limit of West Leicestershire, saw what he believed to be an example of this bird flying overhead. The inclusion of species on this account would almost justify passengers passing through a town by railway being entered among the list of visitors thereto.

The same natural tendency to make the most of the subject will probably account for the introduction of sub-ordinal and sectional names (*e.g.* Carnivora Vera, Æluroidea, Arctoidea, &c.), which are of no possible importance in a work of this nature, and are really an incumbrance.

The author tells us he has followed the latest descriptions throughout his work, and we see that in several instances he is even in advance of many writers in regard to the adoption of early names on the ground of priority. Thus the name *Microtus* is employed for the voles, in lieu of the well-known *Arvicola;* but in this particular

instance it would surely have been well for the author to have departed from his rule and introduced the latter term as a synonym. A still more glaring instance of the inadvisability of dropping all mention of synonyms occurs in treating of the lesser shrew (p. 13), for which the name *Sorex minutus*, Linn., is adopted, in place of the later *S. pygmæus*, Pall. Now, the author refers to Bell's "British Quadrupeds" for the distinctive characters of this species, which is there mentioned only as *S. pygmæus*; thus laying himself open to the criticism of those who are not specialists that he has confused the terms *pygmæus* and *minutus*. This species has, moreover, never been recognized in the district, so that its mention seems rather unnecessary. In discarding the name *Lepus timidus* in favour of *L. europæus* for the common hare, our author follows those who regard the letter of the law as more than the spirit; and although there is but little, if any, doubt that at least some of the hares to which Linnæus applied the name of *L. timidus* were really of that species to which we commonly apply the name *L. variabilis*, yet we cannot help thinking that the former name might be advantageously retained in its common acceptation.

Among the Ungulata, the author retains the fossil *Bos longifrons* (*frontosus*) as a distinct species, although it has been shown over and over again that it can only be regarded as a race of *B. taurus*. Similarly, all recent observations tend to show that *Bos primigenius* is nothing more than a larger variety of the same species; while there appear to be no valid grounds for specifically distinguishing the Pleistocene *Bison priscus* from the living Lithuanian aurochs. The author would confer a great benefit upon palæontologists if he could show how the skull he refers to the so-called *Sus palustris* can be specifically distinguished from one of *S. scrofa*.

In commenting upon the absence of remains of fossil Carnivora from the Leicestershire Pleistocene, Mr. Browne does not appear to be aware how extremely rare these remains are in the equivalent deposits of other counties. Thus, at Barrington, in Cambridgeshire, where bones and teeth of Ungulates are found by the hundred or thousand, those of Carnivores may be reckoned by units or tens; and the introduction of special hypotheses to account for their absence in Leicestershire is, therefore, quite superfluous.

The total number of mammals mentioned is forty-eight (including man), but of this list only twenty-five are now found in a wild state in the area described. The number of species of birds is very large, as we might expect in an area of the size of that forming the subject of the work. Several species, such as the gannet, cormorant, &c., are, however, but occasional stragglers from the coast; while in other cases, as we have already remarked, the evidence of occurrence within the two counties is of the slightest. A good lithographic plate of Pallas's sand-grouse, and a coloured one of the cream-coloured courser, are given; and we also have an elaborate table of the dates of arrival of summer immigrants. In the reptiles, the five existing species are almost lost among a number of fossil forms, to which they have but a very remote kinship. This swamping of recent forms by their fossil allies is, however, not so marked among the fishes, owing to the circumstance that all the fossil forms belong to extinct families, which follow the recent ones. Mr. Browne follows Prof. Cope in abolishing the orders Teleostei and Ganoidei, and arranging the representatives of the former and the typical groups of the latter in a sub-class Teleostomi, which is ranked as equivalent to the Elasmobranchii. The *Salmonidæ* are thus immediately followed by a family which the author, in defiance of all grammatical rules, terms *Leptolepidæ*, and which forms a transition from the Ganoids to the Teleostei. It seems strange that, while employing the correctly-formed term *Rhizodontidæ* (instead of *Rhizodidæ*), the author should retain names like *Leptolepidæ* and *Osteolepidæ* in place of *Leptolepididæ* and *Osteolepididæ*; but here, perhaps, he merely follows those who ought to know better. The number of fossil fishes from the Lias quarries of Barrow-on-Soar is very considerable; and we believe that the Leicester Museum is rich in this respect, as well as in the remains of Saurians from the same locality.

The author seems to have spared no labour in looking up references and making his work in all respects as nearly complete as possible; and, since the volume is handsomely got up and well printed, with a remarkable freedom from misprints, it should take a place in the first rank of local faunas.

R. L.

THE SCIENTIFIC PAPERS OF ASA GRAY.

Scientific Papers of Asa Gray. Selected by Charles Sprague Sargent. Two Vols. (London: Macmillan and Co., 1889.)

NO more fitting monument could have been raised to the memory of the late Dr. Asa Gray—who was almost as well known to botanists on this side of the Atlantic as on the other—than a reprint of a selection o his numerous writings. During a period of upwards of fifty years he was actively engaged in the investigation and publication of the botany of North America, and studies of a wider range. As Prof. Sargent says, in his preface to the present collection, "The number of his contributions to science and their variety is remarkable, and astonishes his associates even, familiar as they were with his intellectual activity, his various attainments, and that surprising industry which neither assured position, the weariness of advancing years, nor the hopelessness of the task he had imposed upon himself, ever diminished."

The hopeless task, it may be explained, was a complete "Synoptical Flora of North America." Botanists need not be told how he laboured to complete this gigantic undertaking, even at an age when most men are past work. Taking up the work where the unfinished "Flora of North America," by Torrey and Gray, ceased thirty-five years previously, Gray published the remainder of the Gamopetalæ in 1878. This was followed in 1884 by a re-elaboration of the Compositæ and neighbouring natural orders; and the whole was re-issued in the form of one volume in 1886. This volume comprises about 1000 closely printed pages of descriptive matter—descriptive matter perhaps unsurpassed in botanical literature, and dealing with 567 genera and 3521 species. Whatever may be done by Gray's successors towards completing the "Synoptical Flora," his own contribution is a

most valuable one—valuable because it embodies the whole of his numerous scattered writings on the group in question.

In making a selection of Dr. Gray's work for republication, Prof. Sargent naturally did not choose descriptive botany, though an index to the genera and species described in a variety of more or less inaccessible publications would be of the utmost service to botanists; for even under the most favourable conditions a long time must elapse before the completion of the "Synoptical Flora."

The selection, "which was found difficult and embarrassing," is limited to reviews of works on botany and related subjects, essays, and biographical sketches, and it is on the whole, doubtless, as good a one as could have been made. Gray wrote "more than eleven hundred bibliographical notices and longer reviews," and, as space for only fifty is found in a volume of 400 pages, it follows that "it was necessary to exclude a number of papers of nearly as great interest and value as those which are chosen."

Dr. Gray's method, if I may so term it, of reviewing the productions of his contemporaries was of such an instructive, temperate, and impartially critical character that these reviews have a permanent value. On reading some of them again, one is more than ever impressed with the fact that he made himself thoroughly acquainted with the works he criticized, and that he well fulfilled his duty alike to the public and the author. He did not hesitate to point out what he regarded as defects in the writings of his most intimate friends; but he was more careful to give an analysis of the contents of a book, with his own views thereon, than to condemn it on its faults or weak points.

These reviews cover a wide field, as well as a long period, and still remain profitable and interesting reading. The selection is too limited to be a history of botany during the last half-century, but it is sufficiently comprehensive to give an idea of the most notable events. It is true that the essays on the Darwinian theory are not here reproduced, as they had already been republished by their author.

The first volume, which is devoted to reviews, commences with a detailed notice of the second edition of Lindley's "Natural System of Botany" and ends with Ball's "Flora of the Peruvian Andes," reminding us of our most recent loss in the very small circle of private gentlemen who may be said to have studied botany successfully.

Early among the reviews is that of Endlicher's "Genera Plantarum," a work published at intervals between 1836 and 1840; and, almost at the end, a short article on the completion of Bentham and Hooker's "Genera Plantarum," 1862–83. In the latter we find a comparison of the number of genera admitted in various works of the same class, from the appearance of the first edition of Linnæus's "Genera Plantarum," in 1737, down to Bentham and Hooker, and remarks on the ideas of generic limits entertained by the different authors, and on the relative quality of their work.

Interspersed between these are notices of such widely different subjects as De Candolle's "Prodromus"; von Mohl's "Vegetable Cell"; Boussingault, "On the Influence of Nitrogen"; Bentham's "Hand-book of the British Flora"; De Candolle's "Géographie Botanique"; Hooker's "Distribution of Arctic Plants"; Ruskin's "Proserpina"; Darwin's "Insectivorous Plants"; and Wallace's "Epping Forest."

Among the fourteen "Essays" in the second volume, those on the longevity of trees, the flora of Japan, Sequoia, and forest geography and archæology, may be named as specially interesting.

The biographical sketches are thirty-eight in number, ranging from Brown and Humboldt to Bentham and Boissier. As only some two hundred pages are devoted to them, these sketches are, many of them, necessarily very brief; but, as Gray had a personal knowledge of most of the men of whom he wrote, they contain original and interesting observations and facts not to be found elsewhere. And all who knew Dr. Gray will enjoy reading again his opinion of other men and their works.

W. Botting Hemsley.

MANURES AND THEIR USES.

Manures and their Uses. By Dr. A. B. Griffiths. (London: George Bell and Sons, 1889.)

THIS is a hand-book for farmers and students, and may be described as a smaller and less ambitious successor to the treatise on manures, by the same author, reviewed some months ago in Nature. The principal value of this latter work consists in the direct information it contains as to sources of phosphatic, potassic, and nitrogenous manures, including guanos, in all parts of the world. The analyses, localities, amounts imported, and values, are all interesting facts for farmers, and this little book may well take its place in an agricultural library as supplying knowledge which otherwise might need research through many scattered sources of information. When, however, we consider the book as a means for imparting sound views on agricultural principles, we must advise caution on the part of the reader. Dr. Griffiths is one of those teachers who are infected with an inordinate affection for chemical manures. He believes, with M. Ville, that "the farmer who uses nothing but farmyard manure exhausts his land." Now, a man who starts with such an obvious fallacy can scarcely get into the right path. This doctrine is contrary to science and practice; and until Dr. Griffiths relinquishes it he cannot hope to enjoy the confidence of any farmer. We venture to put the matter in two or three positions from which it can be clearly viewed. Dr. Griffiths says, "This [farmyard] manure is erroneously supposed to contain *all* the necessary plant-foods required for the growth of crops." Erroneously! why, farmyard manure at least must contain all the constituents of straw, for it is largely made of straw. Similarly, it must contain the elements of turnips and root crops, when it is composed of them in no small proportion. Also it must contain the constituents of corn, because all meals and cakes which are consumed by cattle, and all hay, which is also consumed by cattle, contain the constituents of corn in the form of nitrogen, phosphorus, sulphur, potash, lime, magnesia, &c. Whether looked at chemically or approached through pure reasoning, it is clear that farm-

yard manure is the true restorer of fertility, the very milk of plants, the very life-blood of the soil, if such an expression may be allowed. Farmyard manure during its decay has its elements liberated from organic combinations gradually, and when wanted, as well as in a condition so available for the food of plants, that as a manure it is inimitable. No other manure can in all cases be applied to all crops with the same marked effects. It is strange that farmyard manure alone acts promptly and certainly upon leguminous crops such as beans, peas, and clover. No chemical manure, whether nitrogenous or phosphatic, can be relied upon to affect these crops, and yet farmyard dung tells upon them at once. Dr. Griffiths lays stress upon the fact that animals retain phosphates and nitrogen for the formation of bones, nerves, and muscles, and therefore to some extent rob the land. This fact is, however, entirely over-ridden by the customary importation of extraneous matter on to the farm in the form of foods purchased. The amount of phosphates and nitrogen removed by animals in their bodies is as nothing compared to the tons of cake, meal, hay, and even roots which are imported. Neither must we forget the town manure which is so often bought by farmers, and which will compensate for such a loss as that which Dr. Griffiths fears. Too much prominence is given to chemical manures, and too little importance is attached to stock-feeding as a manurial agency. Dr. Griffiths quotes many writers upon matters on which they are scarcely to be regarded as authorities. On such matters he might just as well have told us his opinion, instead of backing it up with the name of a solicitor who has been dead for years and whom nobody now knows of. Neither is an agriculturist, pure and simple, an authority on a chemical point such as the valuation of farmyard manure on the basis of its chemical constituent parts.

Dr. Griffiths claims to have made a discovery with regard to the use of iron sulphate as a fertilizer, and a good deal of space is devoted to this subject, which is not without interest. Half a hundredweight of iron sulphate per acre produces extraordinary results, according to experiments recorded in this book. No doubt this is Dr. Griffiths's great point, and far be it from us to detract from its significance. If it is as potent a fertilizer as Dr. Griffiths thinks, we shall probably hear more of it. He is evidently not the man to let the matter rest. W.

OUR BOOK SHELF.

Histoire Naturelle des Cétacés des Mers d'Europe. By P. J. Van Beneden. Pp. 664. (Brussels: F. Hayez. 1889.)

It is fifty-three years since the veteran Professor of Zoology in the University of Louvain published his first paper on the Cetacea, entitled "Caractères spécifiques des grands Cétacés tirés de la conformation de l'oreille osseuse." During the greater part of this long period he has made this group of animals especially his own, having industriously collected from every available source information upon them, which he has given to the world, not only in his great works on the osteology of the Cetacea and the fossil Cetacea of Antwerp, but also in a series of memoirs which have appeared from time to time in the publications of the Belgian Academy of Sciences. During the last three years the "Mémoires couronnés et autres Mémoires," published by that learned body in octavo form, have contained a number of articles from his pen upon the Cetacea of the European seas, and it has been a happy idea of the author to collect these together, and republish them in a handy form, so as to render them accessible to many who would have difficulty in referring to them when scattered throughout the pages of the journal in which they first appeared.

The work treats systematically of all the species known to inhabit any of the seas by which Europe is surrounded, and under each species are sections devoted to the literature, the history, the synonymy, the characters, the organization, the habits, the geographical distribution, the mode of capture, the museums in which specimens are known to exist, the published figures, and finally an account of the commensals and parasites which dwell upon or within them. On all these subjects the information given is derived from years of close and diligent gathering, and the result is an exhaustive account of our present knowledge of the European Cetacea. As a book of reference to all who are engaged in the study of cetology this work is absolutely invaluable, and if figures, even in outline, of all the species had been added, it might have gone far to occupy the place of the much-needed popular hand-book of this still little understood, though interesting order of mammals.

The number of species admitted is judiciously restricted, many of those appearing in previous works being relegated either definitely or provisionally to synonyms. Twenty-six are, however, left, all undoubtedly distinct forms. Of these, seven are whalebone whales, viz. *Balæna biscayensis*, *B. mysticetus*, *Megaptera boöps*, *Balænoptera rostrata*, *B. borealis*, *B. musculus*, and *B. sibbaldii;* five are Ziphioids, viz. *Physeter macrocephalus*, *Hyperoödon rostratus*, *Ziphius cavirostris*, *Micropterus sowerbyi*, and *Dioplodon europæus;* and the remaining fourteen are Delphinoids, viz. *Phocæna communis*, *Orca gladiator*, *Pseudorca crassidens*, *Globicephalus melas*, *Grampus griseus*, *Lagenorhynchus albirostris*, *L. acutus*, *Eudelphinus delphis*, *Tursiops tursio*, *Prodelphinus tethyos*, *P. dubius*, *Steno rostratus*, *Delphinopterus leucas*, and *Monodon monoceros*. The only exceptions we can take to this nomenclature are the adoption of the generic term *Micropterus* in preference to *Mesoplodon*, as the former was preoccupied by a genus of Coleoptera, and the use of the needless term *Eudelphinus* for the common dolphin. If this should be generally accepted, the good old Linnean genus *Delphinus* would disappear altogether from the list. That it should be greatly restricted by the lopping off of aberrant branches was inevitable, but surely the name might have been left for such a characteristic species.

W. H. F.

Hand-book of Practical Botany for the Botanical Laboratory and Private Student. By E. Strasburger. Edited, from the German, by W. Hillhouse, M.A., F.L.S. Second Edition, Revised and Enlarged. With 116 original and 33 additional Illustrations. (London: Swan Sonnenschein and Co., 1889.)

The first edition of Prof. Hillhouse's translation of Strasburger's "Practical Botany" was reviewed in NATURE (vol. xxxv. p. 556). The new edition has been considerably enlarged, and is now intermediate in extent between the smaller and the larger German editions. The new matter, mainly derived from the larger "Botanisches Practicum," second edition, adds greatly to the value of the book. The most important additions are the accounts of the reproduction of Fucus and of Chara, and of the fertilization and embryology of Picea. The much fuller description of the reproduction of Mucor must also be noticed, as well as the considerable alterations, affecting both text and figures, in the chapters on vascular bundles. Further, the structure of the grain of wheat is now described—a very useful addition.

Some verbal inaccuracies which had crept into the first translation have been corrected, and in every respect the editor may be congratulated on the work in its present form. It will be of the greatest use to students—especially, perhaps, to those who have to work alone.

D. H. S.

Traité d'Optique. Par M. E. Mascart. Tome I. (Paris: Gauthier-Villars, 1889.)

THIS is the first half of a very elaborate treatise on optics, the full scope of which we cannot tell till the second volume appears, as no hint is given of what is yet to come. This first volume begins with the fundamental principles of the wave-theory of light, deduces from them the elementary laws of geometrical optics, discusses the properties of a co-axal system of refracting surfaces, describes the structure of the eye, expounds the facts of colour-mixture, points out the conditions which determine the resolving power of a telescope, develops at great length the theories of diffraction and interference, with some of their principal applications, and devotes about 80 pages to polarization and double refraction. There is practically nothing about the microscope, and nothing at all about the paths of rays in media of continuously varying density.

The book is by no means easy reading, and the labour of perusing it is increased by the smallness of the reference letters (with their numerous accents and suffixes) which occur in the figures. The plan involves much specialization. For instance, the proof of the formula for retardation on which the theory of Newton's rings depends is not given in the sections devoted to Newton's rings and colours of thin plates, but some 370 pages earlier. In many cases, when the student has found a formula which appears to contain the information of which he is in quest, he has to search carefully through a long series of preceding pages before he can find the meaning of some symbol which occurs in it. The volume contains a vast store of information, but not generally in a form to suit hasty seekers after truth. It requires to be studied at leisure, and the time so spent will not be wasted. Great pains have obviously been taken to embody the latest information and present it in the clearest form. We may instance the spiral curves which illustrate the values of Fresnel's integrals, and the curve (to which a folding-plate is devoted) showing the relations of the colours of diffraction fringes to the three primary colours. There is an excellent discussion of the theory of concave gratings, both for reflection and refraction. The least attractive chapter is that entitled "Properties of Vibrations." It is a discussion of the composition of simple harmonic motions, and occupies 40 pages bristling with elaborate formulæ. We think a more moderate display of mathematics under this head would have sufficed.

The order of arrangement adopted in the volume is rather peculiar, and baffles all *à priori* conjecture. For instance, the discussion on colour-mixtures occurs in a chapter on "Interferences," and the investigation of the conditions which determine the resolving power of a telescope is given in the introductory chapter under the head of "Preliminaries."

The book is essentially a mathematical treatise, all experimental descriptions being reduced to the narrowest possible limits.

The preface states that the work is addressed mainly to "pupils of the Faculties and Schools of higher instruction," but we think its principal use in this country will be as a book of reference for teachers. Its value for this purpose will be greatly increased if a good alphabetical index is added at the end of the second volume.

J. D. EVERETT.

Bibliothèque photographique: Le Cylindrographe, Appareil panoramique. Par P. Moëssard, Commandant du Génie breveté, attaché au Service géographique de l'Armée. (Paris: Gauthier-Villars, 1889.)

THIS is a description of a photographic camera invented by Colonel Moëssard, in which the lens is pivoted on an axis, and the sensitive film is arranged in a cylindrical form about this axis, on a radius equal to the focal length of the lens. By this means a panoramic view of angular breadth up to 170° can be taken. The camera being fixed in position, the lens is uncapped, and then rotated quickly or slowly, according to the speed of the plate, and the intensity of light in any direction. The author claims for the instrument useful employment in surveying, either in the carefully detailed plans of an ordnance survey, or in the rapid views useful for warlike purposes, which the instrument can afford. Two photographs taken with the aid of the instrument illustrate very favourably its powers, especially for architectural purposes.

A Hand-book of Modern Explosives. By M. Eissler. (London: Crosby Lockwood and Son, 1889.)

IN this book the author of "Modern High Explosives" has collected much useful information about the various explosives now in use. The greater part of the work is devoted to nitro-compounds, but short accounts of the other types of explosives now being manufactured are added. The manufactures of gun-cotton and nitro-glycerine receive full treatment, together with the modifications introduced in the various large factories both of America and Europe. The important subject of the use of explosives in fiery mines has a chapter to itself. The description of the tests of flameless powders is of especial interest; in fact, the official reports of the tests of many of the most important explosives are perhaps the most instructive portions of the book. The chapter dealing with the practical application of explosives should be useful not only to the miner, but also to officers of both services to whom blasting and the use of explosives generally may at any time become a necessary auxiliary. An interesting account of the history and trials of the Lalinsky gun, together with the manufacture and use of gun-cotton shells, is also well worthy of their perusal. Little is said on the use of explosives below water, especially on the subject of the removal of wrecks, which would stand far fuller treatment. Four appendices are added, two dealing with the analysis and determination of stability of explosives, and one containing abstracts from the principal provisions of the Explosive Act of 1875. Although there is much that is necessarily old, still this is a book that will be read with interest by most who are accustomed to work with high explosives. The illustrations are well executed, and the whole wonderfully free from printer's errors.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Peltier Effect, and Contact E.M.F.

WITHOUT any further reference to the heading of a letter on p. 102, signed "The Reviewer," I wish to discuss an interesting argument therein propounded as proving that a true electromotive force at contact between two metals cannot be the cause or sole cause of the Peltier effect, unless the latter be simply proportional to absolute temperature. The argument is very like one that I indistinctly remember to have heard suggested some time ago by Prof. Schuster, and it struck me at the time as ingenious and not easily answerable.

On seeing it in print, however, a natural answer occurs to me, which it may be worth while to give. The whole point of the reasoning depends on assumed properties of vacuum.

The assumptions are as follow :—

(1) That a perfect vacuum is an absolute non-conductor of electricity.

(2) That no contact E M.F. exists between a metal and a vacuum.

(3) That vacuum has a specific inductive capacity.

Grant all these, and the argument is sound. Decline to admit any of them, and it proves nothing. Break down the first two of them, and it proves too much: it proves the non-existence of any thermal contact-force whatever between conductors. For if there were any E.M.F. at the metallic contact, and none at the other or vacuum contacts, a continuous current would flow, propelled by energy derived from a cold place.

This argument is indeed the ordinary one to prove that the algebraic sum of the E.M.F.'s at all the junctions of a closed conducting circuit in which no energy but heat is supplied must be zero when the temperature is uniform.

The proof scarcely holds when insulators are interposed, though the *fact* may be true nevertheless. When chemically active substances with their extraneous supply of energy are interposed, the fact itself is no longer true. But how do we know what is true when vacuum is interposed? The hypothesis on which the argument is founded is a baseless conjecture.

But it may be said, Are not the hypotheses probable? Do you not yourself believe them? I believe in (1) and (3) provisionally, but certainly not in (2). The contact E.M.F. between two substances is probably some surface action or skin phenomenon, and I see no reason why it should not occur as well in the boundary between metal and void as in the boundary between one metal and another. Indeed, it is not improbable that the sum of the E.M.F.'s in *every* circuit of chemically inert substances, whether conducting or not, and inclusive of vacuum, is zero under uniform temperature conditions.

All that is wanted to establish this is the knowledge that in a circuit of any one substance at non-uniform temperature the total E.M.F. shall be zero,[1] or that the Thomson effects in a single substance always balance each other; *i.e.* that the total E.M.F. in a circuit shall depend on a potential function of temperature, or $dE = f'(t)dt$.

Now it is quite true that this $f'(t)$ is the Peltier coefficient divided by absolute temperature, and that $f(t)$ in its most general form contains an arbitrary constant, but what of that? Nothing is known of $f(t)$ except that it is a potential function: it is not known to represent any physical effect. I never said that the Peltier effect enabled us to find the most general form of the function $f(t)$; I said it gave us the E.M.F. at a junction.

And there is much ground for the assertion; for it is easy to show that in a simple AB circuit, with junctions at t_1 and t_2, the total E.M.F. is

$$E = \Pi_1 - \Pi_2 + \int_{t_2}^{t_1} (\Theta_A - \Theta_B)dt;$$

just as if the resultant E.M.F. were the algebraic sum of two Peltier E.M.F.'s and of two Thomson E.M.F.'s.

My only contention is that this equation, which is undeniably true when the Π are interpreted as heat-coefficients, is also true and immediately interpretable when they stand for contact E.M.F.'s. The burden of proof as to the physical existence of an unnecessary and in every sense arbitrary constant rests with those who doubt this simple explanation.

It is difficult to see how a doubt can arise, or how the Peltier and Thomson productions or destructions of heat can be accounted for without local E.M.F.'s. Nohow, so Dr. Hopkinson has proved, and I also have insisted (*Phil. Mag.*, October 1885, and March 1886), except by some wildly gratuitous assumption of an actual physical specific heat for electricity, dependent on the temperature and on the metal in which it happens to be.

Liverpool, December 14, 1889. OLIVER J. LODGE.

Mirages.

THE article in NATURE of November 21, 1889 (p. 69), recalls to me mirages I saw in March 1888, while travelling in the East on the steam yacht *Ceylon*.

On the 29th we were crossing the Black Sea from Sebastopol. It was a fine cool day and quite calm. In the afternoon a false or mirage horizon about 3° above the true one was visible for a few hours. No objects were within range of vision. The mirage disappeared as the sun declined.

The next day was very much warmer, and we saw a more marked and interesting mirage in the afternoon as we were steaming across the Sea of Marmora away from Constantinople. In this case it appeared only in the west, and objects were seen reflected in an inverted position. A small conical-shaped island was seen with its inverted image at times distinct from and at times blending with the original. The image was distinctly seen of some land, which was actually below the horizon. The mirage of the reflection of the sun in the sea was, when seen through a glass, especially beautiful. It resembled a glorious cataract of golden water. This mirage lasted till quite the dusk of the evening, and then gradually thinned down and died away.

I do not know whether mirages at sea are uncommon; but as the officers on board did not remember seeing one before, I thought these instances might be worth recording.

ARTHUR E. BROWN.

Thought Cot, Brentwood, December 31, 1889.

Self-luminous Clouds.

I AM very sorry that I took no notes, some six or seven years ago, on the first and only occasion of my seeing self-luminous clouds, but though I can give neither date nor positions, the following facts are still fresh in my memory.

Passing through Bushey Park after dark, I noticed an aurora borealis, and, as I had only recently seen the rather rare phenomena of the rays of the setting sun converging towards a point in the east, I followed the direction of one of the principal beams of light towards the south, when, at a point somewhat south of my zenith, I noticed an equatorial belt of luminous clouds. I found that each cloud belonged to a ray, and faded and brightened with it, but was separated by about 60° of clear sky. This belt of clouds extended down to the western horizon, the eastern one was obstructed by trees, while shortly afterwards small dark clouds appeared on that side, and the sky soon became overcast.

The luminous clouds were quite transparent, so that even faint stars could be seen through them when at their brightest. I have heard from Scandinavian captains that these luminous belts are sometimes seen in northern latitudes, and are sure signs of bad weather. I have written these few remarks in the hope that those of your readers who may have the chance of seeing an aurora borealis will also look out for these clouds, and if possible determine their position. C. E. STROMEYER.

Strawberry Hill, January 4.

The Revised Terminology in Cryptogamic Botany.

THE anglicized forms of most of the terms in common use, employed in the "Hand-book of Cryptogamic Botany" recently issued by Mr. G. Murray and myself, have not up to the present time found much support from our fellow-botanists. I propose, therefore, to give, in some detail, the reasons which have induced us to adopt them, and to urge their general use on writers on cryptogamic botany. For this purpose we will take as our text extracts from three reviews of the "Hand-book," marked, as all the critiques have been, with only one or two exceptions, by a generous appreciation of the difficulties of our task, and a too great leniency to the many shortcomings of the work :—"The most conspicuous, though not the most important, of these [changes] is the adoption of anglicized terminations for Latin and Greek technical words. This is a matter in which it is hard to draw the line aright. . . . As a matter of taste we think the authors have gone much too far in this direction. They complain of the 'awkwardness and uncouth form of these words'; we should have thought the reproach applied much more strongly to 'cœnobe,' 'sclerote,' 'nemathece,' and 'columel'" (NATURE). "An Englishman may guess what 'archegone' is short for, for example; but why puzzle a foreigner with a new form of a word with which he is familiar in every treatise hitherto written on the special subject in any European language?" (*Academy*). "Too sanguine expectations on this head might well be toned down by remembering the complete failure of the somewhat similar experiment made by Lindley. . . . Primworts, spurgeworts, bean-capers, and hip-

[1] Hopkinson virtually pointed this out, *Phil. Mag.*, October 1885.

purids are decidedly simpler, even if less euphonious, than Primulaceæ, Euphorbiaceæ, Zygophyllaceæ, and Halorageæ; yet the longer Latin terms are still universally used, while the quasi-English ones have never obtained even temporary acceptance" (*Journal of Botany*).

The last of these criticisms appears to rest on a confusion between the principles of nomenclature and those of terminology. In nomenclature, rigid rules have been laid down, and accepted by all leading naturalists of all countries, in order that the scientific names of species, genera, orders, &c., may correspond in scientific treatises in all languages. In the terminology of flowering plants no such rule has ever been attempted to be laid down; but each writer, when writing in his own language, uses terms, usually of classical origin, and derived from common roots, but of a form as far as possible amenable to the laws of the language in which he writes. All that we are contending for is the extension of the same principle to cryptogamic botany; one of the main objects in the publication of our "Hand-book" being to make the study of flowerless plants as attractive to the public at large as is that of flowering plants.

In order to show how recent is the universal adoption of this practice in phanerogamic botany—a change largely due to the influence of Dr. Lindley's writings—we append a list of a few terms in use in standard works of original research or of reference, published within the last thirty-two years, which presented themselves the first to our hand; viz.—"The Miscellaneous Botanical Works of Robert Brown" (1866); Mr. Currey's translation of "Hofmeister on the Higher Cryptogamia, &c." (1872); Berkeley's "Introduction to Cryptogamic Botany" (1857); and Bentley's "Manual of Botany" (2nd ed., 1870):—

Achænium	Bentley	*Ovulum*	Brown
Anthera	Brown	*Perianthium*	Brown
Arillus	Bentley	*Pericarpium*	Brown
Bractea	Brown	*Pistillum*	Brown
Carpellum	Brown	*Rhizoma*	Berkeley
Integumentum	Berkeley	*Spermatozoon*	Currey
Involucrum	Brown	*Stamina* (plural)	Brown
Ovarium	Brown	*Stipula*	Currey

With the exception of words which have been incorporated into our language, such as *corolla*, *nucleus*, &c., comparatively few of those used in describing flowering plants now retain their classical forms; the most conspicuous exceptions being those applied to the structure of tissues, such as *epidermis* and those ending in *enchyma*; and can anything be more puzzling than the forms in common use for the terms derived from the Greek *δέρμα*—*epidermis*, *hypoderma*, and *periderm*? We have no doubt that, had our critic lived in the days of Robert Brown and Lindley, he would have thought all the innovations introduced by the latter "uncouth" simply because we were not used to them; and would have said that Lindley had "gone much too far." In some of those adopted by ourselves we have, in fact, been forestalled by others, as in the cases of *antherid* and *archegone* by Lindley, and *sporange* by Oliver.

We now come to the charge made by our critic in the *Academy*, that the terms we have introduced would "puzzle foreigners." Unfortunately, our polyglottism, or rather oligoglottism, will not allow us to vie with our reviewer in his acquaintance with every European language; we are compelled to confine ourselves chiefly to three; but these include by far the greater part of European botanical literature—in fact, every treatise which nine out of ten English readers will wish to consult in the original. The statement quoted above seems to have been rashly made.

In *Italian*, as far as our knowledge goes, the practice is absolutely uniform: no botanical writer of repute uses the classical forms; but every technical term has its Italian spelling and termination. To such an extent is this adaptation to the laws of orthography of the language carried, that we find "xylem" converted into *xilema*, "phloem" in to *floema*, "hormogonium" into *ormogoniô*, and "hyphæ" into *ife*; and this by the first writers "on special subjects."

Our acquaintance with *Swedish*, *Danish*, *Dutch*, and *Spanish* is too slight to allow us to speak with confidence; but in all these the general practice is, we believe, the same as in Italian, though not to the same extent; with the best writers, when writing in their own language, the use of terms with Latin or Greek terminations appears to be the exception rather than the rule.

In *French*, the practice is by no means so uniform as in Italian; but still that of the highest authorities is, on the whole, very decidedly in favour of French, rather than Latin or Greek, forms of the words in most common use. From works picked up almost at random, we select the following:—

Anthéridie	Van Tieghem, Guignard, Philibert, De Wildeman, Bornet, Thuret.	*Parenchyme*	Guignard, Heckel, Fayod, Bornet, Tulasne.
Archégone	Van Tieghem.	*Perithèce*	Costantin.
Baside	Tulasne, Roumeguère (*basidie*, Fayod).	*Pollinide* (Florideæ)	Guignard.
Capitule	Bornet.	*Procarpe*	Bornet, Thuret.
Conidie	Costantin, Roumeguère (*conid*, Bornet).	*Propagule*	Bornet.
Épiderme	Van Tieghem, Rénault.	*Prothalle*	Guignard.
Favelle	Bornet, Thuret.	*Pycnide*	Costantin, Roumeguère.
Gamétange	De Wildeman.	*Sclérote*	Van Tieghem, Fayod.
Glomérule	Bornet, Tulasne.	*Sore*	Thuret.
Gonidie	De Wildeman.	*Sporange*	Bornet, Thuret, Roumeguère, Tulasne, Van Tieghem, De Wildeman, Guignard, Philibert.
Hormogonie	Bornet.	*Stipe*	Fayod, Roumeguère.
Hyphe	De Wildeman.	*Stomate*	Philibert, Thuret.
Nucléole	Guignard.	*Thalle*	Thuret, Gay, De Wildeman, Fayod.
Oogone	De Wildeman (*oogonie*, Roumeguère).	*Zoosporange*	Flahault.
Opercule	Philibert.		
Ostiole	Thuret, De Wildeman.		
Paraphyse	De Wildeman.		

The great stronghold of the conservatives in terminology is the *German* language. No doubt a large mumber of the best writers do here maintain the classical form of most technical cryptogamic terms, including some in which it has already been abandoned with us, such as *conceptaculum*, *receptaculum*, *stolo*, and *perianthium*, just as we still meet with *ovarium*, *ovulum*, and *protoplasma*. This is no doubt largely due to the greater difficulty which the German language has than the French or our own in naturalizing aliens. But even here the practice is by no means uniform, and Germanized forms are coming yearly more and more into use. In order that there may be no question as to the recency and authority of the examples quoted, the following list has been compiled exclusively from the standard treatises in Schenk's "Handbuch der Botanik"; had other works of equal authority been consulted, the list might have been considerably extended:—

Apophyse	Goebel	*Hormogon*	Zopf
Archespor	Goebel	*Mycel*	Zimmermann
Basidie	Zopf	*Paraphyse*	Zopf
Carpogon	Falkenberg	*Parenchym*	Haberlandt, Zimmermann, Detmer, Schenk, Zopf
Cilie	Zopf	*Plasmod*	Zopf
Collenchym	Haberlandt, Zimmermann	*Prokarp*	Falkenberg
Conidie	Zopf	*Sklerenchym*	Haberlandt, Detmer, Schenk
Endospor	Goebel	*Sporogon*	Goebel
Enzyme	Zopf		
Epithel	Haberlandt		
Exospor	Goebel		

We do not mean that these words are exclusively used by the writers quoted; it is not uncommon to find the Latin and the German form used indifferently on the same page. It is noteworthy also that even the most rigid conservatives do not use the Latin form in the plural of such words as "oogonium," "sporangium," "antheridium," "sclerotium," &c., but always the German forms, *Oogonien*, *Sporangien*, *Antheridien*, *Sklerotien*, &c.; such words as "oogonia," "sporangia," "antheridia," "sclerotia," &c., are, as far as our experience goes, to be found only in English and American writings and in Latin diagnoses.

Analyzing, therefore, the statement that the Latin and Greek forms of words used in cryptogamic terminology are "familiar in every treatise hitherto written on the special subject in any European language," we find that in Italian the practice is unanimously, and in French (as also, we believe, in most other European languages) preponderatingly in the opposite direction; and that German is the only widely read language of Continental Europe in which even the weight of authority is still on that side.

There are some terms in which, no doubt, the classical form must be retained, especially those which, when deprived of their

classical termination, become monosyllabic, such as "thallus," "sorus," "hypha," and "ascus," just as we still speak of a "corolla," a "stigma," a "hilum," and a "raphe." But, with regard to the great majority of terms in current use in descriptive cryptogamic botany, we entertain not the smallest doubt that the change will gradually be brought about which has, within the last forty years, become established in phanerogamic botany; and we would venture to suggest to our fellow-workers in cryptogamic botany in this country and in America, whether it will not be best to accept it frankly once for all.

ALFRED W. BENNETT.

Exact Thermometry.

I AM quite in agreement with Prof. Sydney Young (NATURE, December 19, p. 152), that after the lapse of a sufficient time—let us say, an infinite time—the constant slow rise of the zero-point of a thermometer at the ordinary temperature will attain a definite limit; but I cannot accept his view that the effect of heating the thermometer to a high temperature is simply to increase the rate at which *this* final state is approached. If the results of experiment at the ordinary temperature be expressed in a mathematical formula which admits of making the time infinite, the limiting value of the rise (on that condition) will not exceed on the average 2° C., even in a thermometer of lead glass. After exposure to a high temperature, and in the same thermometer, so great an ascent as 18° C. is a possible measurement, actually realized. The two phenomena are therefore very different in their nature.

The view that, owing to the more rapid cooling of the outer parts of the bulb after it has been blown, the inner parts are in a state of tension, and that it is the gradual equalization of the tension throughout the glass that causes the contraction, has frequently been held, and will probably be for a long time the favourite hypothesis upon the subject. It breaks down, however, when we attempt to calculate what the amount of the contraction might be, on the supposition that it is well founded: only a very small portion of the contraction could be thus accounted for. I regret that I cannot now conveniently refer to Guillaume's interesting demonstration of this result.

Prof. Young has placed on record an experiment with three thermometers, which he heated to 280° C. The zero movement, however, only ranged from 1° to 1°·2,—small readings which might very possibly have been obtained, or not, on either of the thermometers at other times. It is consequently very difficult to draw any inference from this experiment. I may, however, mention that closed thermometers made of lead glass are very apt to show a rise of zero after heating to about 120° C. and upwards to some temperature in the neighbourhood of 270° C., and after that a descent of zero; the temperature of 280° C. would in that case be an unsatisfactory one for a test experiment, and the effect of plasticity might very possibly be masked. On the other hand, if the three thermometers were of hard glass, all the zero movements would in that case be greatly diminished, and the results would be in less bold relief.

I do not know any substance more curious or interesting in its properties than glass; and I should be glad if Prof. Young—into whose able hands the matter has fallen—could decisively test my suggestion that plasticity is the main cause of the zero ascent after 120° C. Probably it has little or nothing to do with the ascent at the ordinary temperature. It is, however, known that fine threads of glass are undoubtedly plastic at the ordinary temperature.

EDMUND J. MILLS.

Melrose, N.B., December 29, 1889.

THE PALÆONTOLOGICAL EVIDENCE FOR THE TRANSMISSION OF ACQUIRED CHARACTERS.[1]

MUCH of the evidence brought forward in France and Germany in support of the transmission of acquired characters, which has been so ably criticized in Weismann's recent essays, is of a very different order from that forming the main position of the so-called Neo-Lamarckians in America. It is true that most American zoologists, somewhat upon Semper's lines, have supported the theory of the direct action of environment, always assuming, however, the question of transmission. But Cope, the able if somewhat extreme advocate of these views, with Hyatt, Ryder, Brooks, Dall, and others, holding that the survival of the fittest is now amply demonstrated, submit that, in our present need of an explanation of the origin of the fittest, the principle of selection is inadequate, and have brought forward and discussed the evidence for the inherited modifications produced by reactions in the organism itself—in other words, the indirect action of environment. The supposed arguments from pathology and mutilations have not been considered at all: these would involve the immediate inheritance of characters impressed upon the organism and not springing from internal reactions, and thus differ both in the element of time and in their essential principle from the above. As the selection principle is allowed all that Darwin claimed for it in his later writings, this school stands for Lamarckism *plus*—not *versus*—Darwinism, as Lankester has recently put it. There is naturally a diversity of opinion as to how far each of these principles is operative, not that they conflict.

The following views are adopted from those held by Cope and others, so far as they conform to my own observations and apply to the class of variations which come within the range of palæontological evidence. In the life of the individual, adaptation is increased by local and general metatrophic changes, of necessity correlated, which take place most rapidly in the regions of least perfect adaptation, since here the reactions are greatest; the main trend of variation is determined by the slow transmission, not of the full increase of adaptation, but of the disposition to adaptive atrophy or hypertrophy at certain points; the variations thus transmitted are accumulated by the selection of the individuals in which they are most marked and by the extinction of inadaptive varieties or species: selection is thus of the *ensemble* of new and modified characters. Finally, there is sufficient palæontological and morphological evidence that acquired characters, in the above limited sense, are transmitted.

In the present state of discussion, everything turns upon the last proposition. While we freely admit that transmission has been generally assumed, a mass of direct evidence for this assumption has nevertheless been accumulating, chiefly in the field of palæontology. This has evidently not reached Prof. Weismann, for no one could show a fairer controversial spirit, when he states repeatedly: "Not a single fact hitherto brought forward can be accepted as proof of the assumption." It is, of course, possible for a number of writers to fall together into a false line of reasoning from certain facts; it must, however, be pointed out that we are now deciding between two alternatives only, viz. pure selection, and selection *plus* transmission.

The distinctive feature of our rich palæontological evidence is that it covers the entire pedigree of variations: we are present not only at but before birth, so to speak. Among many examples, I shall select here only a single illustration from the mammalian series—the evolution of the molar teeth associated with the peculiar evolution of the feet in the horses. The feet, starting with plantigrade bear-like forms, present a continous series of readjustments of the twenty-six original elements to digitigradism which furnish proof sufficient to the Lamarckian. But, as selectionists would explain this complex development and reduction by panmixia and the selection of favourable fortuitous correlations of elements already present, the teeth render us more direct service in this discussion, since they furnish not only the most intricate correlations and readjustments, but the complete history of the addition

[1] This article is an informal reply to the position taken by Prof. Weismann in his essays upon heredity. I have borrowed freely from the materials of Cope, Ryder, and others, without thinking it necessary to give acknowledgment in each case.

of a number of entirely new elements—*the rise of useful structures from their minute embryonic, apparently useless, condition*, the most vulnerable point in the pure selection theory. Here are opportunities we have never enjoyed before in the study of the variation problem.

The first undoubted ancestor of the horse is *Hyracotherium;* let us look back into the early history of its multicuspid upper molars, every step of which is now known. Upon the probability that mammalian teeth were developed from the reptilian type, Cope predicted in 1871 that the first accessory cusps would be found on the anterior and posterior slopes of a single cone, *i.e.* at the points of interference of an isognathous series in closing the jaws. Much later I showed that precisely this condition is filled in the unique molars of the Upper Triassic *Dromotherium*. These with the main cusp form the three elements of the tritubercular crown. Passing by several well-known stages, we reach one in which the heel of the lower molars intersects, and, by wearing, produces depressions in the transverse ridges of the upper molars. At these points are developed the intermediate tubercles which play so important a *rôle* in the history of the Ungulate molars. So, without a doubt, every one of the five main component cusps superadded to the original cones, is first prophesied by a point of extreme wear, replaced by a minute tubercle, and grows into a cusp. The most worn teeth, *i.e.* the first true molars, are those in which these processes take place most rapidly. We compare hundreds of specimens of related species; everywhere we find the same variations at the same stages, differing only in size, never in position. We extend the comparison to a widely separate phylum, and find the same pattern in a similar process of evolution. Excepting in two or three side lines the teeth of all the Mammalia have passed through closely parallel early stages of evolution, enabling us to formulate a law: *The new main elements of the crown make their appearance at the first points of contact and chief points of wear of the teeth in preceding periods*. Whatever may be true of spontaneous variations in other parts of the organism, these new cusps arise in the perfectly definite lines of growth. Now, upon the hypothesis that the modifications induced in the organism by use and disuse have no directive influence upon variations, all these instances of sequence must be considered coincidences. If there is no causal relationship, what other meaning can this sequence have? Even if useful new adjustments of elements already existing may arise independently of use, why should the origin of new elements conform to this law? Granting the possibility that the struggle for existence is so intense that a minute new cusp will be selected if it happens to arise at the right point, where are the non-selected new elements, the experimental failures of Nature? We do not find them. Palæontology has, indeed, nothing to say upon individual selection, but chapters upon unsuccessful species and genera. Here is a practical confirmation of many of the most forcible theoretical objections which have been urged against the selection theory.

Now, after observing these principles operating in the teeth, look at the question enlarged by the evolution of parallel species of the horse series in America and Europe, and add to the development of the teeth what is observed in progress in the feet. Here is the problem of correlation in a stronger form even than that presented by Spencer and Romanes. To vary the mode of statement, what must be assumed in the strict application of the selection theory? (*a*) that variations in the lower molars correlated with coincident variations of reversed patterns in the upper molars, these with metamorphoses in the premolars and pocketing of the incisor enamel; (*b*) all new elements and forms at first so minute as to be barely visible immediately selected and accumulated; (*c*) in the same individuals favourable variations in the proportions of the digits involving readjustments in the entire limbs and skeleton, all coincident with those in the teeth; (*d*) finally, all the above new variations, correlations and readjustments, not found in the hereditary germ-plasm of one period, but arising fortuitously by the union of different strains, observed to occur simultaneously and to be selected at the same rate in the species of the Rocky Mountains, the Thames Valley, and Switzerland! These assumptions, if anything, are understated. Any one of them seems to introduce the element of the inconstant, whereas in the marvellous parallelism, even to minute teeth markings and osteological characters, in all the widely distributed forms between *Hyracotherium* and *Equus*, the most striking feature is the constant. Viewed as a whole, this evolution is one of uniform and uninterrupted progression, taking place simultaneously in all the details of structure over great areas. So nearly does race adaptation seem to conform to the laws of progressive adaptation in the individual, that, endowing the teeth with the power of immediate reactive growth like that of the skeleton, we can conceive the transformation of a single individual from the Eocene five-toed bunodont into the modern horse.

The special application of the Lamarckian theory to the evolution of the teeth is not without its difficulties, some of which have been pointed out to me by Mr. E. B. Poulton. To the objection that the teeth are formed before piercing the gum, and the wear produces a loss of tissue, it may be replied that it is not the growth, but the reaction which produces it, which is supposed to be transmitted. Again, this is said to prove too much; why is the growth of these cusps not continuous? This may be met in several ways: first, in the organism itself these reactions are least in the best adapted structures, a proposition which is more readily demonstrated in the feet than in the teeth—moreover, since the resulting growth never exceeds the uses of the individual, there is a natural limit to its transmission; secondly, the growth of the molars is limited by the nutritive supply—we observe one tooth or part growing at the expense of another; third, in some phyla we do observe growth which appears to lead to inadaptation and is followed by extinction. In one instance we observe the recession of one cusp taking place *pari passu* with the development of the one opposed to it. These and many more general objections may be removed later, but they are of such force that, even granting our own premises, we cannot now claim to offer a perfectly satisfactory explanation of all the facts.

The evidence in this field for, is still much stronger than that against, this theory. To sum up, the new variations in the skeleton and teeth of the fossil series are observed to have a definite direction; in seeking an explanation of this direction, we observe that it universally conforms to the reactions produced in the individual by the laws of growth; we infer that these reactions are transmitted. If the individual is the mere pendent of a chain (Galton), or upshoot from the continuous root of ancestral plasm (Weismann), we are left at present with no explanation of this well-observed definite direction. But how can this transmission take place? If, from the evident necessity of a working theory of heredity, the *onus probandi* falls upon the Lamarckian—if it be demonstrated that this transmission does not take place—then we are driven to the necessity of postulating some as yet unknown factor in evolution to explain these purposive or directive laws in variation, for, in this field at least, the old view of the random introduction and selection of new characters must be abandoned, not only upon theoretical grounds, but upon actual observation.

Reading between the lines of Weismann's deeply interesting essays, it is evident that he himself is coming to this conclusion. HENRY FAIRFIELD OSBORN.

Princeton College, August 23.

A FIELD LAID DOWN TO PERMANENT GRASS.

A VALUABLE paper, by Sir J. B. Lawes, on the history of a field laid down to permanent grass, has been reprinted, by Messrs. Spottiswoode, from the Journal of the Royal Agricultural Society of England. The field in question forms part of the Rothamsted estate, and was laid down to permanent grass nearly thirty years ago, by Dr. Gilbert, to whom it was let in 1856. It has been mown for hay every year from the commencement; and in the present pamphlet Sir J. B. Lawes gives full particulars as to the economical results, the constituents supplied in the manures and removed in the crops, the changes within the soil in the formation of the meadow, and the botany of the meadow. The following are his summary and general conclusions:—

(1) By the judicious employment of manures, both natural and artificial, arable land has been converted into permanent grass, not only without loss, but with some profit to the tenant.

(2) The important constituents, nitrogen and phosphoric acid, were supplied in the manures in larger quantities than they were removed in the crops; but potash in only about the same quantity as it was removed.

(3) The application of dung, not only compensates for much of the exhaustion from the removal of hay, but it has a beneficial influence on the botanical character of the herbage.

(4) Although the grass has been mown every year for nearly thirty years, there has been a considerable accumulation of fertility within the soil.

(5) Analysis has shown that there has been an increase of nitrogen in the surface-soil, beyond that which could be explained by excess supplied in manure over that removed in crops, and by the combined nitrogen coming down in rain, and the minor deposits from the atmosphere. Part, if not the whole, of this increase is probably derived from the subsoil by deeply-rooted plants, which afterwards leave a nitrogenous residue within the surface-soil. Or, possibly, some of it may have its source in the free nitrogen of the atmosphere, brought into combination within the soil, under the influence of micro-organisms, or other low forms.

(6) In laying down arable land to permanent grass, especially if hay is to be removed, it is essential to supply, not only nitrogenous, but an abundance of mineral manures, and especially of potash, a large quantity of which is removed in the crops, and must be returned. When the grass is not mown, but fed, the exhaustion is much less, but it is greater when consumed for the production of milk than when for that of store or fattening increase.

THE TOTAL ECLIPSE OF DECEMBER 22.

MISFORTUNE has attended the double expedition sent by the Royal Astronomical Society to observe the total eclipse of December 22. In Africa observations were made impossible by bad weather. Observations were secured off the coast of French Guiana, but at a cost which is deeply to be deplored—the death of Father Perry.

The telegram received from Demerara is as follows:—"104 corona American Perry dead dysentery." With regard to the part of this telegram which needs explanation, the *Times* of January 6 says:—"104 is resolvable into the factors 2, 4, and 13, of which the first number means that the weather was only moderately good; the second that successful exposures were made with the Abney 4-inch lens, but that the development was not carried out, owing either to unfavourable climatic conditions, or possibly to the illness of Father Perry; and the third, that successful photographs were obtained with the 20-inch mirror, but again the development was not completed. The words corona American signify most probably that the corona was of the same form as that seen on January 1, 1889, when a total eclipse was successfully observed in California, and the form was then that now generally ascribed to a period of minimum sun-spots, elongated at the sun's equator and radial but short at the poles."

NOTES.

THE list of those who received New Year's honours and appointments included Brigade-Surgeon George King, F.R.S., Bengal Medical Service, Superintendent of the Royal Botanical Gardens, Calcutta. He has been made Companion of the most eminent order of the Indian Empire.

THE seventy-second anniversary of the Institution of Civil Engineers occurred last Thursday, when a revised list of the members of all classes showed that the numbers on the books amounted to 5904, representing an increase of $3\frac{1}{3}$ per cent. in the past twelve months.

THE Institution of Electrical Engineers will hold the first meeting of the current term this evening, when the President, Dr. John Hopkinson, F.R.S., will deliver his inaugural address.

THE annual general meeting of the Royal Meteorological Society will be held at 25 Great George Street, Westminster, on Wednesday, the 15th inst., at 7.15 p.m., when the Report of the Council will be read, the election of Officers and Council for the ensuing year will take place, and the President (Dr. W. Marcet, F.R.S.) will deliver an address on "Atmospheric Dust," which will be illustrated by a number of lantern slides.

THE *Mining Journal* is to be congratulated on the very admirable portait of Dr. Archibald Geikie which appeared in its issue of December 28. The portrait was accompanied by a short but very good account of Dr. Geikie's life and labours.

DR. RAOUL GAUTIER has been appointed Professor of Astronomy at the University of Geneva, and has at the same time been made director of the Observatory. His father, Colonel E. Gautier, retains his connection with the latter establishment, with the title of honorary director.

THE Professorship of Agriculture and Rural Economy at the Royal Agricultural College, Cirencester, vacant by the resignation of Prof. McCracken, has been conferred upon an old student and gold medallist of the College, Mr. James Muir.

THE arrangements of the Royal Botanic Society for 1890 include exhibitions of spring flowers on March 26 and April 23; summer exhibitions of plants, flowers, and fruit, on May 14 and June 11; and an evening *fête* and exhibition on July 2. Botanical lectures will be given on May 9, 16, 23, and 30, and on June 6 and 13. These lectures will be free to all visitors in the Gardens.

ON Thursday, January 16, Prof. R. Meldola, F.R.S., will begin a course of twelve special evening lectures at the Finsbury Technical College, on coal-tar products. The object of the course is to describe the technology of the raw materials manufactured from the tar. The theoretical treatment will serve as a general introduction to the chemistry of the aromatic compounds. A syllabus can be had on application to the College.

In May next, the six hundredth anniversary of the foundation of the University of Montpellier will be celebrated.

M. Cosson, member of the French Academy of Sciences, and the author of many memoirs on the flora of Algeria and Tunis, died a few days ago in Paris, and was buried on the 4th inst.

We review to-day the volumes which conclude the series of Reports on the zoological results of the *Challenger* Expedition. In a prefatory note introducing Vol. II. of the Report on Physics and Chemistry, just issued, Dr. Murray explains that with the exception of a volume on deep-sea deposits, which will be issued in March next, and a summary volume, which, it is hoped, may be finished in about a year thereafter, the entire series of Reports is now completed. These Reports have been issued at intervals during the last nine years, whenever ready, and without any reference to systematic arrangement. They are bound up in forty-seven large quarto volumes, containing 27,650 pages of letterpress, 2662 lithographic and chromo-lithographic plates, 413 maps, charts, and diagrams, together with a great many woodcuts.

Some time ago Mr. J. T. Cunningham, Naturalist at the Plymouth Marine Biological Laboratory, wrote to the *Times* about the occurrence of anchovies on the south coast of England. In another letter, printed in the *Times* on Wednesday, he has given some fresh information about the matter. From Mr. Whitehead, of Torquay, he learns that the sprat fishermen at that place were catching a number of anchovies in their sprat nets together with sprats; that about a fifth of their catches consisted of anchovies. Mr. Dunn has sent him specimens from Megavissey. These were caught, as it were, accidentally in pilchard nets. Mr. Cunningham has made inquiries among the pilchard and herring fishermen at Plymouth, and finds that almost every time they shoot their nets they catch a few anchovies—from one to a dozen. The mesh of a pilchard net is much too large to hold an anchovy, and these occasional specimens are caught only in parts of the nets that get entangled; they are not meshed in the ordinary way. Of the anchovies he has obtained from the pilchard fishermen, he says there is no doubt whatever as to their being of the same species (*Engraulis encrasicholus*) as those which we import from France and Italy.

A rather serious subsidence has occurred near Dane Bridge, Northwich. A large hole, nearly 10 feet deep and covering a space of 50 feet by 30 feet, has been formed near the roadway. The Bridge Inn is now 24 inches out of the perpendicular, or some 5 inches more than it was before the subsidence. The inn had been securely bolted and the walls secured some time since, otherwise it would probably have collapsed. Some wooden structures standing on the opposite side of the road have been rendered untenantable. The gas and water mains were dislocated, and had to be repaired by the local board.

The General Report of the Survey of India Department for 1887–88, which has recently been published, indicates a gradual increase in the annual amount of work done. The triangulation along the Madras Coast has been extended 370 miles in length; and similar operations have been conducted in Baluchistan, one series along a parallel of 30° N., and another along the meridian of 67° E., both meeting at Quetta and having an aggregate length of 270 miles. The topographical surveys during the year covered an area of 15,673 square miles. It is gratifying to note that the system, started in the previous year, of employing the village *patwáris* as cadastral surveyors has been continued with very encouraging results, the aggregate area surveyed cadastrally being 5435 square miles. The special telegraphic longitude operations were resumed, and 7 arcs of longitude in Southern India measured, with the particularly interesting result of indicating an excess of gravitation toward the ocean surrounding India. Geographical surveys in Burmah have been made on a large scale, the Ruby Mine tract receiving special attention. A valuable addition to our knowledge of Afghanistan is furnished by the report of Yusuf Sharif, who accompanied the Afghan Boundary Commission, and succeeded in surveying 4600 miles of new country on his return. The statistics of the output of maps and reproductions at the principal offices show a marked increase. The value of the Dehra Dun station for purposes of solar photography is forcibly demonstrated by the fact that photographs of the sun were obtained on no less than 327 days, and forwarded to the Solar Physics Committee, to complete the Greenwich series. The Report is accompanied by the usual maps and narratives of the various expeditions.

We owe a new and interesting application of photography to M. Bertillon, the well-known director of the Identification Department at the Paris Prefecture of Police. M. Bertillon has been devoting himself for some months to the study of the physical peculiarities engendered by the pursuit of different occupations. The police have frequently to deal with portions of bodies, and it would greatly aid their investigations to be able to determine the calling of the murdered person in each particular case. The hand is as a rule the part naturally most affected by the occupation, and M. Bertillon has taken a very large series of photographs, each one showing on a large scale the hands, on a smaller scale the whole figure of the workman at his work, so that one may see at a glance the position of the body, and which are the parts that undergo friction from the tools in use. From the hands of the navvy all the secondary lines disappear, and a peculiar callosity is developed where the spade handle rubs against the hand; the hands of tin-plate workers are covered with little crevasses produced by the acids employed; the hands of lace-makers are smooth, but they have blisters full of serum on the back and callosities on the front part of the shoulder, due to the friction of the straps of the loom; the thumb and the first joints of the index of metal-workers show very large blisters, whilst the left hand has scars made by the sharp fragments of metal. Experts in forensic medicine (Vernois among others) have before drawn attention to the subject, but this is the first time that an investigation has been carried out on a large scale, and in M. Bertillon's hands it should lead to the best results.

Shocks of earthquakes continue to be felt in the province of Semiryetchensk, Russian Turkestan. After September 12, they were felt nearly every day, the most severe shocks having been experienced on September 17, at 11.45 a.m.; on the 22nd, at 1.15 p.m.; on the 23rd, at 4.55 a.m. On September 30, at 6.30 p.m., there was a particularly severe shock, preceded by a loud underground noise.

Severe shocks of earthquake were felt on the northern and north-eastern shores of Lake Issyk-kul nearly every day from November 19 to December 5. Many chimney-pots in several villages were destroyed by the shock of November 19.

The latest information as to the earthquake which visited Lake Issyk-kul on July 12 is given in the *Akmolinsk Gazette*. It lasted from 3.15 to 3.30 a.m., and destroyed, or rendered uninhabitable, all buildings in the villages Uital, Sazanova, Preobrajensk, and Teplyi Klutch, of the Issyk-kul district. Eight persons were killed, and 43 injured, some of them severely. The greatest disasters, however, appear to have occurred among the Kirghizes, who camped in the Kunghei Alatau, on the northern shore of Lake Issyk-kul. They had no fewer than 26 killed and 15 injured. The numbers of cattle killed during the earthquake were: 283 horses, 75 horned cattle, and 379 sheep. Several villages of the district

of Vyernyi also suffered very much. At Przevalsk (formerly Karakol, on the southern shore) and the surrounding villages many houses were destroyed; while amidst the Taranchis of the district of Vyernyi 21 persons were killed and 2 severely injured. At Vyernyi itself (50 miles north of the lake) the earthquake was relatively feeble; but at Jarkend all houses were rendered uninhabitable. In the west of Lake Issyk-kul the shocks were feeble, but in the north the wave of the earthquake spread as far as Kopal (180 miles from Issyk-kul, as the crow flies), and even as far as Sergiopol, which is 380 miles distant from the northern shore of the lake.

THE Council of the Italian Meteorological Society, publishes an *Annuario Meteorologico*, in which will be found much useful information for general readers. The volume for 1890 contains 276 small octavo pages, and is divided into four parts:—(1) Ephemerides and astronomical tables. This part also contains a special appendix giving the concordance of the calendars and other particulars of the 17 eastern nations. (2) Tables for the reduction of meteorological observations, by Padre Denza, with useful examples of how the corrections are applied, and also meteorological and magnetical statistics. (3) Geographical and topographical elements, together with an instructive paper on recent electrical terms and measurements. (4) A series of short articles on various sciences, among which we may specially mention one by Padre Denza, on the mode of determining the meridian line and time, for the use of observers who have only simple instruments. The most recent ideas upon the formation of hail, by Prof. L. Bombicci. On the types of isobars which favour frosts, by Prof. P. Busin, with suggestions for any observers willing to work at this subject. And, on the cause of earthquakes, in which the various theories are discussed, by Dr. C. De Giorgi.

THE Deutsche Seewarte has published, in a separate memoir, the results of the meteorological observations taken at its nine coast stations for the two lustra 1876–80 and 1881–85, together with summaries for the whole decade. The work contains very useful information relating to the climate of Northern Germany, and the hope is expressed that other institutions will publish similar results for their respective systems. In *Symons's Monthly Meteorological Magazine* for November it is pointed out that the years begin with December, in opposition to the regulations of the Vienna Congress that the years should begin with January, and an explanation of this is asked for. The explanation is given in the introduction: by this method the Seewarte has been able to give seasonal means, as well as monthly means. The December observations, which precede those for January, are for the same year as all the other months, not for the preceding year. The greatest annual range of temperature is 107°·1 at Neufahrwasser. The greatest daily rainfall occurred at Hamburg—viz. 3·37 inches. The annual percentage of rainy days varies from 41·6 to 59·7.

THE Annual Report of the Chief Signal Officer of the United States, for the year 1889, sets forth the extended and important character of the meteorological work that is carried on. Apart from weather forecasts, and storm warnings, the duties include the gauging and reporting of rivers, the reporting of temperature and rainfall conditions for the cotton interests, frost warnings in the interest of agriculture, and the notification of advancing cold waves for the benefit of the general public. The Chief Signal Officer estimates that the gratuitous distribution of meteorological data in the United States in a single week is greater than in all Europe in the entire year. The weather forecasts are issued twice daily, at 8 a.m. and 8 p.m., for a period of twenty-four hours, and the percentage of success shows a general average of 81. The present system of flag signals gives clear and definite information as to whether a storm is to be light or severe, whether its centre is approaching or has passed the station, and from what quarter high winds are expected. With regard to scientific researches, systematic observations of atmospheric electricity have been made, to determine whether these could be made use of in weather forecasting, the result being that negative electricity may be observed without being in any way related to precipitation, past, present, or future, and that such observations do not promise to be of practical use. Prof. C. Abbe has prepared a popular and non-mathematical exposition of the laws of storms, with a view to their better prediction. The Chief Signal Officer states that the Report brings together many new results, and that Prof. Abbe finds the source and maintaining power of a storm in the absorption by the cloud of solar heat, and in the liberation of heat in the cloud by those particles that subsequently fall to the ground as rain or snow, and endeavours to show that the movement of the storm centre is principally influenced by the location and amount of such precipitation.

REMARKABLE electrical phenomena are witnessed at the new observatory on the steep and isolated Säntis (821 5) in Northern Switzerland. Thunderstorms are extremely frequent; thus in June and July last year, only three days were without them. As a rule, thunder peals from midday till evening. The noise is short, partly owing to shortness of flashes and partly to the small amount of echo. The thunderstorms come on quite suddenly, in a clear sky. One of the surest indications of their approach is the bristling of the observer's hair. During hail, the iron rods of the house give a hissing sound, associated with luminous effects.

M. E. HOSPITALIER, the electrician, has begun the publication of a work in two volumes, entitled "Traité Elémentaire de l'Energie électrique." The first volume, comprising the definition, principles, and general laws, has been issued. Vol. II., on industrial applications, will be issued during the present year.

IN the current number of the *American Naturalist* Mr. Clement L. Webster gives an interesting account of various "mound-builder mounds" near Old Chickasaw, Iowa. Speaking of three human skeletons found in one of these mounds, the writer says that the crania show "an extremely low grade of mental development." They are smaller than the Neanderthal skull.

M. VAYSSIÈRE has published the second part of his monograph of the Opisthobranchiate Mollusca of the Gulf of Marseilles. It contains many fine plates.

THE origin of the very extensive pampas-formation in South America, a humus-covered loess of fine dust-like material, from 100 to 160 feet thick, with limestone concretions, and numerous fine passages, has attracted the attention of several geologists. From an important recent contribution to the subject by Roth (German Geological Society), it would appear that wind, river, lagoon, and coast deposits may all be distinguished in the pampas. The coast deposits are chiefly recognized by sand and marine shells. The lagoon formations are darker in colour and of small extent and thickness. The deposits from rivers are either from those rising in the mountains, or from those rising in the pampas themselves. The former contain, near the mountains, blocks of stone rolled down, and the granular nature of the deposit grows ever finer in the course of the rivers, which lose themselves in the pampas, in a region rich in lagoons, with a pretty abundant vegetation under recurrent rains. The deposits from the poor streams rising in the pampas have round, smooth, lime concretions, with smooth bone fragments of mammals. But most extensive are the æolic or air formations, of which the vertical root-like tubes and irregularly-formed lime concretions are characteristic. Violent winds carry the fine water-deposited

material in all directions over the plains till vegetation comes and retains it. The uniform character of the pampas loess arises, according to Roth, not from the material and mode of deposition, but chiefly from its transformation under the influence of vegetation. The roots taking up the matters they need, decompose the soil, and the humus arising from the decay of the plants acts on the new material spread over the surface by wind and rain, along with fresh plants, by way of decomposition. A further metamorphosis occurs by water carrying down matter through the porous layers, with the result of new combinations, and a harder, more compact loess in the lower parts. From observations of marine Tertiary beds of (probably) Miocene age in Entre Rios, over typical pampas loess, Roth infers that the formation of loess began some time in the Eocene period; in diluvial times it grew in intensity, and has gone on till now without interruption.

AN interesting study has been lately made by Herr Tarchenoff (*Pflüger's Archiv*) of electric currents in the skin from mental excitation. Unpolarizable clay-electrodes, connected with a delicate galvanometer, were applied to various parts—hands, fingers, feet, toes, nose, ear, and back; and, after compensation of any currents which occurred during rest, the effects of mental stimulation were noted. Light tickling with a brush causes, after a few seconds' period of latency, a gradually increasing strong deflection. Hot water has a like effect; cold, or the pain from a needle-prick, a less. Sound, light, taste, and smell stimuli act similarly. If the eyes have been closed some time, mere opening of them causes a considerable deflection from the skin of the hand. Different colours here acted unequally. It is remarkable that these skin-currents also arise when the sensations are merely imagined. One vividly imagines, *e.g.*, he is suffering intense heat, and a strong current occurs, which goes down when the idea of cold is substituted. Mental effort produces currents varying with its amount. Thus, multiplication of small figures gives hardly any current; that of large, a strong one. If a person is in tense expectation, the galvanometer mirror makes irregular oscillations. When the electrodes are on hand or arm, a voluntary movement, such as contraction of a toe or convergence of the eyes, gives a strong current. In all the experiments it appeared that, with equal nerve excitation, the strength of the skin-currents depended on the degree to which the part of the skin bearing the electrodes was furnished with sweat-glands. Thus some parts of the back, and upper leg and arm, having few of these, gave hardly any current. Herr Tarchenoff considers that the course of nearly every kind of nerve-activity is accompanied by increased action of the skin-glands. Every nerve-function, it is known, causes a rise of temperature, and accumulation of the products of exchange of material in the body. Increase of sweat-excretion favours cooling, and the getting rid of those products.

A METEORITE of special interest to chemists has been examined by M. Stanislas Meunier. It fell at Mighei, in Russia, on June 9, 1889, and it was evident, from a cursory inspection, that it was of a carbonaceous nature. In external appearance it exhibited a deep greenish-black colour, relieved by numerous small brilliant white crystals; the surface was considerably wrinkled, and blown out into swellings. The material was very friable, and readily soiled the fingers. A section under the microscope was observed to consist largely of opaque matter interspersed with crystals of a magnesian pyroxene and peridote. Fine particles of metallic iron and nickeliferous iron were readily collected by a magnet from the powdered rock, having all the characteristics of meteoric iron. The density of the meteorite was not very high, 2·495. About 85 per cent. of the rock was found to be attacked by acids, the portion so attacked being shown by analysis to consist mainly of a silicate of magnesium and iron having the composition of peridote. On the remaining 15 per cent. being heated in a current of dry oxygen gas, it readily took fire and burnt brilliantly. The products of combustion, which were allowed to pass through the usual absorption tubes containing pumice and sulphuric acid and potash, showed that the meteorite contained nearly 5 per cent of organic matter. In order to obtain some idea as to the nature of the carbonaceous substance present, a quantity of the rock was powdered and then digested with alcohol; on evaporation the alcoholic extract yielded a bright yellow resin, which was readily precipitated from the alcoholic solution by water, and much resembled the kabäite of Wöhler. The most curious chemical properties of the meteorite, however, are exhibited with a cold aqueous extract of the powdered rock. The filtered liquid is quite colourless, but exhales a faint odour due to an organic salt which carbonizes on evaporation to dryness, and may be burnt upon platinum foil. The aqueous extract further contains nearly 2 per cent. of mineral matter possessing properties of a novel character. Barium chloride solution gives a heavy white precipitate, which, however, is not barium sulphate. Silver nitrate gives a voluminous curdy reddish-violet precipitate, reminding one of silver chromate, but of quite a distinct and peculiar tint, and which blackens in a very few minutes in daylight. The substance which exhibits these reactions is unchanged by evaporation to dryness and ignition to redness, readily dissolving in water again on cooling and giving the above reactions. The silver nitrate precipitate, when allowed to stand for some time undisturbed in the liquid, becomes converted into colourless but brilliantly refractive crystals, which polarize brightly between crossed Nicols under the microscope, and which are insoluble in boiling water. The properties of this new substance contained in the water extract appear to approximate most closely to those of certain metallic tellurates, but the new compound appears also to differ in certain respects from those terrestrial salts.

THE additions to the Zoological Society's Gardens during the past week include a Brown Capuchin (*Cebus fatuellus* ♂) from Guiana, presented by J. H. Bostock; a Common Gull (*Larus canus*), a Black-headed Gull (*Larus ridibundus*), British, presented by Mr. E. Keilich; two Schlegel's Doves (*Chalcopelia puella*) from West Africa, presented by Major C. M. MacDonald; a Common Barn Owl (*Strix flammea*), British, presented by Mr. H. Craig; two Swainson's Lorikeets (*Trichoglossus novæ-hollandiæ*) from Australia, deposited.

OUR ASTRONOMICAL COLUMN.

OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at Greenwich at 10 p.m., January 9 = 5h. 17m. 32s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) Nebula in Orion ...	—	Greenish.	5 29 52	− 5 29
(2) 20 Leporis U.A. ...	6	Reddish-yellow.	5 6 14	−11 59
(3) η Orionis	4	Whitish-yellow.	5 19 0	− 2 30
(4) β Tauri	2	White.	5 19 18	+28 31
(5) 99 Birm.	8	Reddish-yellow.	5 4 25	− 5 38
(6) U Canis Minoris ...	Var.	Reddish?	7 35 22	+ 8 38
(7) T Arietis	Var.	Yellow.	2 42 11	+17 3

Remarks.

(1) The bright lines so far recorded in the visible part of the spectrum of the Great Nebula in Orion are as follows:—

Wave-lengths.		Observers.
5872 (D_3)	...	Dr. Copeland.
559	...	Mr. Taylor.
520	...	,,
500	...	Dr. Huggins.
495	...	,,
486 (F)	...	,,
470	...	Mr. Taylor.
447	...	Dr. Copeland.
434 (G)	...	Dr. Huggins.

The principal line in the photographic spectrum is near wave-length 373, and this seems to be special to certain parts of the nebula, according to Dr. Huggins's researches.

Although so much admirable work has already been done, there is still abundant scope for further investigations. One of the chief points requiring attention at present is the character of the brightest line, near λ 500. Researches on the spectra of meteorites, coupled with previous records of the line as having a fringe on its more refrangible side, led Prof. Lockyer to suggest, in 1887, that it was the remnant of the fluting near λ 500 seen in the spectrum of burning magnesium. Observations have since been made by Prof. Lockyer, Mr. Taylor, and myself, and all agree that the line is not sharp on the more refrangible side. Further observations are suggested. High dispersion is not necessary, or indeed desirable.

Direct comparisons of the chief nebula line with the magnesium fluting are also required, but this is an observation of great delicacy, requiring high dispersion. It must also be demonstrated that under the same conditions of comparison the F line of hydrogen is coincident with the third nebula line.

It has been suggested that the line near 559 recorded by Mr. Taylor is the remnant of the brightest manganese fluting; this can only be decided by direct comparisons.

In my own observations I noted that the F line is not seen in all parts of the nebula, and in this respect it resembles the ultra-violet line. This localization of the lines opens up a new field of work.

(2) This is one of the finest examples of stars of Group II. The bands 1 to 9 are perfectly well seen, but there is no record of the presence or absence of line absorptions. Observations of the carbon flutings are suggested, a spirit-lamp flame being convenient for comparisons. The two flutings to be examined, both for position and compound structure, are those near λ 517 and 474. The latter is a group of five flutings, extending from about λ 468 to λ 474, and under some conditions the point of maximum brightness of the group is shifted from 474 to 468. Comparisons of bands 4 and 5 with the brightest flutings of manganese and lead should also be made.

(3) This is a star with a spectrum of the solar type, of which the usual differential observations are required. The relative thicknesses of the hydrogen and other lines should also be noted.

(4) Gothard describes this star as belonging to Group IV. The usual observations are required.

(5) This is a star of Group VI., in which band 9 is dark, and band 6 pale. Dunér does not record any of the secondary bands. These and absorption lines should be looked for.

(6) This variable has a period of 423 days, and ranges from 8·5 at maximum to 13·5 at minimum (Gore). The spectrum has not yet been recorded. Maximum on January 9.

(7) This is a variable with a spectrum of the Group II. type. The period is 324 days, and the magnitude varies from about 8 at maximum to 9·5 at minimum. The maximum will not occur until January 17, but observations for the bright lines of hydrogen, &c., may be commenced at once. Variations of the widths and intensities of the bands before and after maximum may also be looked for.

A. FOWLER.

IDENTITY OF COMET VICO (1844) WITH BROOKS'S (1889).—In a note on some comets of short period (*Bulletin Astronomique*, November 1889), M. L. Schulhof observes that a comparison of the elements of Vico's comet (1844) given by Le Verrier with those of Brooks's comet (1889) shows a striking similarity. According to Mr. Chandler (*Astronomical Journal*, No. 205), Brooks's comet in May 1886 was at a distance 0·064 from Jupiter, and in heliocentric longitude 185°, whilst Vico's comet found itself about 1885–86, according to the elements of M. Brünnow in heliocentric longitude 162°, and approximately 0·4 from Jupiter. M. Schulhof adds, however, that the only objection to the hypothesis is that the action of Jupiter at a distance 0·4 would hardly have been sufficient to change so considerably the perihelion distance and the time of revolution. It will be sufficient to calculate back the perturbations of Brooks's comet as far as 1885 to definitely settle this question.

An investigation of the elements of Comets Lexell and Finlay has led to the conclusion that they are not identical, but the results found are not to be taken as conclusive, a farther and more exact determination of the elements of Finlay's comet having been undertaken.

OBSERVATIONS OF SOME SUSPECTED VARIABLES.—Observations of Lalande 26980 = 14h. 42·7m. + 6° 28′·9 (1875), by Rev. John G. Hagen, of Georgetown College, give the negative result that there is no proof of variation between the years 1884–89, and although an average of 15 observations a year have been made, the extreme range of magnitude is less than 0·2.

Three stars were found that showed rather a large difference from the Bonn D.M. magnitudes, and were watched from 1886 to 1889. No variation, however, was noticed during these three years. The following are the three stars and the magnitudes found compared with Argelander's:—

	°	m. m.	m.
D.M.	55·2587	... 7·8 ± 0·1 ;	D.M. = 8·8.
D.M.	44·3368	... 7·6 ± 0·1 ;	D.M. = 7·0.
D.M.	44·3402	... 7·7 ± 0·0 ;	D.M. = 8·1.

SPECTRUM OF A METALLIC PROMINENCE.—Prof. Vogel in a letter to Prof. Tacchini (*Mem. Società Spettroscopisti Italiani*, November 1889) observes that the positions of the lines measured in a metallic prominence on June 28 were incorrectly given by Prof. Spoerer in the *Memorie* for October (see NATURE, vol. xli. p. 115), and that the following should be substituted:—

Wave-length.	Origin.	Wave-length.	Origin.
667·6 ...	... Fe	553·4 ...	... Ba, Fe, Sr.
C ...	... H.	531·6 ...	... Ceronium.
649·6 ...	... Ba.	526·9 ...	... Ca, Fe.
646·2 ...	... Ca.	518·8 ...	... Ca, Fe.
D_1 ...	... Na.	b_1 ...	... Mg.
D_2 ...	... Na.	b_2 ...	... Mg.
D_3 ...	... Helium.	b_3 ...	... Fe, Ni.
		b_4 ...	... Mg, Fe.

The above table only contains a small number of the bright lines seen in this eruption.

COMET SWIFT (*f* 1889, NOVEMBER 17).—The following corrected elements are given by Dr. Zelbr (*Astr. Nachr.*, 2944):—

T = 1889 November 29·66411 Berlin Mean Time.

$$\left.\begin{aligned} \pi &= 40^\circ\ 55'\ 52''\!\cdot\!8 \\ \Omega &= 331\ 26\ 40\cdot1 \\ \iota &= 19\ \ 3\ 21\cdot1 \end{aligned}\right\} \text{Mean Eq. } 1889\cdot0.$$

$$\begin{aligned} \phi &= 39\ \ 8\ 23\cdot1 \\ \log a &= 0\cdot559784 \\ \log \mu &= 2''\cdot710331 \\ \text{Period} &= 6\cdot91 \text{ years.} \end{aligned}$$

Dr. Lamp has computed the ephemeris given below from these elements:—

1890.	R.A. h. m. s.	Decl. ° ′	1890.	R.A. h. m. s.	Decl. ° ′
Jan. 8 ...	1 19 48 ...	+25 50·9	Jan. 19 ...	1 59 43 ...	+27 46·2
9 ...	23 25 ...	26 2·8	20 ...	2 3 21 ...	27 55·0
10 .	27 2 ...	26 14·4	21 ...	6 59 ...	28 3·5
11 ...	30 39 ...	26 25·7	22 ...	10 36 ...	28 11·8
12 ..	34 17 ...	26 36·7	23 ...	14 14 ...	28 19·8
13 ...	37 54 ...	26 47·5	24 ...	17 51 ...	28 27·4
14 ...	41 32 ...	26 58·0	25 ...	21 28 ...	28 34·8
15 ...	45 10 ...	27 8·2	26 ...	25 4 ...	28 41·9
16 ...	48 48 ...	27 18·1	27 ...	28 40 ...	28 48·7
17 ...	52 27 ...	27 27·7	28 ...	2 32 15 ...	28 55·3
18 ...	1 56 5 ...	27 37·1			

The brightness on Jan. 8 = 0·48 and on Jan. 28 = 0·30, that at discovery being taken as unity.

M. Schulhof notes (*Bulletin Astronomique*, November 1889) that, according to the elements of this comet, it is probably identical with Blanpain's comet (1819), which M. Clausen has shown to be identical with Grischow's comet (1743).

SOLAR SPOTS AND PROMINENCES.—In the November *Memorie della Società degli Spettroscopisti Italiani*, Prof. Tacchini contributes a note on spots and faculæ observed from July to September of this year. A comparison of these observations with those of the preceding quarter shows an augmentation of the phenomena described and a diminution of the frequency of days without spots.

Spectroscopic observations made by Prof. Tacchini during the same period as the above show the mean daily number of prominences to have been 2·93, with an average altitude of

38″·8. This is an increase on the results of the preceding quarter both in the number and height of prominences. Two elaborate plates are included in the *Memorie*, indicating the prominences observed at Rome and Palermo from September to December 1886.

GEOGRAPHICAL NOTES.

The following news was received a few days ago at St. Petersburg from Colonel Roborovski, the present chief of the late M. Prjevalsky's projected expedition. They crossed the Tian-Shan by the Barskaun and Bedel Passes, and reached the Taushkan-daria. Then they crossed the Kara-teke chain, and when they were on the banks of the Yarkend river, they found out that the Kashgar-daria no longer reaches the Yarkend-daria, but is lost in the irrigation canals of Maral-bash. They followed the Yarkend river, which rolls a mass of muddy water between quite flat banks, covered for some 15 to 30 miles on both sides of the river, by thickets of *Populus euphratica*, *Populus prunosa*, tamarisks, *Halostachus* shrubs, and rushes. Sand deserts spread on both sides,—towards the west to Kashgar, and eastwards to Lob-nor. Many ruins of old cities are met with in the deserts which are never visited by the natives. In the thickets of shrubs which fringe them there are numbers of tigers and wild boars, while amidst the *barkhans* of the deserts the wild camels are freely grazing. From Yarkend, the expedition went south, towards the hilly tracts, where it stayed for a month, and then it moved towards Kotan, whence Colonel Roborovski wrote on October 7. He proposed to winter at Niya, and to search for a pass to Tibet across the border-ridge to which Prjevalsky gave the name of "Russian Ridge." If they succeed they will spend next summer in Tibet.

In a lecture lately delivered before the Geographical Society of Bremen, Prof. Kuekenthal, of Jena, gave some account of his researches in King Charles Land. Geologically, these islands belong to Spitzbergen, and not, as was formerly supposed, to Francis Joseph Land. During his stay of nearly three months, Prof. Kuekenthal thoroughly investigated this remote district, which is almost unapproachable, the surrounding seas being densely packed with icebergs. The islands are almost entirely without vegetation; only a few mosses struggle for existence on the clay soil. Numerous walrus skeletons are thrown up by the sea. Game is plentiful; Prof. Kuekenthal shot 14 bears (besides bringing back two live specimens), 39 walruses, and as many seals. Many insects and crustaceans were obtained from the land lakes.

THE ANNIVERSARY OF THE ROYAL SOCIETY.

The President, after giving an account of the scientific work of many Fellows deceased during the past year, addressed the Society as follows:—

On account of the great importance of Joule's labours, both directly, in the advancement of science, and indirectly, through the knowledge thus acquired, in enabling improvements to be made in the practical application of science for industrial purposes, it has been suggested that it might be desirable to raise some public memorial to him, and the Council has appointed a Committee to consider the question.

I have referred, and that very briefly, to some only of the Fellows whom we have lost during the past year, but fuller details both of them, of other Fellows whom we have lost, and of our recently deceased Foreign Members, will be found in the obituary notices which appear from time to time in the Proceedings, according as they are received from the Fellows who have kindly undertaken to draw them up.

Of those who last year were on our list of Foreign Members, we have since lost one who was truly a veteran in science. More than three years have elapsed since the celebration of the centenary of the birth of M. Chevreul, and two more recurrences of his birthday came round before he was called away. He will be known for his researches on the contrast of colours. But his great work was that by which he cleared up the constitution of the fixed oils and fats, and established the theory of saponification. Few scientific men still surviving were even born when this important research was commenced—a research in the course of which he laid the foundation of the method now universally followed in the study of organic compounds, by showing that an ultimate analysis by itself alone is quite insufficient, and that it is necessary to study the substances obtained by the action of reagents on that primarily presented for investigation.

There is one whose name, though he was not a Fellow, I cannot pass by in silence on the present occasion. I refer to Thomas Jodrell Phillips Jodrell, who died early in September, in his eighty-second year. About the time of the publication of the reports of the Duke of Devonshire's Commission, the subject of the endowment of research was much talked of, and Mr. Jodrell placed the sum of £6000 in the hands of the Society for the purpose of making an experiment to see how far the progress of science might be promoted by enabling persons to engage in research who might not otherwise be in a condition to do so. But before any scheme for the purpose was matured, the Government Grant for the promotion of scientific research was started, under the administration of Lord John Russell, then Prime Minister. This rendered it superfluous to carry out Mr. Jodrell's original intention, but he still left the money in the hands of the Society, directing that, subject to any appropriation of the money that he might make, with the approval of the Royal Society, during his lifetime, the capital should, immediately upon his death, be incorporated with the Donation Fund, and that in the meantime the income thereof should be received by the Royal Society. Of the capital, £1000 was several years ago assigned to a fund for the reduction of the annual payments to be made by future Fellows, and the remaining £5000 has now, of course, been added to the Wollaston Donation Fund. By the Fee Reduction Fund the annual payment of ordinary Fellows elected subsequently to the time of the change was made £3 instead of £4, and the entrance fee abolished. As to the Donation Fund, a very wide discretion was, by the terms of the original foundation, left in the hands of the Council as to the way in which they should employ it in the interest of science.

Since the Croonian Foundation for lectures was put on its present footing, it has been made the means of securing for us the advantage of a lecture delivered before the Society by distinguished foreign men of science. In the present year our Foreign Member, M. Pasteur, was invited to deliver the lecture. Unfortunately, the state of his health would not allow him to deliver it himself, but at one time he hoped that he would have been able to be present at its delivery. It was ultimately arranged that his fellow-labourer at the Pasteur Institute, Dr. Roux, should deliver the Croonian Lecture in his stead; and several of the Fellows have heard his lucid account, first of the discoveries of M. Pasteur in relation to diseases brought about by microscopic organisms, and then further researches of his own in the same field.

In addressing the Fellows at the anniversary last year, I mentioned that Commandant Desforges had kindly offered to compare that portion of Sir George Schuckburgh's scale, with reference to which the length of the seconds pendulum had been determined by Kater and Sabine, with the French standard metre; and as the ratio of this to the English standard yard was accurately known, the length of the pendulum, as determined by these accurate observers, would thus for the first time be brought into relation with the English yard by direct comparison with accurately compared measures of length. The comparison was shortly afterwards executed, and the scale, which, of course, was very carefully packed for its journey to Paris and back, has long since been replaced in the apartments of the Society. This highly desirable comparison occupied but a few days in its execution; which affords one example of the scientific advantages derivable under an international agreement, from the establishment of the Bureau des Poids et Mesures. Our own country, which for some years held aloof from the Convention, forming the sole exception to the general agreement among nations of importance, joined it some years ago; and we thus have the privilege of availing ourselves, as occasion may arise, of the appliances at the office in Paris for such comparisons of measures of length or weight.

The services of Mr. Arthur Soper, as a special assistant, have been retained during the past session, with advantage to the library. He has completed the much-needed shelf catalogue, and the re-arrangement of the books where necessary. In the course of this work the volumes of a purely literary character

have been collected together, and a selection of the most valuable have been preserved in a properly protected case. Of the remainder, about 150 volumes (in addition to those reported last year) have been presented to various public libraries, and a slip catalogue of the volumes which are retained, containing about 1700 entries, has been prepared.

The manuscripts (other than the originals of ordinary papers read at the meetings) which have accrued to the Society since the publication of Halliwell's Catalogue have been collected from various parts of the building into the Archives Room, with the object of preparing a complete catalogue of the manuscripts at present in the possession of the Society.

Since the last anniversary, twenty-four memoirs have been published in the Philosophical Transactions, containing a total of 753 pages and 33 plates. Of the Proceedings, twelve numbers have been issued, containing 1062 pages and 6 plates. Dr. R. von Lendenfeld's "Monograph of the Horny Sponges," mentioned in my last anniversary address, has also been issued during the year in a quarto volume of 940 pages of text and 51 plates.

The Fellows are aware that for a great many years the Royal Society has devoted a part of its funds to the collection, preparation for the press, and correction of the proofs of a Catalogue of Scientific Papers. We have endeavoured to make the work as complete as possible, and to include scientific serials in all languages. The first part, covering the period 1800-63, is printed in six thick quarto volumes, of which the last appeared in 1872. The decade 1864-73 occupies two more volumes, of which the second was published in 1879. This work, in the preparation of which the Royal Society has spent a large sum, is for the benefit of the whole civilized world, and the sale of it could not be expected nearly to cover the cost of printing, paper, and binding. On a representation to this effect being made to Government, when the first part was ready for the press, the Lords of the Treasury consented that it should be printed at the public expense, the proceeds of the sale of the work, after reserving a certain number of copies for presentation, being repaid to the Treasury. In consideration of the large outlay involved in the preparation, those Fellows of the Society who wished to purchase the work could do so at about two-thirds of the cost to the general public. A similar application to the Treasury with reference to the decade 1864-73 met with a similar response, and we proceeded, as I mentioned at the anniversary last year, with the preparation of the manuscript for the next decade, 1874-83, which was then nearly ready. On making application towards the end of last year to the Treasury for the printing of this decade, our request was not acceded to. While declining, however, to continue any further the printing of this great work, the sum of £1000 was put in the Estimates, and has since been voted by Parliament, to assist us in the publication, and the copies of the work still remaining unsold have been handed over to us. This has enabled us to conclude negotiations with Messrs. Clay and the Syndics of the Cambridge University Press for the printing of the decade last mentioned, and at the same time to make some provision towards the future continuation of the work, without, as it may be hoped, encroaching to a greater extent than hitherto on our own resources.

The utility of the work would obviously be much increased if it could be furnished with some sort of key enabling persons to find what had been written on particular subjects. I am not without hopes that this very desirable object may yet be accomplished, notwithstanding the magnitude of any such undertaking.

Within the last year the Council of the Royal Society has accepted a duty in connection with scientific agriculture, of which it will be interesting to the Fellows to be informed. It is well known that for the last fifty years, or thereabouts, Sir John Lawes has carried out on his estate at Rothamsted an elaborate and most persevering series of experiments on the conditions which influence the growth and yield of crops of various kinds, the effect of manures of different kinds, the result of taking the same crop, year after year, from off the same land without supplying to it any manure, &c. Long as these experiments have already been continued, there are questions, particularly as regards the capabilities of the sub-soil, which require for their satisfactory answers that similar experiments should be continued on the same land for a still longer period. In respect of such questions, the investigator of the science of agriculture is in a position resembling that in which the astronomer is often placed, in having to make observations, the full interest of which it must be left to posterity to enjoy.

To prevent the interruption of these experiments, which it would take a life-time to repeat on fresh ground, and at the same time to provide for the carrying out of researches generally bearing on the science of agriculture, Sir John Lawes has created a trust, securing to the trustees a capital sum of £100,000, and leasing to them for ninety-nine years, at a peppercorn rent, certain lands in his demesne on which the experiments have hitherto been carried on, together with his laboratory. The trust is intended to be for original research, not for the instruction of students. The general direction of the experiments and researches to be carried on is vested in a committee of management consisting of nine persons, of whom four are to be appointed by the President and Council of the Royal Society.

The trustees named in the deed were Sir John Lubbock, Dr. Wells, and our Treasurer, Dr. Evans. One of these is now no more. Lord Walsingham has been appointed a trustee in place of the late Dr. Wells.

The Copley Medal for the year has been awarded to Dr. Salmon for his various papers on subjects of pure mathematics, and for the valuable mathematical treatises of which he is the author. Dr. Salmon's published papers are all valuable. Among others may be mentioned his researches on the classification of curves of double curvature, and on the condition for equal roots of an equation; the very important theorem of the constant anharmonic ratio of the four tangents of a cubic curve; his researches on the theory of reciprocal surfaces; his paper on quaternary cubics. But any notice of his contributions to the advancement of pure mathematics would be incomplete which did not specially mention his invaluable text-books on conic sections, higher plane curves, solid geometry, and the modern algebra—works which not only give a comprehensive view of the subjects to which they relate, but contain a great deal of original matter.

Of the Royal Medals, it is the usual though not invariable practice to award one for mathematics or physics, including chemistry, and one for some one or more of the biological sciences. No distinction is, however, made between the two medals in point of order of precedence, and I will, therefore, take the names of the medallists in alphabetical order.

The Council have awarded one of the Royal Medals this year to Dr. Walter Holbrook Gaskell for his researches in cardiac physiology, and his important discoveries in the anatomy and physiology of the sympathetic nervous system.

In his memoir, "On the Rhythm of the Heart of the Frog" (Croonian Lecture, Phil. Trans., 1882), and in a subsequent memoir, "On the Innervation of the Heart of the Tortoise" (*Journ. of Physiol.*, vol. iv.), Dr. Gaskell very largely advanced our knowledge of the physiology of the heart-beat, more especially as relates to the sequence of the beats of the several parts, the nature of the inhibitory action of the vagus nerve, and the relations of tonicity and conducting power to rhythmical contraction. These memoirs, however, lacked completeness on account of their not taking into full consideration the action of the cardiac augmentor or accelerator fibres, the existence of which had been previously indicated in the case of mammals, and suspected in the case of the frog and allied animals.

By a striking experiment (*Journ. of Physiol.*, vol. v.) Dr. Gaskell subsequently gave the first clear demonstration of the presence in the frog of cardiac augmentor fibres; also he gave a clear account of the nature of the action of their fibres, and the relations of that action to the action of the vagus fibres. Revising his previous work by the help of the light thus gained, Dr. Gaskell was enabled to give the first really consistent and satisfactory account of the nature of the heart-beat, of the modifications of beat due to extrinsic nerves, and of the parts played by muscular and nervous elements respectively.

Important as was this work on the heart, Dr. Gaskell's subsequent work "On the Structure, Functions, and Distribution of the Nerves which govern the Vascular and Visceral Systems" (*Journ. of Physiol.*," vol. vii.) has a far higher importance and significance. In spite of the knowledge which during the past thirty or forty years has been gained concerning vaso-motor nerves and the nerves governing the movements of the viscera, physiologists had up to the time of the appearance of Dr. Gaskell's memoir failed to obtain a clear conception of the nature and relations of the so-called sympathetic nervous system. By his researches, in which the several methods of

gross anatomical investigation, minute histological examination, and experimental inquiry were, in a striking manner, made to assist each other, Dr. Gaskell, by tracing out the course and determining the nature of vaso-constrictor and vaso-dilator fibres, and comparing them with the cardiac augmentor and inhibitory fibres, and with the fibres governing the visceral muscles, has already reduced to order what previously was to a large extent confusion, and has opened up what promises to be the way to a complete understanding of the whole subject.

The results arrived at, besides their great physiological importance, on the one hand promise to be of great assistance in practical medicine, and on the other are eminently suggestive from a purely morphological point of view.

The other Royal Medal has been awarded to Prof. Thomas Edward Thorpe for his researches on fluorine compounds, and his determination of the atomic weights of titanium and gold.

Prof. Thomas Edward Thorpe's experimental work has secured for him a place in the first rank of living experimentalists.

His researches, which are not confined to one department of chemical science, but extend over many branches, are all distinguished both by accuracy and originality of treatment. As examples of the high character of his investigations, those of the determinations of the atomic weights of titanium and gold may be specially cited as permanently settling the value of two most important chemical constants; whilst his researches on the fluorine compounds, including the discovery of thiophosphoryl fluoride, a body capable of existing undecomposed in the state of gas, and his latest work on the vapour-density of hydrofluoric acid, do not fall short of the highest examples of classical chemical investigation.

The Davy Medal has been awarded to Dr. W. H. Perkin for his researches on magnetic rotation in relation to chemical constitution.

Dr. Perkin is well known as the originator of what is now a great industry, that of the coal-tar colours, by his preparation and application to tinctorial purposes of a colouring matter which had previously merely been noticed as affording a chemical test for the presence of aniline. This, however, is now a long time ago, and it is for more recent work, the interest of which is purely scientific, that the medal has been awarded to him.

Dr. Perkin first showed, in 1884, that a definite relationship exists between the chemical constitution of substances and their power of rotating the plane of polarization of light when under magnetic influence; and he pointed out how the "molecular coefficient of magnetic rotation" or "molecular rotatory power" might be deduced.

In 1884 he presented to the Chemical Society a lengthy paper describing his method, and the results obtained for a very large number of paraffinoid hydrocarbons and haloid and oxygenated derivatives thereof; from these he deduced "constants," which he has since shown to be applicable in calculating the magnetic rotatory power of paraffinoid compounds generally. From time to time he has published further instalments of his work, and only quite recently has described the results obtained on examining nitrogen compounds, which exhibit many most interesting peculiarities.

The results are of special value on account of the exceptional care devoted to the preparation of pure substances, and the guarantee, which Dr. Perkin's reputation affords, that everything possible has been done to secure accuracy; and also because the substances chosen are for the most part typical substances, or belong to series in which a simple relationship exists.

HAIL-STORMS IN NORTHERN INDIA.

IN a paper recently published in the Journal of the Asiatic Society of Bengal, Mr. S. A. Hill describes certain severe hail-storms and tornadoes that occurred on April 30 and May 1, 1888, in the Gangetic *doab* and Rohilkand in Northern India.[1] Tornadoes are not very common in India, but they appear to have been somewhat more prevalent than usual in the spring of 1888, the storms in question having been preceded on April 7 by a very destructive tornado at Dacca in Bengal, a full description of which was given by Mr. Pedler and Dr. Crombie in a previous number of the Society's Journal. Like all previously recorded storms of this character, these occurred in the spring, when the seat of minimum pressure is established in the Lower Punjab, and a trough of low pressure extends from this region eastward to the Gangetic plain. To the south of this trough very dry west winds, the hot winds of Northern India, prevailed in Rajputana and Central India, while, to the north of it, damp easterly winds blew up the northern margin of the plain and across the outer slopes of the Himalaya. It is apparently in the meeting of these two winds, where the former blows in an upper, the latter in the lower, stratum, that are generated the thunder squalls that form a normal feature of the spring months in Northern India; and tornadoes, as Prof. Ferrel has shown, are merely an exaggerated development of the thunder squall. In the present instance, ordinary storms of this character, and dust storms, occurred pretty generally over all the north-western districts of the North-West Provinces, simultaneously with the tornadoes in Rohilkand and the Gangetic *doab*.

From the evidence quoted by Mr. Hill, it does not appear indeed to be positively established that any of the storms described exhibited all the characteristic features of tornadoes, as was undoubtedly the case of the Dacca storm. No mention is made in any of the reports of any whirling column having been actually observed; and that whirlwinds were the real agents of destruction seems to be inferred chiefly from the destructive force of the wind, especially its lifting power, and some rather vague reports on the wind's changes during the passage of the storm. On a point of this kind, however, in India, negative evidence goes for little, and the chief subject discussed in Mr. Hill's paper, viz. the conditions which determine these atmospheric disturbances, is of equal interest, whether they were really tornadoes or only remarkably severe hail-storms of the more usual kind.

In the barometric changes of the days preceding the storms there does not appear to be anything that throws much light on their genesis. The relative distribution of pressure shown by the observations on the Indo-Gangetic plain underwent but little variation, and the existence of a slight secondary depression in the immediate neighbourhood of the storm tract, on April 30, is inferred solely on the evidence of two Himalayan stations at elevations of 5300 feet and 6000 feet above the sea, and may be delusive. There had, however, been a general steady fall of the barometer for three days before the storms of April 30—one of those oscillations, apparently, which Mr. Abercromby has termed surges, and a rapid rise set in after the storms. As has been pointed out elsewhere, this is an ordinary recurrent feature of the season.

It is in the changes in the vertical distribution of temperature that Mr. Hill finds the conditions that determined the atmospheric disturbance. Taking as his fundamental data the observed temperatures of the three stations, Roorkee at 886 feet, Dehra at 2233 feet, and Mussooree at 6881 feet, and assuming that these represent approximately the rate of vertical decrease over the neighbouring plain, he computes the fall of temperature for increments of 1000 feet up to 10,000 feet by means of a simple formula of interpolation, and finds that, up to the forenoon of April 30, the condition of unstable equilibrium which results from the diurnal heating of the plains did not extend beyond 3000 or 4000 feet above the ground surface. This would set up a considerable amount of convective interchange between these lower strata, but the cloud-forming strata would still be in a stable condition, at least in a non-saturated atmosphere. On the afternoon of April 30, the conditions were changed. With a great fall of temperature at the lowest and highest stations, as compared with the previous day at the same hour, that of the intermediate station was but little affected, and hence the computed table shows a reduction of the vertical decrement at low levels, a corresponding increase at the higher levels, and a transfer of the condition of unstable equilibrium from the former to the latter. Simultaneously with this change took place that violent disturbance of the atmosphere that resulted in the hail-storms on the plains.

Mr. Hill's conclusions are entirely in accord with what might be expected on *a priori* grounds. But before they can be fully accepted, it is necessary to scrutinize the data, and as the result of this scrutiny we must confess they do not seem to us completely convincing. We may put aside the question whether and to what extent the empirical formula of interpolation adopted by Mr. Hill really expresses the law of decrement of temperature, since, although it would evidently fail for extrapolation much beyond the altitude of 7000 feet, it probably does not involve any very serious error below that limit, provided the numerical values afforded by observation are trustworthy. The

[1] *Op. cit.*, vol. lviii., Part 2, No. 2, 1889.

critical point of the whole reasoning is whether the observed temperatures at the three stations Roorkee, Dehra, and Mussooree, can be safely accepted as approximately representing those of the free atmosphere over the plains at the same levels, and this seems to us at least open to question. In the case of the lowest and highest stations, indeed, there is not much to object to. Roorkee is a fairly representative station of the northern part of the Gangetic plain, and the Mussooree Observatory, situated on the very crest of the ridge of the Himalaya, overlooking the plains, is probably as little affected by the local heating of the ground as any mountain observatory can be. But Dehra, which furnishes the really critical datum of Mr. Hill's reasoning, is on the plain of the *Dún*, a flat valley six or eight miles across, stretching between the Sivaliks and the foot of the Mussooree ridge, and it is by no means self-evident that the local temperature is not largely affected by causes which are quite inoperative in the free atmosphere at the same elevation over the plains.

In our opinion, then, Mr. Hill's conclusion that the storms of April 30 and May 1 were determined by a change in the vertical distribution of temperature, transferring the condition of instability from the lower to the higher atmospheric strata, is at least open to doubt. To a certain extent, indeed, it is supported by the evidence of other observatories in the North-West Himalaya, especially Chakrata, the situation of which is very similar to that of Mussooree; but the difference of their elevation (170 feet) is too small to allow of its having much weight in determining the point at issue.

The most noteworthy feature of the storm of April 30 was the fearful loss of life caused by it at Moradabad. Not less than 230 deaths were reported at this station alone, the vast majority of which were caused directly by the hail. The collector's report states that men caught in the open and without shelter were simply pounded to death. The spring is especially the season of native weddings, and "more than one marriage party was caught by the storm near the banks of the river and was annihilated." It is, however, suggested by Mr. Hill that many of the deaths may have been caused by cold. "Immediately before the storm the temperature had been very high, and many if not the majority of the deaths due to it may have been occasioned by the persons exposed to its fury being knocked down and temporarily packed in ice." At Moradabad the hailstones are stated to have been the size of plums—probably the *bér* plum, *Zizyphus jujuba*, the cultivated form of which is two or three times as large as a walnut.

In the storm of May 1, the hailstones at some places were larger, though the destruction was less. At Ghaziabad they are said to have been as large as cricket balls, and one was picked up at Delhi weighing 4½ ounces. At Tilhar they are reported to have been larger than goose eggs, and at a neighbouring place they averaged 3 inches in diameter. Their form is described as a flat oval.

SCIENTIFIC SERIALS.

Bulletin de l'Académie Royale de Belgique, November 1889.—On the existence of a gizzard, and on its structure, in the family of the Scolopendridæ, by M. Victor Willem. The presence of a gizzard in the lower organisms was first determined by M. F. Plateau in 1876. But the gizzard of insects was long supposed to be merely a triturating organ acting mechanically, without any physiological function. Continuing Plateau's researches, M. Willem now finds that the gizzard is not only present in several genera of the Scolopendrid family, but that it is a true digester. The structure is fully described, and illustrated by two plates, on which are figured the gizzards of *Scolopendra hispanica*, *S. cingulata*, *S. heros*, *Scolopocryptops*, *Cryptops punctatus*, and *C. hortensis*. In these last, the apparatus is most highly developed, being even more complicated than amongst the higher order of insects. No explanation is offered of this apparent anomaly.—Unexpected proof of diurnal nutation, and necessity of taking it into consideration in the reduction of observations, by M. F. Folie. The coefficient of diurnal nutation as already approximately determined at 0·01s. by the author's numerous researches, has recently been confirmed in a somewhat remarkable manner by the results of M. Kobold's observations of the Polar star with the meridian circle of Strassburg. The azimuthal errors of this instrument were found to present, not an annual, but a diurnal period, and Kobold's corrections are shown to be illusory, being due to his neglect of the element of diurnal nutation in the reduction of his observations. When this element is taken into account, the results harmonize with those previously arrived at by M. Folie.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, Dec. 19, 1889.—"On the Effects of Pressure on the Magnetization of Cobalt." By C. Chree, M.A., Fellow of King's College, Cambridge. Communicated by Prof. J. J. Thomson, F.R.S.

It has long been known, from the classic researches of Dr. Joule, that a rod of iron free from stress increases in length when magnetized in a comparatively weak field. When, however, the strength of the field is continually raised, it has been found by Mr. Shelford Bidwell that the rod ceases to increase in length, and then shortens, so that in a sufficiently strong field the length becomes less than it was originally. It has also been found by Villari, Sir W. Thomson, and others, that when a rod of iron is exposed to successive loadings and unloadings of a given weight in a magnetic field, there appears a corresponding cyclic change of magnetization. In this cyclic change the maximum magnetization occurs when the load is "on," or when the load is "off," according as the field is weaker or stronger than a certain critical field depending on the load, called by Sir W. Thomson the Villari critical field.

Cobalt has been found by Mr. Shelford Bidwell to shorten when magnetized in weak fields, but to lengthen in very strong fields. The field in which it ceases to shorten is very much higher than the field in which iron ceases to lengthen. Also in weak fields Sir W. Thomson has found the magnetization of a cobalt rod under cyclic applications of tension to be least when the tension is "on."

Now, Prof. J. J. Thomson has shown that on dynamical principles the effect of changes of magnetization on the length of a rod of magnetic metal, and the effect of changes in the length of the rod on the magnetization, must be fundamentally connected. In his "Applications of Dynamics to Physics and Chemistry," he has arrived at mathematical equations connecting the two phenomena, such that from a knowledge of the one set of phenomena the character of the other set can be deduced.

The conclusions derived from the theory are in the case of iron in accordance with the results of experiment, at least in their general character. In cobalt there is also an agreement between theory and experiment, so far as Sir W. Thomson's experiments go. In the absence of further experiments it would, however, be impossible to tell whether or not this agreement extended to the strong fields in which occurred the important phenomena observed by Mr. Shelford Bidwell. The application of Prof. J. J. Thomson's formulæ to Mr. Shelford Bidwell's results led him to the conclusion that under cyclic applications of pressure a cobalt rod should experience cyclic change of magnetization, and that the maximum magnetization should answer to pressure "on," or to pressure "off," according as the magnetic field was weaker or stronger than a critical field, corresponding to the Villari field in iron. It was for the purpose of determining whether such a critical field did actually exist that the present investigation was commenced at Prof. J. J. Thomson's suggestion.

Employing the magnetometric method, it was found that the agreement between theory and experiment was at least as satisfactory in cobalt as in iron. The application of pressure-cycles in a magnetic field led to a cyclic change of magnetization in a cobalt rod, in which the maximum magnetization occurred when pressure was "on," or when it was "off," according as the strength of the field was below or above 120 C.G.S. units. This accordingly was the Villari critical field foreshadowed by theory.

In weak fields the first pressure applied after the introduction of the cobalt rod into the magnetizing coil caused a large increase in the induced magnetization. As the strength of the field was raised, this change in the magnetization attained a maximum, then, diminishing, vanished in a field considerably stronger than the Villari field for the cyclic effect, and in all stronger fields consisted in a diminution of magnetization.

Both Villari and Prof. Ewing observed that, after the break of the magnetizing current, cyclic changes of tension produced

eventually, in iron wires, cyclic changes of the residual magnetization. In these, the maximum magnetization answered, as in the induced magnetization in fields below the Villari point, to tension "on."

In the present investigation, the existence of a cyclic change in the residual magnetization of cobalt accompanying cyclic changes of pressure has been established, and the magnitude of the effect examined in a large number of fields, extending from 0 to 400 C.G.S. units. It was found that not only the magnitude, but the sign even, of the effect depended largely on the condition of the rod during the break of the current. When the rod was under pressure during the break, the residual magnetization in the cyclic state showed a maximum under pressure, whatever was the strength of the pre-existing field. When, however, the rod was free from pressure during the break of the current, it was only in the residual magnetization left after the weakest fields that the maximum answered to pressure "on." When the strength of the pre-existing field was raised, the effect passed through the value zero and changed sign.

"On the Extension and Flexure of Cylindrical and Spherical Thin Elastic Shells." By A. B. Basset, M.A., F.R.S.

The method which I have employed in dealing with problems relating to the equilibrium and motion of thin cylindrical and spherical elastic shells, is as follows:—

Taking the case of a cylindrical shell, let OADB be a small curvilinear rectangle described on the middle surface, of which the sides OA, BD are generators, and the sides AD, OB are circular sections. The resultant stresses per unit of length across the section AD are completely specified by the following five quantities, viz. (1) a tension, T_1; (2) a tangential shearing stress, M_2; (3) a normal shearing stress, N_2; (4) a flexural couple, G_2; (5) a torsional couple, H_1; and the stresses across BD may be derived by interchanging the suffixes 1 and 2. If, therefore, we resolve all the forces which act upon the element along OA, OB and the normal, and take moments about these lines, we shall obtain the six equations of motion in terms of these stresses.

The expression for the potential energy is next found, which differs from that obtained by Mr. Love (Phil. Trans., 1888), owing to the fact that he has omitted to take into account several terms involving the product of the extensions and the cube of the thickness.

The variational equation can now be written down, and if it be applied to a curvilinear rectangle bounded by two lines of curvature and worked out in the usual way, the line integral part will determine the values of the edge stresses T_1, T_2, ... in terms of the displacements; and the surface integral part will determine the three equations of motion in terms of the displacements. These results furnish a test of the accuracy of the work, and also of the fundamental hypothesis upon which the theory is based (viz. *that if the surfaces of the shell are not subjected to any surface pressures or tangential stresses, the three stresses, R, S, T, are of the order of the square of the thickness*); for if we substitute the values of the edge stresses in the last three of our original equations, they ought to reduce to identities; whilst if we substitute these values in the first three, we ought to reproduce the equations of motion which we have obtained by means of the variational equation; and this is found to be the case.

The boundary conditions are obtained by Stokes's theorem, which enables us to prove that it is possible to apply a certain distribution of stress to the edge of a thin shell, without producing any alteration in the potential energy due to strain.

Geological Society, December 18, 1889.—W. T. Blanford, F.R.S., President, in the chair.—The following communications were read:—On the occurrence of the genus *Girvanella*, and remarks on Oolitic structure, by E. Wethered. The author referred to his previous work, wherein he had shown that *Girvanella* is not confined to Silurian rocks, and that as a rock-forming organism it is more important than was supposed, occurring in the Gloucestershire Pea-grit, and also in the Coralline Oolite of Weymouth. He now dealt more in detail with its occurrence (1) in the *Carboniferous Oolitic Limestone*; and (2) in the *Jurassic Oolites*. In the Carboniferous limestone of the Avon valley, Oolitic limestone occurs on four horizons, in three of which the Oolites rest on dolomite. In none of these three cases are there signs of *Girvanella*. From beds partly Oolitic, and not resting on dolomite, he has been able to determine two new species. The Oolite not associated with dolomite is less crystalline, and the original structure is better preserved. In referring to *G. pisolitica*, he discussed whether *Girvanella* is most allied to the *Challenger* Foraminifer, *Hyperammina vagans*, or to *Syringammina fragilissima*. Traces of the organism occur in the *Clypeus*-grit, but none are quoted from beds of the Great Oolite, nor from the Portland Oolite. The author had already shown that the pisolites in the Coralline Oolite of Weymouth were not concretions, but forms of *Girvanella*. Excluding these, he showed that the spherules are of four types, of which one is the ordinary Oolitic granule, while each of the others suggests the presence of *Girvanella*. The characters of the genus, as seen under the microscope were indicated, and four new species were described. The President remarked on the importance of investigating the question whether these appearances are organic or not. We should take warning from *Eozoon* as to possible differences of opinion in the interpretation of tubular structure, though these mystifying appearances seem more common in serpentine and chalcedony than in calcite. In the bodies depicted, the wall, the irregularity, and the manner in which the tubes curve round each other are in favour of their being organic. Prof. Rupert Jones thought that these forms were not due to mineral but to organic laws. Dr. Evans, while disclaiming any special knowledge of the subject, suggested that the appearances might be interpreted on the supposition of an organism boring into a comparatively hard substance. Dr. Hinde, who had seen most of the known species of *Girvanella*, spoke of the wide distribution of these organisms. Remarks were also offered by Dr. Hicks, Prof. Bonney, Prof. Judd, the Rev. H. H. Winwood, and the author.—On the relation of the Westleton Beds or "Pebbly Sands" of Suffolk to those of Norfolk, and on their extension inland, with some observations on the period of the final elevation and denudation of the Weald and of the Thames Valley, Part 2, by Prof. Joseph Prestwich, F.R.S.—The author having, in the first part of this paper (Proc. Geol. Soc., June 5, 1889), discussed the relationship of the Westleton Beds to the Crag series and to the Glacial deposits, proceeded in the present contribution to consider the extension of the Westleton Beds beyond the area of the Crag, and described their range inland through Suffolk, East, West, and South Essex, Middlesex, North and South Hertfordshire, South Buckinghamshire, and North and South Berkshire, noticing their relationship to the overlying Glacial beds, where these were developed, and the manner in which they reposed upon older deposits. He gave an account of the heights of the various exposures above Ordnance Datum, and mentioned the relative proportion of the different constituents in various sections, thus showing that in their southerly and westerly extension they differed both in composition and in mode of distribution from the Glacial deposits. Distinction was also made between the Westleton Beds and the Brentwood Beds. Attention was next directed to the occurrence of the Westleton series, south of the Thames, in Kent, Surrey, and Hampshire, and their possible extension into Somersetshire was inferred from the character of the deposits on Kingsdown and near Clevedon. In tracing the deposits from the east coast to the Berkshire Downs, the author noticed that at the former place the beds lay at sea-level, but ranging inland, they gradually rose to heights of from 500 to 600 feet; that in the first instance they underlay all the Glacial deposits, and in the second they rose high above them, and their seeming subordination to the Glacial series altogether disappeared; thus at Braintree, where the Westleton Beds were largely developed, they stood up through the Boulder-clay and gravel which wrapped round their base, whilst further west, where they became diminished to mere shingle-beds, they attained heights of from 350 to 400 feet, capping London-clay hills, where the Boulder-clay lay from 80 to 100 feet lower down the slopes, the difference of level between the two deposits becoming still greater in a westerly direction, until finally the Boulder-clay disappeared. The origin of the component pebbles of the beds was discussed, and their derivation traced (1) to the beds of Woolwich age in Kent, North France and Belgium, and possibly to some Diestian beds, (2) to the older rocks of the Ardennes, (3) to the Chalk and older drifts, and (4) to the Lower Greensand of Kent and Surrey, or in part to the Southern drift. The marine nature of the beds was inferred from the included fossils of the type-area, and the absence of these elsewhere accounted for by decalcification. The southward extension of the beds was shown to be limited by the anticlinal of the Ardennes and the Weald, and the scanty palæontological evidence of the nature of that land was noted, and the possible existence of the Scandinavian ice-sheet to the north was referred to in connection with the disappearance of the

beds in that direction. From the uniform character of the Westleton shingles, the author maintained that they must originally have been formed on a comparatively level sea-floor, and that the inequalities in distribution had been produced by subsequent differential movement to the extent of 500 feet or more to the north and west above that experienced to the east and south, where the chronological succession remained unbroken, also that the inequalities below the level of the Westleton beds had been produced since the period of their deposition, as, for instance, the gorge of the Thames at Pangbourne and Goring, and most of the Preglacial valleys in the district; furthermore, evidence was adduced in favour of the formation of the escarpments of the Chalk and Oolites since Westleton times, whilst certain observations supplied data for estimation of the relative amounts of pre- and post-glacial denudation of the valleys. It was stated, in conclusion, that the time for the vast amount of denudation was so limited that it was not easy to realize that such limits could suffice, but the author did not see how the conclusions which he had arrived at could well be avoided. After the reading of this paper there was a discussion, in which the President, Mr. Topley, Prof. Hughes, and others took part.

Linnean Society, December 19, 1889.—Mr. J. G. Baker, F.R.S., Vice-President, in the chair.—Prof. P. M. Duncan made some supplementary remarks on a specimen of *Hyalonema Sieboldii*, which he had exhibited at a previous meeting.—Mr. W. Hatchett Jackson exhibited and gave an account of an electric centipede (*Geophilus electricus*), detailing the circumstances under which he had found it at Oxford, and the results of experiments which he had made with a view of determining the nature and properties of a luminous fluid secreted by it. This, he found, could be separated from the insect, and could be communicated by it to every portion of its integument. An interesting discussion followed, in which Mr. Briese, Mr. A. W. Bennett, Prof. Stewart, Mr. A. D. Michael, Dr. Collingwood, Mr. Christy, and Mr. J. E. Harting took part. The last-named speaker pointed out that the observations made by Mr. W. Hatchett Jackson on this centipede had been long ago anticipated by Dr. Macartney in an elaborate paper on luminous insects published in the Philosophical Transactions for 1810 (vol. c. p. 277).—A paper was then read by Mr. T. Johnson on *Dictyopteris*, in which he gave a detailed account of the life-history of this brown seaweed, with remarks on the systematic position of the *Dictyotaceæ*. Dr. Scott, Mr. George Murray, and Mr. A. W. Bennett criticized various portions of the paper, and acknowledged the important scientific bearing of the facts which had been brought out by Mr. Johnson's careful and minute researches.—In the absence of the author, Mr. W. P. Sladen detailed the more important portions of a paper by the Rev. John Gulick, on intensive segregation and divergent evolution in land Mollusca; a paper which might be regarded as a continuation and amplification of the views which the same author had expressed in a former paper published in the Society's Journal last year (vol xx., Zool., pp. 189-274).

Paris.

Academy of Sciences, December 30, 1889.—M. Hermite in the chair.—List of the prizes awarded to successful competitors in the various branches of science during the year 1890:—*Geometry*: Prix Francœur, M. Maximilien Marie; Prix Poncelet, M. Édouard Goursat. *Mechanics*: Extraordinary Prize of 6000 francs, MM. Caspari, Clauzel, and Degouy, 2000 francs each; Prix Montyon, M. Gustave Eiffel; Prix Plumey, M. Widmann. *Astronomy*: Prix Lalande, M. Gonnessiat; Prix Valz, M. Charlois; Prix Janssen, Mr. Norman Lockyer. *Physics*: Prix L. La Caze, M. Hertz. *Statistics*: Prix Montyon, two prizes awarded—one to the late M. Petitdidier and M. Lallemand, the other to Dr. F. Ledé. *Chemistry*: Prix Jecker, MM. A. Combes, R. Engel, and A. Verneuil; Prix L. La Caze, M. F. M. Raoult. *Geology*: Prix Delesse, M. Michel Lévy. *Botany*: Prix Desmazières, M. E. Bréal; Prix Montagne, MM. Ch. Richon and Ern. Roze; Prix Thore, MM. de Bosredon and de Ferry de la Bellone. *Agriculture*: Prix Vaillant, M. Ed. Prillieux. *Anatomy and Zoology*: Grand Prize of the Medical Sciences, MM. L. Félix Henneguy and Louis Roule. *Medicine and Surgery*: Prix Montyon, three prizes were awarded to M. A. Charrin, to MM. A. Kelsch and P. L. Kiener, and to M. Basile Danilewsky, respectively; Prix Bréant, M. A. Laveran; Prix Barbier, MM. M. E. Duval, Ed. Heckel, and F. Schlagdenhauffen; Prix Godard, M. A. Le Dentu; Prix Lallemand, M. Paul Loye; Prix Bellion, MM. F. Lagrange, and Laborde and Magnan; Prix Mège, Dr. A. Auvard. *Physiology*: Prix Montyon, M. A. d'Arsonval; Prix L. La Caze, M. François Franck; Prix Pourat, Dr. Johannes Gad and Dr. J. F. Heymans; Prix Martin-Damourette, M. J. V. Laborde. *Physical Geography*: Prix Gay, M. Drake del Castillo. *General Prizes*: Prix Montyon (Unhealthy Industries), honourable mention of Dr. Maxime Randon; Prix Trémont, M. Jules Morin; Prix Gegner (Physiology), M. H. Toussaint; Prix Petit d'Ormoy (Natural Sciences), M. Jean Henri Fabre; Prix Petit d'Ormoy (Mathematical Sciences), M. Paul Appell; Prix Leconte (Chemical Explosives), M. Paul Vieille; Prix Laplace, two prizes, *ex æquo*, to MM. E. A. A. Verlant and E. Ch. E. Herscher.—The following prizes were proposed for the year 1890:—Grand Prize of the Mathemathical Sciences: To perfect in any important point the theory of differential equations of the first order and of the first degree. Prix Bordin: To study the surfaces whose linear element may be reduced to the form

$$ds^2 = [f(u) - \phi(v)](du^2 + dv^2).$$

Prix Francœur: Inventions or works tending to the progress of pure and applied mathematics. Prix Poncelet: The author of any work tending most to further the progress of pure and applied mathematics. Extraordinary Prize of 6000 francs: Any improvements tending to increase the efficiency of the French naval forces. Prix Montyon: Mechanics. Prix Plumey: Improvement of steam-engines or any other invention contributing most to the progress of steam navigation. Prix Lalande: Astronomy. Prix Damoiseau: To perfect the theory of the long periodical irregularities in the movement of the moon caused by the planets. Prix Valz: Astronomy. Prix Janssen: Physical Astronomy. Prix Montyon: Statistics. Prix Jecker: Organic chemistry. Prix Fontannes: The author of the best work on palæontology. Prix Vaillant: Researches on the agencies that have caused the foldings in the terrestrial crust—part played by horizontal displacements. Prix Gay: Orographic study of any mountain system by new and rapid processes. Prix Barbier: Any valuable discovery in the surgical, medical, or pharmaceutical sciences, and in therapeutic botany. Prix Desmazières: The best work on the whole or any part of the Cryptogamic flora. Prix Montagne: The authors of important works on the anatomy, physiology, development, or description of the lower Crytogamic plants. Prix Thore: Works on the cellular Cryptogams of Europe, and on the habits or anatomy of any species of European insect, alternately. Prix Bordin: Comparative study of the auditory nerve in mammals and birds. Prix Savigny: For young zoological travellers. Prix Serres: On general embryology applied as far as possible to physiology and medicine. Prix Dusgate: The best work on the diagnosis of death, and on the means of preventing premature burials. Prix Montyon: Medicine and surgery. Prix Bréant: The discovery of a certain cure for Asiatic cholera. Prix Godard: On the anatomy, physiology, and pathology of genito-urinary organs. Prix Lallemand: Researches on the nervous system in the widest sense of the term. Prix Bellion: Works or discoveries serviceable to the health of man or to the improvement of the human species. Prix Mège: The author of a continuation and completion of Dr. Mège's essay on the causes that have retarded or favoured the advancement of medicine. Prix Montyon: Experimental physiology. Prix Pourat: On the properties and functions of the nervous cells attached to the organs of sense or to any one of them. Prix Delalande-Guérineau: For the French traveller or naturalist who shall have rendered the greatest service to France or to science. Prix Jérome Ponti: The author of any scientific work the continuation or development of which may be deemed valuable to science. Prix Montyon: Unhealthy industries. Prix Trémont: For any naturalist, artist, or mechanic needing help in carrying out any project useful or glorious for France. Prix Gegner: In aid of any *savant* distinguished by solid work done towards the advancement of the positive sciences. Prix Laplace: For the best student leaving the École Polytechnique.

Berlin.

Physical Society, December 20, 1889.—Prof. von Helmholtz, President, in the chair.—Dr. Assmann demonstrated his aspiration thermometers and psychrometers after having first explained the theory and construction of the latter (see Nature, vol. xxxvii. p. 215, and vol. xl. p. 660). He first dipped one

the thermometers into warm water at 45° C., in such a way that its external metallic envelopment was in contact with the water and took on the temperature of the latter, while at the same time aspiration could proceed undisturbed. When the clock-work was not set in motion and the turbine in the upper part of the instrument was at rest, the thermometer indicated a temperature of 35° C.; but as soon as aspiration was started by setting the clock-work in motion, the temperature recorded fell to 22°·5 C., being now identical with that indicated by a second thermometer not immersed in water. In the next place, a series of experiments was made in order to determine the rate of flow of the air through the thermometer. To effect this the thermometer was attached by an air-tight joint to the upper end of a glass cylinder whose capacity was 5 litres, whose interior was moistened with soapy water, and whose lower end was closed with a soap-film. On setting the instrument in work the time required for the aspiration of 5 litres of air was measured by the time the soap-film occupied in ascending from the lower to the upper end of the cylinder. The speaker showed that when the turbine was in motion the rate of flow of the aspired air was about 2·5 m. per second; when in addition to the turbine an external injector was used, the velocity rose to rather more than 3 m.; when the injector alone was used the velocity was similarly 3 m. The bellows which he had used in his earlier instruments gave a very variable and much slower current of air. Finally, he demonstrated the action of the instrument when employed as a psychrometer. By surrounding the thermometer with gauze and moistening the latter the instrument recorded a temperature of 18° C., while at the same time a similar non-moistened thermometer recorded 21° C. An ordinary psychrometer which was placed in close proximity to the other indicated 21° C. with the dry-bulb, and 16° C. with the wet. The President pointed out that when determining temperatures with an aspiration thermometer the rarefaction of the air must lead to a slight fall of temperature, which is, however, partly compensated for by the friction of the air. Both these factors can be calculated from the known rate of flow of the air.

In the report of the Berlin Physical Society, NATURE, January 2, p. 215, in the fourth line from the bottom, for "Society" read "Institute."

DIARY OF SOCIETIES.

LONDON.

THURSDAY, JANUARY 9.

ROYAL SOCIETY, at 4.30.—New Experiments on the Question of the Fixation of Free Nitrogen (Preliminary Notice): Sir J. B. Lawes, Bart., F.R.S., and Prof. Gilbert, F.R.S.—On Electric Discharge between Electrodes at Different Temperatures in Air and in High Vacua: Prof. J. A. Fleming.—A Milk-dentition in Orycteropus: Oldfield Thomas.

MATHEMATICAL SOCIETY, at 8.—On the Deformation of an Elastic Shell: Prof. H. Lamb, F.R.S.—On the Relation between the Logical Theory of Classes and the Geometrical Theory of Points: A. B. Kempe, F.R.S.—On the Correlation of Two Spaces, each of Three Dimensions: Dr. Hirst, F.R.S.

INSTITUTION OF ELECTRICAL ENGINEERS, at 8.

ROYAL INSTITUTION, at 3.—Electricity (adapted to a Juvenile Auditory): Prof. A. W. Rücker, F.R.S.

FRIDAY, JANUARY 10.

ROYAL ASTRONOMICAL SOCIETY, at 8.

INSTITUTION OF CIVIL ENGINEERS, at 7.30.—The Irrigation Works on the Cauvery Delta: Alfred Chatterton.

SATURDAY, JANUARY 11.

ROYAL BOTANIC SOCIETY, at 3.45.

ESSEX FIELD CLUB, at 7.—The Inter-Relations of the Field Naturalist's Knowledge: Prof. J. Logan Lobley.

SUNDAY, JANUARY 12.

SUNDAY LECTURE SOCIETY, at 4.—Heroes of British India; the Men who Conquered, Ruled, and Saved it: Willmott Dixon.

TUESDAY, JANUARY 14.

ZOOLOGICAL SOCIETY, at 8.30.—On a New Species of Otter from the Lower Pliocene of Eppelsheim: R. Lydekker.—A Complete List of the Sphinges and Bombyces known to occur on the Nilgiri Hills of Southern India, with Descriptions of New Species: G. F. Hampson.—On some Cranial and Dental Characters of the Domestic Dog: Prof. Bertram C. A. Windle and John Humphreys.—Fourth Contribution to the Herpetology of the Solomon Islands: G. A. Boulenger.

INSTITUTION OF CIVIL ENGINEERS, at 8.—Recent Dock Extensions at Liverpool: George Fosbery Lyster.

WEDNESDAY, JANUARY 15.

SOCIETY OF ARTS, at 8.

ROYAL METEOROLOGICAL SOCIETY, at 7.15—Annual General Meeting.—Report of the Council.—Election of Officers and Council.—Atmospheric Dust (illustrated by Lantern Slides): Dr. W. Marcet, F.R.S., President.

ENTOMOLOGICAL SOCIETY, at 7.—Annual Meeting.—Election of the Council and Officers for 1890.—Address by the Right Hon. Lord Walsingham, F.R.S., President.

UNIVERSITY COLLEGE CHEMICAL AND PHYSICAL SOCIETY, at 4.30.—The Magnetization of Iron and Nickel: J. J. Stewart.

THURSDAY, JANUARY 16.

ROYAL SOCIETY, at 4.30.

LINNEAN SOCIETY, at 8.—Life-History of a Remarkable Uredine on Jasminum grandiflora: A. Barclay.—Certain Protective Provisions in some Larval British Teleosteans: E. Prince.

FRIDAY, JANUARY 17.

SOCIETY OF ARTS, at 8.

PHYSICAL SOCIETY, at 5.—On a Carbon Deposit in a Blake Telephone Transmitter: F. B. Hawes.—On Electric Splashes: Prof. S. P. Thompson.—On Galvanometers: Prof. W. E. Ayrton, F.R.S., T. Mather, and W. E. Sumpner.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Food in Health and Disease: Dr. J. Burney Yeo (Cassell).—A Guide for the Electric Testing of Telegraph Cables, 3rd edition: Colonel F. Hoskiœr (Spon).—The Educational Annual, 1890; E. Johnson (Philip).—Parallel Translations of Lines and Surfaces, 2nd edition: D. Maver (Aberdeen, Brown).—Year-book of Pharmacy, 1889 (Churchill).—Naturalistic Photography, 2nd edition: P. H. Emerson (Low).—Warren's Table and Formula Book: Rev. J. Warren (Longmans).—Bergens Museums Aarsberetning for 1888 (Bergen).—Geological Mechanism: J. L. Wilson (J. Heywood).—Bibliography of Meteorology, Part 2, Moisture (Washington).—Proceedings of the Society for Psychical Research, Part 15 (Trübner).—Mind, No. lvii. (Williams and Norgate).

CONTENTS.

THURSDAY, JANUARY 16, 1890.

THE NEW MUZZLING REGULATIONS.

AN essential fault of popular government is in danger of being exemplified just now by the possibility of the selfish interests of a few individuals attracting favourable attention, in utter opposition to the true interests of the nation at large.

A very reprehensible leading article which appeared in the *Standard* on the 4th inst., to which we shall presently refer in fuller detail, has started an agitation in the home counties, especially in Kent, in opposition to the valuable regulations recently issued by Mr. Chaplin against hydrophobia or rabies.

It is not uninstructive to review the way in which the issue of these regulations has been brought about, while it is a matter of painful interest to compare our position in England, as regards the prevalence of rabies, with that of some of the more advanced nations on the Continent.

Before M. Pasteur began his wonderful researches into rabies, the vast majority, even of the highly instructed public, regarded hydrophobia as a kind of Divine visitation, and rabies as a form of canine lunacy. Legislation, in the absence of that which has so frequently been called with a double meaning "a healthy despotism," necessarily lagged behind in the arrest of what everyone now knows to be a simple zymotic disease, which, enzootic in England, becomes, by steady increase during every few years of unchecked development, both epizootic and unfortunately epidemic.

The first advance towards rational prevention of the trouble was made in London in 1885–86 by the Chief Commissioner of Police, first by Sir E. Henderson, afterwards by Sir Charles Warren.

The result of their work is well known—namely, the temporary extirpation of rabies in London. In a country with more respect for scientific fact, such a benefit to the community would have been followed by the general establishment of preventive legislation throughout the centres of the disease, so as to arrest it completely; and this having been effected, the adoption of proper quarantine measures would alone of course have been required to free us for ever from the evil by preventing its reintroduction from abroad.

Partly owing to the fact that, until the most wise establishment by the present Government of a General Board of Agriculture, there was no special authority for moving in the matter, no such general action was taken. Lord Cranbrook, however, was earnestly convinced of the importance of the subject, and conferred a lasting benefit on all those interested in it by appointing that Select Committee of the House of Lords whose Report and evidence not only furnished a complete and exhaustive account of rabies, but also strongly emphasized the necessity of the adoption of thorough legislative measures, especially of muzzling, to prevent and eradicate the malady.

In the meanwhile, rabies in dogs, and of course concurrently its fatal attacks on men, steadily increased, until the spring of last year (1889) saw us threatened again in London with an epidemic like that of 1885.

All the large dog-owners and breeders who had experienced the manifest value of the regulations of 1885 called for the reinstitution of the muzzle, and at the present time the *Field*, *Fancier's Gazette*, &c., afford strong proof, in the earnestness of their expressions of satisfaction at the present muzzling order, of the folly of their contemporary who has endeavoured to oppose it.

Of course, as before, a few agitators, trading on the innate selfishness of some natures, and supported by the money of a small band of individuals whose names should be for ever preserved as having sought to work harm to their fellow-creatures, recommenced their irresponsible attacks on the authorities and others for this much-needed sanitary regulation, and it is a recrudescence of this selfish obstruction which the *Standard* has attempted for some (as yet unknown) reason to revive.

An amusing, if degrading feature of such opposition is the constant change of front which the inevitable progress of scientific truth forces upon these people, as their misstatements and ignorance become revealed to the public. At different stages of the agitation, their leaders, Miss Cobbe, "Ouida," and others, have stated with inexplicable self-contradiction, that no such disease as rabies existed, that it was wholly imaginary, that it was rare in England, that the police ran no risks in extirpating it, that the muzzle produced this (non-existent) disease, and so on to the end of the chapter. But while the logical difficulties in which these writers involve themselves must excite amusement, it is a matter of serious regret that they cannot be legally dealt with like other disseminators of false news, such for instance as those who in the wilderness of the "great gooseberry season" cry "'orrible murder" when homicide is *pro tem.* non-existent. The evil done by these latter is indeed small, compared with that of the far graver false statements which we have cited above.

In spite, however, of this flood of misrepresentations the muzzling regulations were enforced in London, and with notable benefit, and by the recent order they have been continued and extended by Mr. Chaplin, so as to cut right at the root of the evil, viz. in all the centres of the disease simultaneously.

It was with the consciousness that this measure would be required by the country of the President of the Board of Agriculture, that the anti-muzzlites made a last effort against it by holding a public meeting. The real nature of this agitation, which had been notorious from the commencement, was then made most amusingly conspicuous. We refer to the fact that this variety of obstruction is in truth only a branch of the anti-vivisectionist agitation, and worthy of such a parent stem. It seems that at the meeting an *amendment in strong support of muzzling* was carried by a majority of something like 80 per cent. The fact of the origin of the Association which had summoned the meeting having been alluded to, the Chairman, the Bishop of Ely, first (we are glad to see) repudiated the idea that he was an anti-vivisectionist, and then went on to say that the anti-vivisectionists had nothing to do with the anti-muzzling agitation. This repudiation on the Bishop's part was followed by the resignation of the originators of the movement, Miss Cobbe and others, demonstrating the truth of what we have just said and the inaccuracy of the Bishop's second statement.

The general facts bearing upon the origin and development of the agitation were fully exposed at the meeting,

so that the strong expression of opinion in favour of the muzzling regulations (in conjunction with the disingenuousness of the argument of their opponents) is easily understood.

From a survey of the known behaviour of animals affected with rabies, and in accordance with the measures customarily adopted in dealing with infection among animals, where as in the present case it is not desirable to interfere with their free movement from place to place, Mr. Chaplin declared a number of counties as infected, taking areas around to provide sufficient margin against conveyance of contagion.

It is this wise and carefully-designed attempt to stamp out the disease, which the *Standard*, alone in the Press, has attacked in the most unmeasured language. Having no "case" from the scientific and medical stand-point, the editor through his leader-writers abuses his opponent's attorney (if Mr. Chaplin will forgive the simile). The Conservatives in Kent are positively called upon by the leading daily paper of their party to vote against their own Government, and why? Because they are asked to help stamp out rabies; and at what cost? it may be asked. None save that of the hire of a muzzle.

This is where the difficulty of our kind of Government arises. Because a solitary voice in the Press objects to a sanitary measure, which has nothing whatever to do with politics, ill-feeling is to be aroused among the voters. It is, however, satisfactory to add that possibly no such attempt on the part of any journal has ever met with such a chilling reception from the rest of its contemporaries—those who have not refrained from observations on the matter having only mentioned it to utterly condemn it.

A sanitary question, to our mind, becomes a question of moral right or wrong when the means proposed for its solution involve nothing beyond a little reasonable trouble, and it is this view of the matter which we fancy finally crystallizes out in the form of what is called public opinion. After the process of the actual experience of the last five years, public opinion is evidently set in the direction of preventing hydrophobia by muzzling. It is of course impossible that Mr. Chaplin should yield to this, the first abusive attack that has been made upon him in his official capacity, but certainly if anything should support him, it is the cognizance of the unworthiness of the opposition which the *Standard* has fomented against his action in the service of the community.

We should wish in conclusion to direct attention to certain obvious deductions which can justly be drawn from the history of this matter, and other events connected with the subject of rabies.

Both the prevention and the cure of this horrible zymotic malady are the outcome of close scientific experimental work. It was reserved for M. Pasteur to make clear and harmonize the various stages (always obscure and apparently contradictory at first) of our knowledge by the immense progress he inaugurated and carried out in the study of infection.

It is M. Pasteur who himself has pointed out better than anyone how the disease can be prevented from attacking man or animals, and he is the first who has shown in the slightest degree how it can be prevented from developing in the system after it has gained access to the body.

The nineteenth century, however, affords no shelter to the man of science to discover benefits for his fellow-men, for although the progress of knowledge has fortunately destroyed the Inquisition, yet society tolerates the existence of the anti-vivisectionist agitation, which not only scatters broadcast the foulest and falsest aspersions on such a man's life and character, but in its most recent development violently opposes the advance of hygiene.

POLYTECHNICS FOR LONDON.

WHETHER or not the London County Council comes to the wise decision to utilize the provisions of the new Technical Instruction Act, it is probable that for the most part Londoners will have to look for intermediate and higher technical instruction to other agencies than rate-aided schools, at all events in the immediate future. In these matters London is in an exceptional position as the capital of the Empire. In the first place, it is the natural home of the Normal Schools of Science and Art which form part of the machinery of the Science and Art Department. And, besides this, it is the centre of greatest activity of the organization of the City and Guilds Institute, whose three model Colleges are all situated within the metropolitan area.

The proportion, however, of the inhabitants of London whose education is affected by these higher institutions is necessarily small. The Government schools are imperial rather than local, and their situation is chosen regardless of the industrial needs of London. The Central Institution of the City and Guilds likewise belies its name by its situation at South Kensington. The other two schools of the City and Guilds, at Finsbury and Kennington, have a direct and most important relation to surrounding industries, and keep high the standard of what teaching in applied science and art ought to be. But teaching of this high order is very expensive, though the fees charged may be low, and of recent years a newer and more popular movement has sprung up, aiming at a lower standard of instruction carried on at less cost, and adapted, so far as practicable, to the benefit of the mass of working men.

The best type of such institutions in London is the so-called "Polytechnic" in Regent Street. The basis of the organization is the Young Men's Christian Institute started some years ago by Mr. Quintin Hogg. Round this nucleus he has gradually built up an institution in which evening classes, recreation, and gymnastics have all a part. Under his guidance the Institute has grown to great dimensions, and a number of very largely-attended classes of all kinds are now conducted in the building which for many years was occupied by the "Polytechnic" of the diving-bell and Prof. Pepper. Many of the classes are in general and commercial subjects, but there are science and art classes in connection with South Kensington, technological classes in connection with the City and Guilds Institute, and trade and practical classes in various industries and handicrafts. The greater part are held in the evening, but there are also day classes; and day schools for boys and girls are attached to the institution.

It will be seen that this experiment in technical educa-

tion differs very materially in plan from that of such an institution as Finsbury College. The educational side of the Polytechnic does not form an organized school course so much as a set of classes among which a student may choose, and the standard aimed at is not so high. But there is this obvious advantage in taking the Polytechnic as a model for similar institutions that the instruction, so far as it goes, is far less costly than at Finsbury, being largely subsidized by science and art grants.

The example of the Polytechnic has been recently followed, with a certain amount of success, at the People's Palace in Mile End, where the Drapers' Company have devoted the funds which they have withdrawn from the City and Guilds Institute to building and endowing a school somewhat on the Polytechnic lines.

While these institutions have been developing, the Charity Commissioners have been engaged in pursuance of Mr. Bryce's Act of 1883 in framing a scheme for the application of the funds of the City parochial charities for the benefit of the working classes of greater London. The Commissioners came early to the determination to devote a large proportion of the proceeds of the charities to some educational purpose, and decided further that the main direction of the educational institutions thus established should be technical and industrial.

It is not our purpose to enter at all into the questions that have been raised as to the mode of division of the endowment between secular and ecclesiastical purposes, or the wisdom of tying up the greater part of the disposable funds in perpetuity. There are plenty of keen observers who will make their views felt on these questions; and indeed many champions of other schemes, such as the promotion of open spaces, are already in the field. But we must regard the main object to which the funds will be devoted as practically decided. The Charity Commissioners gave notice of it in their last Report, and little exception seemed then to be taken to the project. Since then large sums of money have been raised by local subscriptions on the faith of the proposal. It is too late now to advocate the application of the main part of the fund to any other object than education, and those who are agitating for such a change are, in our opinion, wasting their powder and shot.

But while the public is easily induced to join in a general outcry which, if it has any justification, certainly comes far too late, it is quite possible that, unless vigilant care is exercised, the final scheme may come into force without those alterations and improvements in detail which seem individually of small importance, but may make all the difference between a good and a bad scheme of technical education for London. The funds handled are far larger than those authorized to be raised for the whole of Wales under the new Intermediate Education Act. It behoves all friends of education to take care that these large endowments are used aright.

Let us glance, then, at the main outline of the scheme so far as it relates to technical education. The Commissioners were instructed under the Act to make provision for the "poorer classes." Consequently any technical schools established or aided under the scheme must aim directly at the benefit of the workman rather than that of the manager.

The Commissioners propose to devote large capital grants to the erection of technical and recreative institutes in various parts of London, somewhat on the model of the Regent Street Polytechnic, and to give a permanent endowment to these institutes, as well as to the Polytechnic and the People's Palace already in existence. Each institute is to be governed under a scheme, devised by the Charity Commission, and is to be subject to the general control of a Central Governing Body of Trustees.

The objects of the institutes are threefold. They are to be social centres, where concerts and entertainments may be given, and where outside clubs and working men's societies may have an opportunity of meeting; they are to include young men's and young women's institutes for social and recreative purposes, open to "young persons" between the ages of sixteen and twenty-five; and lastly, they are to provide for the educational wants of the working classes in the neighbourhood. Libraries, museums, swimming-baths, and gymnasia will form part of the equipment of most of these institutions.

It is with the educational work of these "Polytechnics" that we are here most directly concerned. But their educational and social sides must be very closely linked together, and the success of the classes will largely depend on the success of the institute as a whole. Entrance to the clubs may, under the scheme, be made contingent on entrance to the classes, as is now the case at the People's Palace, though such a course seems to us to be unwise. In any case we must not pass over the social side of the institutes without a word. The Young Men's Institute at the Polytechnic has been a great success, but it has been a growth of time, and it has grown round the nucleus of the Y.M.C.A. The social Institute at the People's Palace has sprung suddenly into existence, without the pre-existing nucleus; it is admitted to have been a failure, and is now suppressed. Can the lesson be mistaken? Doubtless the Charity Commissioners are alive to the difficulty. Their detailed regulations for the management of an institute, of which the draft has been published, are, in the main, carefully drawn. But those who hope that the scheme will result in the growth of a number of Palaces of Delight which will delight Mr. Walter Besant's heart will be doomed to disappointment. There will be no "People's Palaces"—only "*Young* People's Institutes." The present People's Palace will be constrained to confine its membership in future to persons between the ages of sixteen and twenty-five. Why this limitation? We see with pleasure that the Goldsmiths' Company, who are founding an institute at New Cross on somewhat the same model as those proposed by the scheme, have struck out the upper limit. There are far too many of these restrictions in the scheme. For example, smoking and dancing are (the latter with certain specified exceptions) forbidden. Surely details such as these can be left to the by-laws of the several institutes. Here, again, the Goldsmiths' Company have shown themselves in advance of the Charity Commission.

We have a similar criticism to make on the whole of the educational scheme. There is too little guidance in matters of principle, too much restriction in matters of detail.

Perhaps the most important thing to ensure is that the Central Governing Body shall be a strong body, exercising effective supervision over the teaching of the various

institutes. Its official name ("Trustees of the City Parochial Charities") is unfortunate; it has too much of a flavour of Mr. Bumble's "porochial" office. It would require an Act of Parliament to change the name, so the best thing to do is to let it be forgotten. The Central Governing Body (for so let us call it) is to be representative of the Crown, the City Corporation, the County Council, the higher Colleges and University of London, the Ecclesiastical Commissioners (temporarily), and the Governing Bodies of the Bishopsgate and Cripplegate Foundations. No one can forecast the action of such a hybrid body until we know the actual men who are to be nominated. A very efficient educational body might be elected as proposed, and on the other hand it mightn't. It is to be hoped that one of the blots on the constitution of the Board—the absence of working-men representatives—will be partly corrected by the inclusion of some working-men leaders among the five Crown nominees. But it is impossible to resist the conviction that the suggested constitution—suitable enough to the time when the Act was passed and London had no organized system of local government—has far too little of the popular element, and that it would be far better to put the whole management of the scheme in the hands of the County Council, or a joint committee of the County Council and School Board.

Supposing that the Central Body is all that could be wished, the next thing to ensure is the satisfactory composition of the governing bodies of the various institutes, and their organic connection with the Central Body. It is essential that the schemes shall be so arranged that the educational programme of all the institutes shall pass through the hands of competent experts, and the educational work shall be adequately supervised, inspected, and revised, from time to time. The Charity Commissioners propose two methods of attaining this result. They give three nominations on each governing Board to the Central Governing Body, and these three members may be experts, though of this there is no guarantee. Further, the secretary of each institute is required to send to the secretary of the Central Governing Body a complete list of proposed classes a week before each term. This is presumably intended to give a power of suggestion, if not revision, to the Central Body, but what is the use of suggestions a week before term? What is wanted is a central committee of well-known experts to advise the Central Governing Body on educational matters. The committee should be small—say three scientific and three artistic representatives. They should be paid for their services, and should be in touch with the science and art divisions of every institute.

There is nothing in the scheme to prevent the appointment of such a Committee, though it would be well if some distinct suggestion of the kind were made. In any case it is a matter to be borne in mind and pressed when the time comes, for it may make all the difference in the world to the future of technical education in London. Let us be frank about the matter. How many men are likely in any given district to be on the governing body of the local institute who know the difference between good teaching and bad? And yet no scheme, however admirably drawn, will produce a good technical school, unless it is worked by such men. On the other hand, with a first-rate governing body we have little fear. Payment by results will lose most of its terrors if those in power know the difference between the incompetence which *cannot* earn grants, and the independence which prefers real teaching to cram. And we may add that it is only by associating with the governing body members engaged in local industries that the practical character of the trade classes can be assured.

So much for the machinery. We must next say a word about the character of the instruction to be aimed at in the institutions. It is to be mainly technical, and hence must be adapted to the special needs of each locality. It is by this time a truism to say that this adaptation will not be brought about by allowing a set of science and art teachers to take the line of least resistance through the South Kensington Directory to the goal of the maximum of grant. A lady is reported to have lately obtained a silver medal for agriculture at a London institution which the Charity Commissioners are proposing to endow. Is this adaptation to local needs and industries?

We wish sincerely that those responsible for the whole scheme had been able to arrange for exceptional treatment of the new institutes in the matter of the apportionment of the Government grant now paid on results. No better opportunity is likely to present itself for an experiment in basing grant on efficient inspection rather than on examination. But what chance is there of such a proposal when our Government departments responsible for public education are cut up into air-tight compartments without connection among themselves? The Charity Commission, the Education Department, and the Science and Art Department still form a great circumlocution office, and until this is altered abuses will continue, which it is nobody's business to remedy. Our great hope, therefore, depends on the choice of the principals, teachers, secretaries, inspectors, and governing bodies, who will make or mar the institutes through which, for many years, Londoners will derive their technical instruction. Let them be enlightened men, with broad views and sympathies, who know their business, or at least know their limitations, and all may be well. But if not, it were better that the whole scheme were put in the fire.

What, again, is to be the scope of the instruction? Is it to be mainly confined to the level of "elementary" science and "second-grade" art? Or are there to be advanced classes in more specialized subjects? Provision is made for such classes in the scheme if they can be arranged without trenching on the endowment. The Commissioners are probably afraid of misapplying funds intended for the poor to the benefit of the middle classes. There is justice in their objection, but such instruction can never be made self-supporting, and it is most important that it should be included in the programme of the institutes, if only to keep the standard high throughout. Here is then an opportunity for the City and Guilds Institute. Let it relieve itself of the charge of its examinations, which may now be transferred on equitable terms to the Science and Art Department under the provisions of the Technical Instruction Act, and let it also transfer to the Government the Central Institution, the geographical situation of which marks it out plainly as an adjunct rather than a rival to the Normal School, and let it apply the energy thus liberated in establishing in every "Poly-

technic" a higher department, providing for the more specialized wants of each locality. This will be a work which no body is so well fitted to undertake as the great Institute which has been a pioneer in higher technical instruction. Such, it appears to us, is the true solution of the question of the relations between the Charity Commissioners' scheme and the City and Guilds of London.

One word of caution in conclusion. The new institutes should be allowed to grow, and not be started on too ambitious a scale at first. Local wants change, and the institutes should develop in harmony with their changes. This is the lesson of the old Mechanics' Institutes and Athenæums. The lesson is repeated in the newer experiments of Mr. Hogg's Polytechnic, and the People's Palace. We do not want to begin with erecting huge shells of bricks and mortar, hoping that life will somehow come into them after a time. The life first, then the buildings, to grow as it expands and deepens—that surely is the law of nature. "Several architectural white elephants" is the dismal but suggestive forecast of a writer in the *Charity Organization Review*, on the supposition that this law is violated. If these warnings are neglected, the promoters of the movement will be merely courting failure, however good their intentions may be. And they will have failed because "they were not poets enough to understand that life develops from within."

ASSAYING.

Text-book of Assaying. By C. Beringer and J. J. Beringer. (London: Griffin and Co., 1889.)

THIS text-book marks an important departure in the literature of assaying. The authors abandon the dreary details of traditionary methods, and attempt with success to rationalize the art of the assayer, rather than to follow the usual course of reproducing "dry" assay methods and elaborate classifications of processes the interest of which is only historical. Assaying is here treated, in a broad sense, as the determination, by analytical methods, of components of ores and of intermediate or finished metallurgical products. Such compounds may be either of value in themselves, or important from being valuable or injurious in the operations of smelting, or in adapting the metals for use.

The methods of the authors, and the measure of success which they have attained, may be fairly judged by their treatment of copper, ead, and iron. Copper ores and furnace materials are still sold in the English market by the "Cornish" assay. This antiquated method of assaying has really no claim to retention, now that more trustworthy methods are well known, and the authors give it but little prominence. They, however, repeat the fallacious argument of its apologists by stating that "it gives the purchaser an idea of the quantity and quality of the metal that can be got by smelting." The Cornish assay does not deserve even this modified approval, as the results it affords neither represent the actual amount of copper contained in the ore, nor the proportion of metal which can be produced by smelting, and several expert assayers, working on portions of the same samples, will obtain results which vary in the most erratic way. Fortunately for those who may be guided by this text-book, its authors proceed to describe assaying processes which are really well calculated to give trustworthy indications as to the quantity and quality of metal obtainable from ores. These are to be found in well proved "wet" methods of determining actual copper contained in ores as well as the components that interfere with the extraction and the quality of the metal. In describing these methods, ample information is given for the guidance of the smelter under the varying conditions of the metal's occurrence. While passing shortly over the Cornish assay, the authors judiciously omit such clumsy "wet" methods of assay as the direct titration by cyanide of potassium, which is retained in some recent books of standing, although it has been abandoned by most skilful assayers. On the other hand, titration by cyanide of potassium after separation of the copper from interfering metals, and the assay by electrolysis, leave little to be desired in rapidity and accuracy, and to these due prominence is given. Failing reasonable manipulative skill, no assay can be accurate, and the expertness demanded by those who conduct the "dry" or Cornish assay is not more easily acquired than is the analytical skill needed for better "wet" methods. In an assay method giving accurately the amount of metal actually present in the ore, the metallurgist has a sure basis for calculation, the results of which can be brought under the control of his experience as to the losses of metal in operations on a large scale. The results of the Cornish assay, with all its inherent uncertainty, have equally to be judged in the light of the smelter's experience as to what the final "out-turn" will be. In lead, again, the dry assay is usually treated in books on assaying with much elaboration, which is no longer useful, if it ever was. It gives results that indicate neither the actual amount of metal contained in the ore, nor the amount which will be produced by smelting, and like the Cornish assay for copper is most unsatisfactory for guidance in smelting. The wet methods of lead assaying which are described are convenient and trustworthy, while the only practically useful methods of dry lead assay are given in sufficient detail. In the assay of iron ores we find dry methods entirely omitted. The wisdom of this cannot be doubted, for the want of exactitude which is characteristic of the dry assay of copper and lead is still more marked in the dry assay of iron. Processes of wet assay capable of giving prompt and strictly accurate results are available, and these are fully described.

The plan of subordinating or ignoring unsatisfactory methods of assay, while giving prominence to those which have proved to be trustworthy, runs through the treatment of methods of assaying the other metals, as well as estimating the components of ores which are not usually dealt with in books on assaying. Among the latter are silica, the earths, sulphur, arsenic, and phosphorus. These demand study by the metallurgist, to whom, under either the necessity of "fluxing" them away, or of minimizing their interference with the purity of the metals, their ready and accurate determination is a matter of the greatest importance. The details of assaying the precious metals, though hardly sufficient for adoption in the assay of bullion in a mint, are all that is needed in a works.

The authors have clearly not been content to merely record published processes, but in order to add to the completeness of their work have given unpublished

results of the experience acquired by themselves and others. The writer notices their description of a process for the estimation of arsenic in minerals and metals, which was devised by himself for use in works under his control, that has not hitherto been published. It consists in the separation of arsenic from its associations, by distillation with ferric chloride mixed with calcium chloride, and subsequent titration of the distillate by iodine. The authors are mistaken in stating that there is a difficulty in obtaining ferric chloride free from arsenic. Even if there were difficulties, it is obvious that the process itself affords a ready means of eliminating arsenic from the ferric chloride mixture, before using it in the actual assay. In this and one or two other cases, there is a tendency to adopt the always undesirable method of "blank" experiments to correct error arising from the use of impure reagents, rather than whenever practicable to avoid the source of danger by the use of pure materials. These are, however, hardly noticeable blemishes in a really meritorious work, that may safely be depended upon by those using it either for systematic instruction or for reference. THOMAS GIBB.

BREWING MICROSCOPY.

The Microscope in the Brewery and Malt House. By Chas. Geo. Mathews, F.C.S., F.I.C., &c., and Francis Edw. Lott, F.I.C., A.R.S.M., &c. (London and Derby: Bemrose and Sons, 1889.)

THERE are certainly few industries the growth and development of which have been more influenced by the progress of pure scientific discovery than those of the brewer and distiller. These industries, formerly carried on upon purely empirical lines, handed down from father to son through countless generations, have in recent years, through the advances in chemical and biological science, been so transformed that their successful conduct at the present time requires a most thorough acquaintance with the leading principles of these sciences. As a consequence of this change, we find an increasing tendency for these industries to become concentrated in a smaller number of hands each producing on a larger and larger scale. The small brewer himself lacking the necessary scientific training, and not able to afford the requisite skilled assistance, gives way before the larger breweries employing a complete scientific staff and provided with the latest improvements.

The present work is, we understand, intended to bring before those connected with brewing a concise account of the assistance which may be derived in the conduct of their business from the use of the microscope. We are of opinion that the authors have been unfortunate already in the choice of their title, as one of the most conspicuous results of modern scientific research in this direction is that the use of the microscope alone is of comparatively little value in the study of micro-organisms in general, whether connected with fermentation or other processes. This inadequacy of microscopic study *per se* the authors in various parts of their work indeed frankly admit. Modern students of these low forms of life have, in fact, become more and more aware of the fallacious results yielded by mere microscopical observation when unaccompanied and uncontrolled by those processes of cultivation which have been developed during the past ten years. Even the work performed under the auspices of the masterly genius and supreme experimental skill of Pasteur has had to be revised and brought up to date by Hansen, with the aid of the more recent methods of research. Now, although the authors appear fully aware of the great change which has taken place since the earlier work of Pasteur, Reess, Fitz, and others, they have not sufficiently distinguished between observations which rest upon the surest foundation and fulfilling the most modern requirements, and those which, though possibly correct, require repetition and confirmation.

The absence of sharp differentiation in this matter cannot fail, we believe, to occasion much confusion in the mind of the ordinary practical student who depends upon text-books and manuals for his guidance and information, and it is, in our opinion, quite unnecessary that he should be burdened with the microscopic descriptions of the various forms of yeast given by the older observers, who were almost certainly dealing with impure cultures, but on the contrary he should rather devote his whole attention to the characters of such undoubtedly pure forms of yeast as have been obtained by the most recent methods. Moreover, unless the necessity of resorting to these cultivation experiments for obtaining accurate information is duly impressed upon the student, he will naturally be inclined to shirk these far more laborious and difficult observations, and place undue reliance upon microscopic features.

These remarks apply, perhaps, with even greater force to the manner in which the authors have dealt with the schizomycetes; in this part of the book we find much space devoted to microscopic descriptions of bacteria of uncertain purity, whilst there is little or nothing said about the methods by which these organisms can be really identified, and their characters defined. We also miss any adequate account of the staining-processes which are so invaluable in obtaining a correct idea of the microscopic forms and dimensions of bacteria. As an instance of the unsatisfactory present condition of brewing microscopy, we may quote the following sentence: "*Bact. lactis*, as seen in beers, is generally in the form of small rods, 2 to 3 μ in length, and sometimes in threads containing from 2 to 5 individuals; it is not certain, however, that this form is *B. lactis*." Thus, in respect of the bacterium which is perhaps of most consequence to the brewer, as being "the most commonly occurring disease-organism encountered in the brewing process" there is this absolute lack of all precise information.

What may be called the more purely scientific part of the work is succeeded by a chapter of "general remarks on the brewing process," which, embodying as it does some of the practical experience of the authors themselves, we would have gladly seen enlarged.

The book, which is printed on excellent paper and elegantly got up, is illustrated with a number of admirably executed plates, many of the best of which are original.

A full index and glossary are appended.

OUR BOOK SHELF.

Flower-Land: an Introduction to Botany. By Robert Fisher, M.A., Vicar of Sewerby, Yorks. (London: Bemrose and Sons, 1889.)

THIS is a capital first book of botany, intended for small children. The style, however, is really more elementary than the matter, and a child who has mastered this book will have made a very good start in the science. There is a good deal of information given about the internal structure and function, as well as the external form, of the organs of plants, and this information is given correctly, as well as clearly.

The book is illustrated by 177 woodcuts, most of which are well suited to their purpose. D. H. S.

Five Months' Fine Weather in Canada, Western U.S., and Mexico. By Mrs. E. H. Carbutt. (London: Sampson Low and Co., 1889.)

IN this book Mrs. Carbutt records her experiences during a remarkably pleasant journey made by herself and her husband in the New World. The scenes she describes have often been described before, but she writes so brightly about what she saw that even readers to whom she has nothing new to tell will find a good deal to interest them in her narrative. They will be particularly pleased with her account of "sunny Mexico, and its merry, courteous people."

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Duke of Argyll and the Neo-Darwinians.

IT has a curious and not uninstructive effect to see the pages of this journal invaded by the methods of discussion which are characteristic of political warfare. The letter of the Duke of Argyll, published in NATURE for December 26, 1889 (p. 173) is a clever debating speech. But it rather obscures than illuminates the questions really at issue. And, after the fashion of the political orator, it attributes to those who disagree with the writer motives which, in so far as they differ from reasoned conviction, are essentially insincere.

In politics, the personal rivalry which is bound up inextricably with the solution of great problems may make it a necessary part of the game to endeavour to belittle one's opponents. But in science it is not so. The newer problems which have been raised by Darwinism depend for their solution upon the discussion of evidence, and no competent biologists will, in the long run, be influenced in the opinions they form about them by anything else.

There is nothing in the Duke's letter which has not been worn threadbare by discussion. Still, there are, no doubt, many readers of NATURE who, while taking a general interest in the matter, have not followed all that has been written about it. I am disposed to think, therefore, that it may not be without its use to go over the ground which the letter covers.

First, as to acquired characters. Let us take a simple case. It is admitted that a blacksmith, by the constant use of his arms, may stimulate their abnormal muscular development; that is an acquired character. But a working man, whose arms are of perfectly average dimensions, may nevertheless have a son with arms which would seem to mark him out for the blacksmith's profession; that would be a congenital variation. Now we know that a congenital variation is likely to be inherited; that is a matter of observation. What is the case as to the acquired character? The answer must be, I take it, that there is no probability that the arms of a blacksmith's son will differ in any respect from those of the average inhabitant in the locality where he was born. The Duke of Argyll, however, suggests that there is "no necessary antagonism between congenital variation and the transmission of acquired characters." This is perfectly reasonable; theoretically, there is none. But this does not make the transmission of acquired characters less doubtful. The Duke has no doubt about it, however. "So far from its being unproved, it is consistent with all observation and all experience. It lies at the foundation of all organic development." Very possibly, but where is the observation and where is the experience? These are *the* biological *desiderata* of the day. Imagine the fate at the Duke's hands of any scientific writer who put forward statements such as these unsupported by a shred of a fact.

"This being so," however, the question then arises, Why do extreme Darwinians so fiercely oppose the idea of the transmission of acquired characters? Well, it is obvious that they do so because they think the evidence in its favour insufficient, and it is clearly the duty of a scientific man, whether an extreme Darwinian or not, to oppose the acceptance of that which experience does not support. But the Duke of Argyll attributes their opposition to two causes: first, jealousy of associating the names of Lamarck and Darwin; and, secondly, the dethronement of their idol Fortuity. The first of these reasons is almost too preposterous to discuss. No serious naturalist would speak with other than respect of Lamarck's position in scientific history; this cannot be effaced however much that of Darwin may be magnified. And no serious naturalist would adhere to any theory Darwin had propounded a moment longer than the evidence seemed to carry conviction. The charge in this particular matter is, however, the more grotesque, because, although Darwin did not esteem as of much value Lamarck's doctrine of development and progression, we know that his own mind became more and more fluid on the question of the "direct action of conditions." The idea is in fact so plausible that the difficulty is not in accepting it, but in shaking oneself free from it. What were probably the last words which Darwin wrote on the subject are contained in a letter to Prof. Semper, dated July 19, 1881. I quote a passage which appears to me to pretty accurately define the present position of the question:—

"No doubt I originally attributed too little weight to the direct action of conditions, but Hoffmann's paper has staggered me. Perhaps hundreds of generations of exposure are necessary. It is a most perplexing subject. I wish I was not so old, and had more strength, for I see lines of research to follow. Hoffmann even doubts whether plants vary more under cultivation than in their native home and under their natural conditions ("Life and Letters," vol. iii. p. 345).

Darwin's difficulty, in point of fact, was exactly that of everyone else. The evidence, instead of being "consistent with all observation and all experience," failed to be forthcoming.

The second reason is equally baseless. Fortuity is no idol of the neo-Darwinians; if it is an idol at all, it is an "idol of the market," imposed upon their understanding by the Duke. But at any rate he does not attribute any blame to Darwin. And as this is a rather important matter, on which I admit that persons who ought to know better have gone astray, I will quote a passage on the subject from Prof. Huxley's admirable biography (Proc. Roy. Soc., No. 269):—

"Those, again, who compare the operation of the natural causes which bring about variation and selection with what they are pleased to call 'chance,' can hardly have read the opening paragraph of the fifth chapter of the 'Origin' (ed. 1, p. 131): 'I have sometimes spoken as if the variations . . . . had been due to chance. This is of course a wholly incorrect expression, but it seems to acknowledge plainly our ignorance of the cause of each particular variation.'"

It is obvious that the use of accidental in the guarded sense in which it is employed by Darwin is widely different from fortuitous as employed by the Duke of Argyll. Darwin took variation as a fact of experience. Its causes and laws have still to be worked out. One of the latter, due to Quetelet, was explained by Prof. George Darwin in this journal (vol. viii., 1873, p. 505). He says: "One may assume, with come confidence, that under normal conditions, the variation of any organ in the same species may be symmetrically grouped about a centre of greatest density."

And this is quite in accord with the remark of Weismann that variability is not something independent of and in some way added to the organism, but is a mere expression for the fluctuations in its type. Variation is therefore not unlimited, and we must admit with Weismann that its limits are determined by "the underlying physical nature of the organism;" or as he again puts it, "under the most favourable circumstances a bird

can never be transformed into a mammal." There is something more therefore than blind chance at work here.

But within the limits, it is a matter of experience that every possible variation may occur. If anyone will take the trouble to examine the leaves of the ribbon-grass so commonly cultivated in gardens, he will find it impossible to obtain any pair in which the green and white striping is exactly alike. If it were possible to raise to maturity all the progeny of some prolific organism, the same diversity (in different degree, of course) would manifest itself; but the whole group of variations in respect of any one organ would obey Quetelet's law. When we attempt to give some physical explanation of this fact, we know from the objective facts which have been made out about fertilization that, although the protoplasmic content of the fertilized ovum is, in a general sense, uniform, its actual structure and physiological components must be combined in as endless variety as the green and white stripes of the leaves of the ribbon-grass. If, with Prof. Lankester, we say that the combinations are kaleidoscopic, I do not see that we go beyond the facts. And it appears to me quite permissible to correlate the ascertained variable constitution of the ovum arising from this cause with the equally ascertained varying structure of the organism developed from it.

Of the varied progeny, we know that some survive and others do not. And what Darwin has taught us is, that the reason of survival is the possession of favourable variations. The surviving race necessarily differs somewhat from its progenitors, and Darwin has further stated that it is probable that by the continued repetition of the process all the diversity of organic nature has been brought about.

The area of fortuity is narrowed down therefore, on this point of view, to the variable constitution of the individual ovum. And it is upon the recognition of this fact, for which there seems to be good scientific evidence, that the Duke of Argyll founds his charge that the neo-Darwinians make fortuity their idol. The reason appears to be that it comes into collision with teleological views. But such collisions are no new event in the history of the biological sciences. And teleology, like a wise damsel, has generally, though temporarily ruffled, managed to gather up her skirts with dignity and make the best of it. For some element of fortuity is inseparable from life as we see it. It is at the bottom one of the most pathetic things about it. Nowhere is this more vividly portrayed perhaps than by Addison in the "Vision of Mirzah." Yet I do not remember that anyone was ever so unwise as to taunt Addison with making fortuity his idol.

But, philosophically considered, what is gained by this tenacity about out-works? I reply, exactly as much as was gained by the tenacity of the Church in respect to the geocentric theory of the planetary system. Scientific men cannot be stopped in the application of their best ability to the investigation of Nature. If their conclusions are false, they will detect the falsity; if true, they will not be deterred from accepting them by some *a priori* conception of the order of the universe. It is not justifiable to say that this is due to any devotion to such an empty abstraction as fortuity. No scientific man is, I hope, so foolish as to suppose that, however completely mechanical may be his conception of Nature, he is in any way competent to account for its existence. The real problem of all is only pushed further back. And the Duke of Argyll's difficulty resolves itself into the old question, whether it is more orthodox to conceive of the universe as an automatically self-regulating machine, or as one which requires tinkering at every moment of its action.

It may be replied that this is all very well, but that it is not the way the neo-Darwinians state their case. I may be, therefore, excused for quoting some passages to the contrary from Weismann's "Studies in the Theory of Descent":—

"This conception represents very precisely the well-known decision of Kant: 'Since we cannot in any case know *a priori* to what extent the mechanism of Nature serves as a means to every final purpose in the latter, or how far the mechanical explanation possible to us reaches,' natural science must everywhere press the attempt at mechanical explanation as far as possible" (p. 638).

Further, he quotes from Karl Ernst von Baer:—

"The naturalist must always commence with details, and may then afterwards ask whether the totality of details leads him to a general and final basis of intentional design" (p. 639).

Again, he says:—

"We now believe that organic nature must be conceived as mechanical. But does it thereby follow that we must totally deny a final universal cause? Certainly not; it would be a great delusion if anyone were to believe that he had arrived at a comprehension of the universe by tracing the phenomena of Nature to mechanical principles" (p. 710).

In truth, this revolt of teleology against Darwinism is a little ungrateful. For, if Darwinism has done anything, it has carried on and indefinitely extended its work. In the last century, teleology was, it seems to me, a valuable motive-power in biological research. Such a book as Derham's "Physico-Theology" (1711) may be read with interest even now. I well remember that my first ideas of adaptive structures were obtained from the pages of Paley. Thirty years ago I do not know, except from them and the notes to Darwin's "Botanic Garden," where such information was to be otained. The basis of research was, however, too narrow to continue; it did not look beyond the welfare of the individual. The more subtle and recondite springs of adaptation opened up by the researches of Darwin, which look to the welfare of the race, were not within its purview. Consequently it dried up, and virtually expired with the Bridgewater Treatises.

To return, however, to the Duke of Argyll. "Neither mechanical aggregation, nor mechanical segregation, can possibly account for the building up of organic tissues." Who has said they did? The Duke has entirely misunderstood the matter. Prof. Lankester never suggested that it was possible to put so much protoplasm into a vessel, and shake out a cockatoo or a guinea-pig at choice. His image of the kaleidoscope had nothing to do with the building up of organisms, only with the varied combination of the elements known to take part in the formation of the fertilized ova from which organisms originate.

I am not sure that I perfectly comprehend what follows. Perhaps some further emendation than that already published is needed in one of the sentences. But it seems evident that the Duke is re-stating his old doctrine of "prophetic germs." He has already defined what he means by these (NATURE, vol. xxxviii. p. 564). "All organs," he says, "do actually pass through rudimentary stages in which actual use is impossible." Here, again, as in the case of the transmission of acquired characters, what one wants is not a reiteration of the assertion, but some definite observed evidence. For the production of this, if only in a single instance, Prof. Lankester pressed the Duke more than a year ago (NATURE, *l.c.* p. 588). None, however, has as yet been forthcoming; and it appears to me that it is not permissible to persist in statements for which he does not attempt to offer a shadow of proof.

The Duke exults in a very amazing fashion over what he strangely calls Prof. Lankester's admission that "natural selection cannot account for the pre-existence of the structures which are prescribed for its choice." I am afraid I have already trespassed on your space too much with quotations; but I have done so in order to show, in some measure at any rate, what is the consensus of opinions amongst students of Darwinism; and I must answer the Duke with one more from Prof. Huxley's admirable biography. It is true that the Royal Society publishes these things in the least attractive way possible; but this particular paper could hardly have escaped attention, as it won the notice and admiration of even a journal so little occupied with scientific discussion as *Truth*.

"There is another sense, however, in which it is equally true that selection originates nothing. 'Unless profitable variations . . . occur, natural selection can do nothing' ('Origin,' ed. 1, p. 82). 'Nothing can be effected unless favourable variations occur' (*l.c.*, p. 108). 'What applies to one animal will apply throughout time to all animals—that is, if they vary—for otherwise natural selection can do nothing. So it will be with plants' (*l.c.* p. 113). Strictly speaking, therefore, the origin of species in general lies in variation; while the origin of any particular species lies, firstly, in the occurrence, and, secondly, in the selection and preservation of a particular variation. Clearness on this head will relieve one from the necessity of attending to the fallacious assertion that natural selection is a *deus ex machinâ*, or occult agency."

And the Duke says he has been waiting for this for thirty years. One can only wonder what Darwinian literature has been the subject of his studies during that time.

W. T. THISELTON DYER.

Royal Gardens, Kew, January 6.

The Microseismic Vibration of the Earth's Crust.

IN Mr. White's article on British earthquakes (NATURE, Jan. 2, p. 202) he refers to me as having *discovered* the microseismic

vibration of the earth's crust. My brother Horace and I were, we believe, the first to verify in England the observations of Bertelli, Rossi, d'Abbadie, and the other (principally Italian) pioneers in this interesting subject.

In our Reports to the British Association for 1881 and 1882 on "The Lunar Disturbance of Gravity," some account will be found of the earlier literature on the subject.

January 9. G. H. DARWIN.

Meteor.

ON Sunday, 12th inst., about 8.10 p.m., a bright meteor was seen here, coming into view near δ Aurigæ. It was of a reddish colour, moved slowly, leaving a short tail, and burst above ϵ Leonis, then with diminished light continued its course to the horizon. T. W. MORTON.

Beaumont College, Old Windsor, January 13.

MAGNETISM.[1]

I.

AS old as any part of electrical science is the knowledge that a needle or bar of steel which has been touched with a loadstone will point to the north. Long before the first experiments of Galvani and Volta the general properties of steel magnets had been observed—how like poles repelled each other, and unlike attracted each other; how the parts of a broken magnet were each complete magnets with a pair of poles. The general character of the earth's magnetism has long been known—that the earth behaves with regard to magnets as though it had two magnetic poles respectively near the rotative poles, and that these poles have a slow secular motion. For many years the earth's magnetism has been the subject of careful study by the most powerful minds. Gauss organized a staff of voluntary observers, and applied his unsurpassed powers of mathematical analysis to obtaining from their results all that could be learned.

The magnetism of iron ships is of so much importance in navigation that a good deal of the time of men of great power has been devoted to its study. It was *the* scientific study of Archibald Smith; and Airy and Thomson have added not a little to our practical knowledge of the disturbance of the compass by the iron of the ship. Sir W. Thomson, in addition to much valuable practical work on the compass, and experimental work on magnetism, has given the most complete and elegant mathematical theory of the subject. Of late years the development of the dynamo machine has directed attention to the magnetization of iron from a different point of view, and a very great deal has been done by many workers to ascertain the facts regarding the magnetic properties of iron. The upshot of these many years of study by practical men interested in the mariner's compass or in dynamo machines by theoretical men interested in looking into the nature of things, is, that although we know a great many facts about magnetism, and a great deal about the relation of these facts to each other, we are as ignorant as ever we were as to any reason why the earth is a magnet, as to why its magnetic poles are in slow motion in relation to its substance, or as to why iron, nickel, and cobalt are magnetic, and nothing else, so far as we know, is to any practical extent. In most branches of science the more facts we know the more fully we recognize a continuity in virtue of which we see the same property running through all the various forms of matter. It is not so in magnetism; here the more we know the more remarkably exceptional does the property appear, the less chance does there seem to be of resolving it into anything else. It seems to me that I cannot better occupy the present occasion than by recalling your attention to, and inviting discussion of, some of those salient properties of magnetism as exhibited by iron, nickel, and cobalt—properties most of them very familiar, but properties which any theory of magnetism must reckon with and explain. We shall not touch on the great subject of the earth as a magnet—though much has been recently done, particularly by Rücker and Thorpe—but deal simply with magnetism as a property of these three bodies, and consider its natural history, and how it varies with the varying condition of the material.

To fix our ideas, let us consider, then, a ring of uniform section of any convenient area and diameter. Let us suppose this ring to be wound with copper wire, the convolutions being insulated. Over the copper wire let us suppose that a second wire is wound, also insulated, the coils of each wire being arranged as are the coils of any ordinary modern transformer. Let us suppose that the ends of the inner coil, which we will call the secondary coil, are connected to a ballistic galvanometer; and that the ends of the outer coil, called the primary, are connected, through a key for reversing the current, with a battery. If the current in the primary coil is reversed, the galvanometer needle is observed to receive a sudden or impulsive deflection, indicating that for a short time an electromotive force has been acting on the secondary coil. If the resistance of the secondary circuit is varied, the sudden deflection of the galvanometer needle varies inversely as the resistance. With constant resistance of the secondary circuit the deflection varies as the number of convolutions in the secondary circuit. If the ring upon which the coils of copper wire are wound is made of wood or glass—or, indeed, of 99 out of every 100 substances which could be proposed—we should find that for a given current in the primary coil the deflection of the galvanometer in the secondary circuit is substantially the same. The ring may be of copper, of gold, of wood, or glass—it may be solid or it may be hollow—it makes no difference in the deflection of the galvanometer. We find, further, that with the vast majority of substances the deflection of the galvanometer in the secondary circuit is proportional to the current in the primary circuit. If, however, the ring be of soft iron, we find that the conditions are enormously different. In the first place, the deflections of the galvanometer are very many times as great as if the ring were made of glass, or copper, or wood. In the second place, the deflections on the galvanometer in the secondary circuit are not proportional to the current in the primary circuit; but as the current in the primary circuit is step by step increased we find that the galvanometer deflections increase somewhat, as is illustrated in the accompanying curve (Fig. 1), in which the abscissæ are proportional to the primary current, and the ordinates are proportional to the galvanometer deflections. You observe that as the primary current is increased the galvanometer deflection increases at first at a certain rate; as the primary current attains a certain value the rate at which the deflection increases therewith is rapidly increased, as shown in the upward turn of the curve. This rate of increase is maintained for a time, but only for a time. When the primary current attains a certain value the curve bends downward, indicating that the deflections of the galvanometer are now increasing less rapidly as the primary current is increased; if the primary current be still continually increased, the galvanometer deflections increase less and less rapidly.

Now what I want to particularly impress upon you is the enormous difference which exists between soft iron on the one hand, and ordinary substances on the other. On this diagram I have taken the galvanometer deflections to the same scale for iron, and for such substances as glass or wood. You see that the deflections in the case of glass or wood, to the same scale, are so small as to be absolutely inappreciable, whilst the deflection for iron at one point of the curve is something like 2000 times as

[1] Inaugural Address delivered before the Institution of Electrical Engineers, on Thursday, January 9, by J. Hopkinson, M.A., D.Sc., F.R.S., President.

great as for non-magnetic substances. This extraordinary property is possessed by only two other substances besides iron—cobalt and nickel. On the same figure are curves showing on the same scale what would be the deflections for cobalt and nickel, taken from Prof. Rowlands's paper. You observe that they show the same general characteristics as iron, but in a rather less degree. Still, it is obvious that these substances may be broadly classed with iron in contradistinction to the great mass of other bodies. On the other hand, diamagnetic bodies belong distinctly to the other class. If the deflection with a non-magnetic ring be unity, that with iron, as already stated, may be as much as 2000; that with bismuth, the most powerful diamagnetic known, is 0·999825—a quantity differing very little from unity. Note, then, the first fact which any theory of magnetism has to explain is: Iron, nickel, and cobalt, all enormously magnetic; other substances practically non-magnetic. A second fact is: With most bodies the action of the primary current on the secondary circuit is strictly proportional to the primary current; with magnetic bodies it is by no means so.

You will observe that the ordinates in these curves, which are proportional to the kicks or elongations of the galvanometer, are called induction, and that the abscissæ are called magnetizing force. Let us see a little more precisely what we mean by the terms, and what are the units of measurement taken. The elongation of the galvanometer measures an impulsive electromotive force—an electromotive force acting for a very short time. Charge a condenser to a known potential, and discharge it through the galvanometer: the needle of the galvanometer will swing aside through a number of divisions proportional to the quantity of electricity in the condenser—that is, to the capacity and the potential. From this we may calculate the quantity of electricity required to give a unit elongation. Multiply this by the actual resistance of the secondary circuit and we have the impulsive electromotive force in volts and seconds, which will, in the particular secondary circuit, give a unit elongation. We must multiply this by 10^8 to have it in absolute C.G.S. units. Now the induction is the impulsive electromotive force in absolute C.G.S. units divided by the number of secondary coils and by the area of section of the ring in square centimetres. The line integral of magnetizing force is the current in the primary in absolute C.G.S. units—that is, one-tenth of the current in amperes—multiplied by 4π. The magnetizing force is the line integral divided

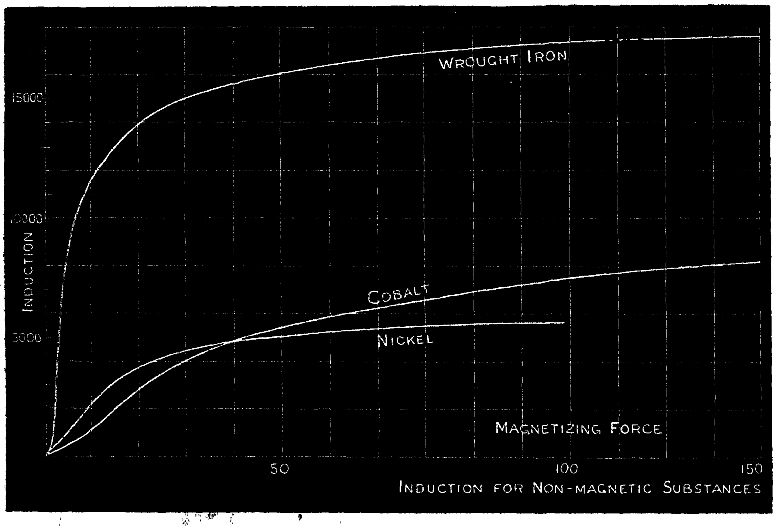


Fig. 1.

by the length of the line over which that line integral is distributed. This is, in truth, not exactly the same for all points of the section of the ring—an imperfection so far as it goes in the ring method of experiment. The absolute electro-magnetic C.G.S. units have been so chosen that if the ring be perfectly non-magnetic the induction is equal to the magnetizing force. We may refer later to the permeability, as Sir W. Thomson calls it; it is the ratio of the induction to the magnetizing force causing it, and is usually denoted by μ.

There is a further difference between the limited class of magnetic bodies and the great class which are non-magnetic. To show this, we may suppose our experiment with the ring to be varied in one or other of two or three different ways. To fix our ideas, let us suppose that the secondary coil is collected in one part of the ring, which, provided that the number of turns in the secondary is maintained the same, will make no difference in the result in the galvanometer. Let us suppose, further, that the ring is divided so that its parts may be plucked from together, and the secondary coil entirely withdrawn from the ring. If now the primary current have a certain value, and if the ring be plucked apart and the secondary coil withdrawn, we shall find that, whatever be the substance of which the ring is composed, the galvanometer deflection is one-half of what it would have been if the primary current had been reversed. I should perhaps say approximately one-half, as it is not quite strictly the case in some samples of steel, although, broadly speaking, it is one-half. This is natural enough, for the exciting cause is reduced from—let us call it a positive value, to nothing when the secondary coil is withdrawn; it is changed from a positive value to an equal and opposite negative value when the primary current is reversed. Now comes the third characteristic difference between the magnetic bodies and the non-magnetic. Suppose that, instead of plucking the ring apart when the current had a certain value, the current was raised to this value and then gradually diminished to nothing, and that then the ring was plucked apart and the secondary coil withdrawn. If the ring be non-magnetic, we find that there is no deflection of the galvanometer; but, on the other hand, if the ring be of iron, we find a very large deflection, amounting, it may be, to 80 or 90 per cent. of the deflection caused by the withdrawal of the coil when the current had its full value. Whatever be the property that the passing of the primary current has imparted to the iron, it is clear that the iron

retains a large part of this property after the current has ceased. We may push the experiment a stage further. Suppose that the current in the primary is raised to a great value, and is then slowly diminished to a smaller value, and that the ring is opened and the secondary coil withdrawn. With most substances we find that the galvanometer deflection is precisely the same as if the current had been simply raised to its final value. It is not so with iron: the galvanometer deflection depends not alone upon the current at the moment of withdrawal, but on the current to which the ring has been previously subjected. We may then draw another curve (Fig. 2) representing the galvanometer deflections produced when the current has been raised to a high value and has been subsequently reduced to a value indicated by the abscissæ. This curve may be properly called a descending curve. In the case of ordinary bodies this curve is a straight line coincident with the straight line of the ascending curve, but for iron is a curve such as is represented in the drawing. You observe that this curve descends to nothing like zero when the current is reduced to zero; and that when the current is not only diminished to zero, but is reversed, the galvanometer deflection only becomes zero when the reversed current has a substantial value. This property possessed by magnetic bodies of retaining that which is impressed upon them by the primary current has been called by Prof. Ewing "hysteresis," or, as similar properties have been observed in quite other connections, "magnetic hysteresis." The name is a good one, and has been adopted. Broadly speaking, the induction as measured by the galvanometer deflection is independent of the time during which the successive currents have acted, and depends only upon their magnitude and order of succession. Some recent experiments of Prof. Ewing, however, seem to show a well-marked time effect. There are curious features in these experiments which require more elucidation.

It has been pointed out by Warburg, and subsequently by Ewing, that the area of curve 2 is a measure of the quantity of energy expended in changing the magnetism of the mass of iron from that produced by the current in one direction to that produced by the current in the opposite direction and back again. The energy expended with varying amplitude of magnetizing forces has been determined for iron, and also for large magnetizing forces for a considerable variety of samples of steel. Different sorts of iron and steel differ from each other very greatly in this respect. For example, the energy lost in a complete cycle of reversals in a sample of Whitworth's mild steel was about 10,000 ergs per cubic centimetre; in oil-

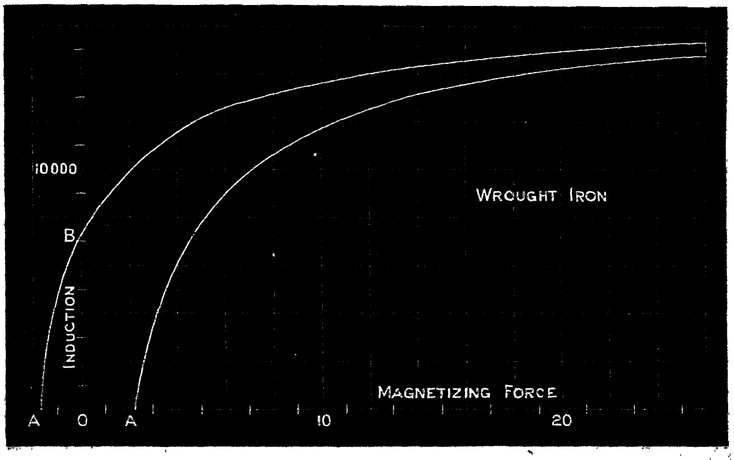


FIG. 2.

hardened hard steel it was near 100,000; and in tungsten steel it was near 200,000—a range of variation of 20 to 1. It is, of course, of the greatest possible importance to keep this quantity low in the case of armatures of dynamos, and in that of the cores of transformers. If the armature of a dynamo machine be made of good iron, the loss from hysteresis may easily be less than 1 per cent; if, however, to take an extreme case, it were made of tungsten steel, it would readily amount to 20 per cent. In the case of transformers and alternate-current dynamo machines, where the number of reversals per second is great, the loss of power by hysteresis of the iron, and the consequent heating, become very important. The loss of power by hysteresis increases more rapidly than does the induction. Hence it is not well in such machines to work the iron to anything like the same intensity of induction as is desirable in ordinary continuous current machines. The quantity O A, when measured in proper units, as already explained—that is to say, the reversed magnetic force, which just suffices to reduce the induction as measured by the kick on the galvanometer to nothing after the material has been submitted to a very great magnetizing force—is called the "coercive force," giving a definite meaning to a term which has long been used in a somewhat indefinite sense. The quantity is really the important one in judging the magnetism of short permanent magnets. The residual magnetism, O B, is then practically of no interest at all; the magnetic moment depends almost entirely upon the coercive force. The range of magnitude is somewhat greater than in the case of the energy dissipated in a complete reversal. For very soft iron the coercive force is 1·6 C.G.S. units; for tungsten steel, the most suitable material for magnets, it is 51 in the same units. A very good guess may be made of the amount of coercive force in a sample of iron or steel by the form of the ascending curve, determined as I described at first. This is readily seen by inspection of Fig. 3, which shows the curves in the cases of wrought iron, and steel containing 0·9 per cent. of carbon. With the wrought iron a rapid ascent of the ascending curve is made, when the magnetizing force is small and the coercive force is small; in the case of the hard steel the ascent of the curve is made with a larger magnetizing current, and the coercive force is large. There is one curious feature shown in the curve for hard steel which may, so far as I know, be observed in all magnetizable substances: the ascending curve twice cuts the descending curve, as at M and N. This peculiarity was, so far as I know, first observed by Prof. G. Wiedemann.

I have already called emphatic attention to the fact that magnetic substances are enormously magnetic, and that non-magnetic substances are hardly at all magnetic:

there is between the two classes no intermediate class. The magnetic property of iron is exceedingly easily destroyed. If iron be alloyed with 12 per cent. of manganese, the kick on the galvanometer which the material will give, if made into a ring, is only about 25 per cent. greater than is the case with the most completely non-magnetic material, instead of being some hundreds of times as great, as would be the case with iron. Further, with this manganese steel, the kick on the galvanometer is strictly proportional to the magnetizing current in the primary, and the material shows no sign of hysteresis. In short, all its properties would be fully accounted for if we supposed that manganese steel consisted of a perfectly non-magnetic material, with a small percentage of metallic iron mechanically admixed therewith. Thus the property of non-magnetizability of manganese steel is an excellent proof of the fact—which is also shown by the non-magnetic properties of most compounds of iron—that the property appertains to the molecule, and not to the atom; or, to put it in another way, suppose that we were to imagine manganese steel broken up into small particles, as these particles became smaller there would at length arrive a point at which the iron and the manganese would be entirely separated from each other: when this point is reached the particles of iron are non-magnetic. By the magnetic molecule of the substance we mean the smallest part which has all the magnetic properties of the mass. The magnetic molecule must be big enough to contain its proportion of manganese. In iron, then, we must have a collection of particles of such magnitude that it would be possible for the manganese to enter into each of them, to constitute an element of the magnet. Manganese is, so far as I know, a non-magnetic element. Smaller proportions of manganese reduce the magnetic property in a somewhat less degree, the reduction being greater as the quantity of manganese is greater. It appeared very possible that the non-magnetic property of manganese steel was due to the coercive force being very great—that, in fact, in all experiments we were still on that part of the magnetization curve below the rapid

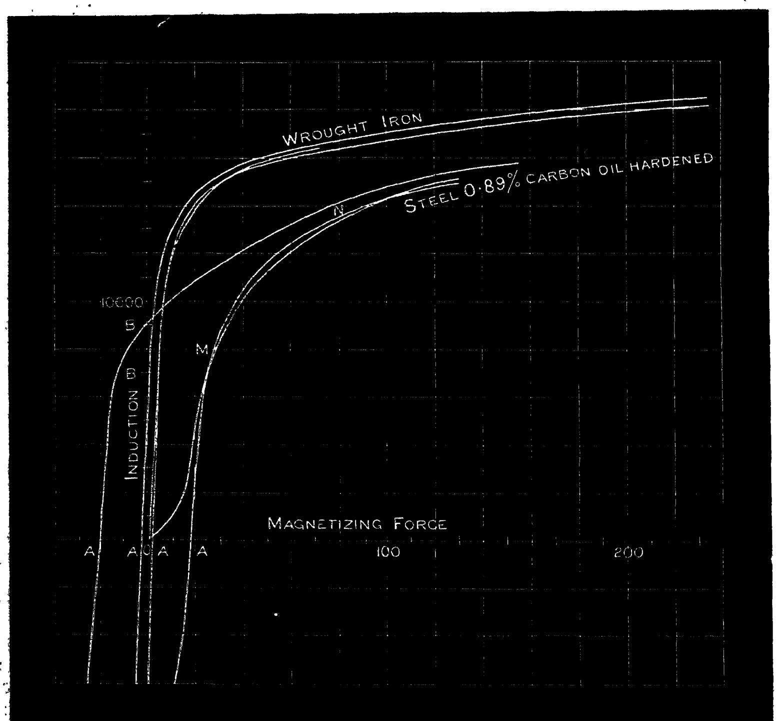


FIG. 3

rise, and that if the steel were submitted to greater forces it would presently prove to be magnetic, like other kinds of steel. Prof. Ewing, however, has submitted manganese steel to very great forces indeed, and finds that its magnetism is always proportional to the magnetizing force.

No single body is known having the property of capacity for magnetism in a degree which is neither very great nor very small, but intermediate between the two extremes. We can, however, mix magnetic and non-magnetic substances to form bodies apparently intermediate. It is, therefore, interesting to consider what the properties might be of such a mixture. It depends quite as much on the way in which the magnetic part is arranged in the mass, as on its actual quantity. Suppose, for example, it is arranged as in Fig. 4—in threads or plates having a very long axis in the direction of the magnetizing force—we may at once determine the curve of magnetization of the mixture from that of the magnetic substance by dividing the induction for any given force in the ratio of the whole volume to the volume of magnetic substance. If, on the other hand, it is as in Fig. 5—with a very short axis in the direction of the force, and a long axis perpendicular thereto—we can equally construct the curve of magnetization. This is done in Fig. 6, which shows the curve when nine-tenths of the material is highly magnetic iron, arranged as in Fig. 5, whilst the other curve of the same figure is that when only one-tenth is magnetic, but arranged as in Fig. 4. You observe how very different is the character of the curve—a difference which is reduced by the much less proportion of magnetic material in the mixture in the one case than in the other. One peculiarity of these arrangements of the two materials in relation to each other is, that the resulting material is not isotropic; that is, its properties are not the same in all directions, but depend upon the direction of the magnetizing force in the material. Of course, this is not at all a probable arrangement but it is instructive in showing the character of the

result as depending upon the construction of the material. Let us, however, consider the simplest isotropic arrangement; let us suppose that one material is in the form of spheres bedded in a matrix of the other: if the spheres are placed at random this is clearly an isotropic arrangement. The result is very different according as the matrix or the spheres are of the magnetic material. Suppose that the volume of the spheres is one-half of the whole volume. In Fig. 7 we have approximately the curve for iron, for a mixture of equal quantities of iron and a non-magnetic material; the spheres being non-magnetic and the matrix iron, and for a mixture, the

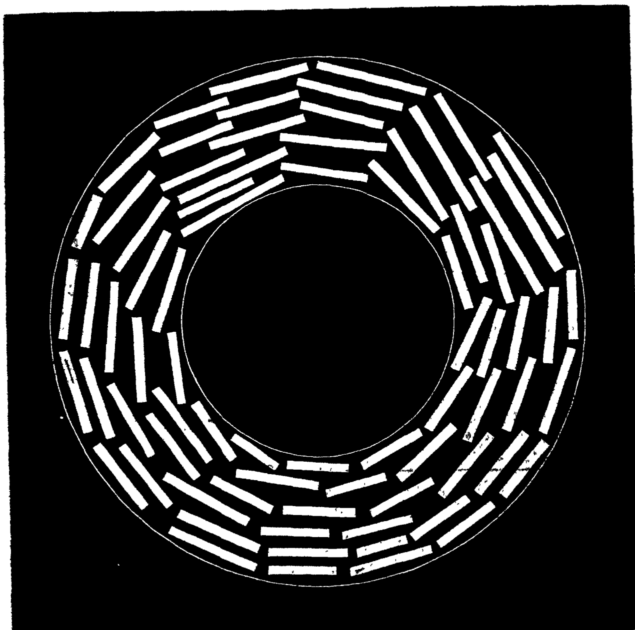


FIG. 4.

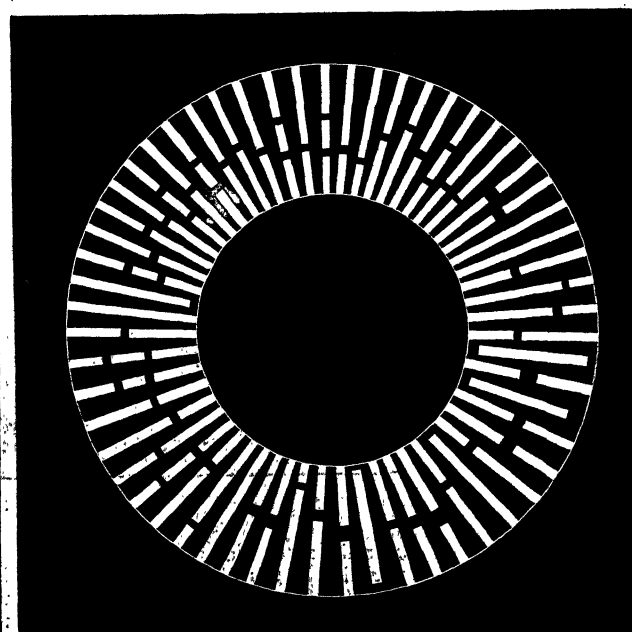


FIG. 5.

spheres being iron and the matrix non-magnetic. Observe the great difference. When the spheres are iron, the induction is near four times the force for all values of the force. When the matrix is iron, the induction is near two-fifths of the induction when the material is iron only.

In speaking of the properties of bodies which, like manganese steel, are slightly magnetic, it may be well here to enter a caution. But little that is instructive is to be learned by testing filings, or the like, with magnets, as these show but little difference between bodies which are slightly magnetic and those which are strongly

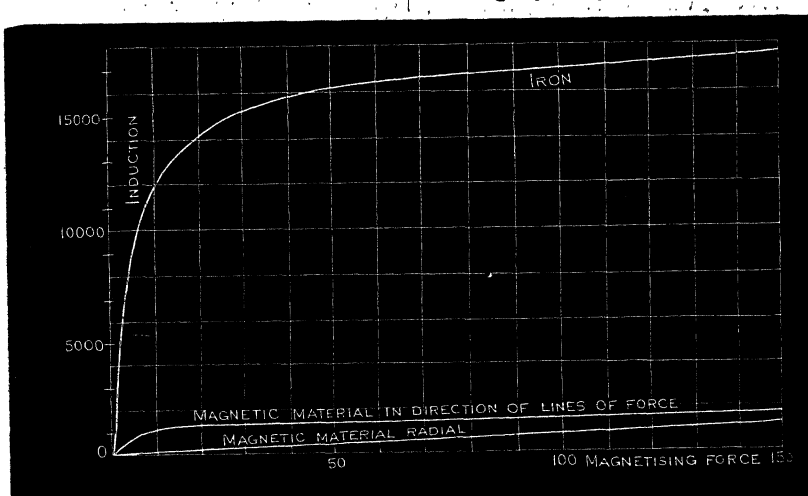


FIG. 6.

magnetic. Suppose the filings to be spheres; in the following table are given comparative values of the forces they would experience in terms of μ, if placed in a magnetic field of given value, μ having its ordinary signification—that is, being the ratio of the kick on the galvanometer when a ring is tried made of the material of the filing to the kick if the ring is made of a perfectly non-magnetic material:—

μ.	Attraction.	
1	0	Non-magnetic body.
1·47	0·18	Manganese steel with 12 per cent.
3·6	1·2	Manganese steel with 9 per cent.
5	1·5	
10	2·1	
100	2·8	
1000	2·98	

Now bodies in which μ is so small as 3·6 belong distinctly to the non-magnetic class; but the test with the magnet would very markedly distinguish them from manganese steel with 12 per cent of manganese. The distinction, however, between $\mu = 3·6$ and $\mu = 1000$ is comparatively small; whereas, under the conditions of experiment, μ is much more than 1000 for most bodies of which iron is the principal constituent.

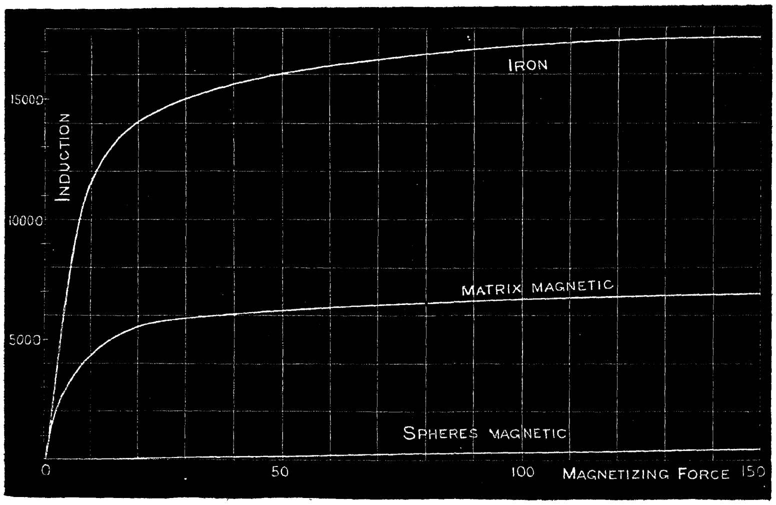


FIG. 7.

The effect of stress on the magnetic properties of iron and nickel have been studied by Sir W. Thomson. A fact interesting from a broad and general point of view is that the effects of stress are different in kind in the case of iron and nickel. In the case of iron, for small magnetizing forces in the direction of the tension, tension increases the magnetization; for large forces, diminishes it. In the case of nickel the effect is always to diminish the magnetization.

(*To be continued.*)

LORENZO RESPIGHI.

DURING the last forty years the Eternal City has possessed two astronomical observatories. It was at the old building, connected with the Collegio Romano, that Scheiner collected the principal materials for his famous work on the sun, called from its dedication to Prince Orsini, the Duke of Bracciano, "Rosa Ursina"; and though it is with some justice that Delambre speaks disparagingly of its contents as compared with its bulk, the observations of the solar spots show with what care they were made, and they afford the first indication of the now familiar fact that their rotation varies in duration in different heliographical latitudes, though Scheiner's idea seems to have been that it was not the same in the two solar hemispheres. But it was not until 1787 that the present observatory of the Collegio Romano was commenced, nor until 1804 that the general interest felt in the great eclipse of February 11 in that year induced Pope Pius VII. to provide G. Calandrelli with the means of furnishing it with suitable instruments. Another astronomical phenomenon, the appearance of the great comet of 1843, led his son Ignazio Calandrelli, to wish to form a new observatory on the Capitoline Hill; but it was not until five years later that Pius IX. was able, in 1848, to provide him with the means for carrying out this design. Meanwhile Calandrelli continued his observations at Bologna, ably assisted by the subject of our notice.

Lorenzo Respighi was born at Cortemaggiore, in the province of Placentia, in 1824. His first studies were made at Parma, from which town he proceeded to the University of Bologna, where he obtained high honours in the departments of mathematics and philosophy in 1847. Nominated Professor of Optics and Astronomy in 1851, he subsequently succeeded Calandrelli as Director of the Observatory. On the retirement of the latter in 1865 (followed by his death in 1866) Respighi was appointed his successor. His earliest papers were on mechanical and optical subjects; but he will be best remembered by his subsequent labours on stellar spectra, on those of the solar corona and protuberances, and on the scintillation of the stars. In 1871 he went on an expedition to Poodocottah, in Hindustan, to observe the total eclipse of December 12 in that year; an account of the observations will be found in the eclipse (41st) volume of the Memoirs of the Royal Astronomical Society, of which Respighi was elected an Associate in 1872. He formed from his observations between 1875 and 1881 a catalogue of 2534 stars in the northern hemisphere from the first to the sixth magnitude, which was published in successive numbers of the Memoirs of the Lincean Academy.

His death took place after a long illness, aggravated by the recent epidemic, on December 10 last, and the Campidoglio Observatory has thus been deprived of its second director, who has so ably and energetically conducted its operations during nearly the last quarter of a century.

W. T. LYNN.

NOTES.

ON Saturday evening, at the Royal Institution, Prof. Max Müller delivered an address to inaugurate the establishment of a school for modern Oriental studies by the Imperial Institute in union with University College and King's College, London. The Prince of Wales presided, and among those present were many eminent persons, including some distinguished Orientals. Prof. Müller presented with admirable force and clearness the need for a great English school for Oriental studies, and had much to tell his hearers as to work done in this direction in other countries. His account of the new Berlin seminary of

Oriental languages was particularly interesting. This institution has the following staff of professors and teachers :—One professor of Chinese ; two teachers of Chinese, both natives—one for teaching North Chinese, the other South Chinese ; one professor of Japanese, assisted by a native teacher ; one professor of Arabic, assisted by two native teachers—one for Arabic as spoken in Egypt, the other for Arabic as spoken in Syria ; one native teacher of Hindustani and Persian ; one native teacher of Turkish ; one teacher of Suaheli, an important language spoken on the East Coast of Africa, assisted by a native. Besides these special lectures, those given by the most eminent professors of Sanskrit, Arabic, Persian, and Chinese in the Universities of Berlin are open to the students of the Oriental seminary. The number of students amounts at present to 115. Of these, 56 are said to belong to the faculty of law, which must be taken to include all who aspire to any employment in the consular and colonial services. Fifteen belong to the faculties of philosophy, medicine, and physical science ; four to the faculty of theology, who are probably intended for missionary work. Twenty-three are mentioned as engaged in mercantile pursuits, three are technical students, five officers in the army, and nine are returned as studying modern Greek and Spanish, languages not generally counted as Oriental, though, no doubt, of great usefulness in the East and in America. Prof. Müller succeeded in conveying a remarkably vivid impression of the fact that England, looking at the subject simply from the point of view of her own material interests, cannot afford to neglect the studies to which so much attention is devoted elsewhere. "England," he said, "cannot live an isolated life. She must be able to breathe, to grow, to expand, if she is to live at all. Her productive power is far too much for herself, too much even for Europe. She must have a wider field for her unceasing activity, and that field is the East, with its many races, its many markets, its many languages. To allow herself to be forestalled or to be ousted by more eloquent and persuasive competitors from those vast fields of commerce would be simple suicide. Our school, in claiming national support, appeals first of all to the instinct of self-preservation. It says to every manufacturing town in England, help us, and, in doing so, help thyself. Whenever the safety and honour of England are at stake we know what enormous sums Parliament is willing to vote for army and navy, for fortresses and harbours —sums larger than any other Parliament would venture to name. We want very little for our School of Oriental Languages, but we want at least as much as other countries devote to the same object. We want it for the very existence of England ; for the vital condition of her existence is her commerce, and the best markets for that commerce lie in the East."

ON Saturday, February 22, the Physikalisch-ökonomische Gesellschaft of Königsberg is to hold its centenary celebration. The proceedings will consist of a Festsitzung at 11 a.m., a visit to the Provinzial-Museum at 1, and a Festessen at 8 p.m.

SEVERAL courses of afternoon lectures which promise to be exceptionally interesting will be delivered during the present season at the Royal Institution. On January 21 Mr. G. J. Romanes, F.R.S., will begin a series of ten lectures, forming the third part of his course on "Before and After Darwin." This series will relate to the post-Darwinian period, and will include a discussion of Weismann's theory of heredity. Prof. Flower, F.R.S., will begin on January 25 a course of three lectures on the natural history of the horse, and of its extinct and existing allies. A course of four lectures on the early developments of the forms of instrumental music will be begun by Mr. F. Niecks on March 6.

THE annual general meeting of the Institution of Mechanical Engineers will be held at 25 Great George Street, Westminster, on January 29, 30, and 31. The chair will be taken each evening by the President at 7.30 p.m. The following are the papers: on the compounding of locomotives burning petroleum refuse in Russia, by Thomas Urquhart ; on the burning of colonial coal in the locomotives of the Cape Government railways, by Michael Stephens ; on the mechanical appliances employed in the manufacture and storage of oxygen, by Kenneth S. Murray.

THE annual general meeting of the Anthropological Institute of Great Britain and Ireland will take place on Tuesday, the 28th inst., at 8.30 p.m., Dr. John Beddoe, F.R.S., President, in the chair. The following will be the order of business :— Confirmation of the minutes, appointment of scrutineers of the ballot, Treasurer's financial statement, Report of Council for 1889, the Presidential Address, report of scrutineers, and election of Council for 1890.

DURING the last few years anthropological studies have excited a good deal of popular interest, and lately it occurred to the Council of the Anthropological Institute that it might be worth while for them to arrange for the preparation of a series of lectures presenting clearly the results of recent anthropological research. Accordingly a course on the following branches of the subject has been planned : physical anthropology ; the geological history of man ; prehistoric and non-historic dwellings, tombs, and ornaments ; the development of the arts of life ; social institutions ; anthropometry. The Assistant-Secretary of the Institute is prepared to arrange for the delivery of these lectures at places within convenient distance of London.

THE first volume of Prof. Thorpe's "Dictionary of Applied Chemistry" (Longmans) will be published in a few days. The work will consist of three volumes, and will treat specially of chemistry in its relations to the arts and manufactures. It will be uniform with the new edition of Watts's "Dictionary of Chemistry," edited by Muir and Morley.

M. GRANEL has been appointed Professor of Botany to the Faculty of Medicine at Montpellier.

ON Monday the Khedive opened the new Museum at Ghizeh, whither the archæological treasures hitherto preserved at Boulak have been transferred.

THE "tercentenary of the invention of the compound microscope" will be celebrated by a Universal Exhibition of Botany and Microscopy, to be held at Antwerp during the present year, under the auspices of M. Ch. de Bosschere, President, M. Ch. Van Geert, Secretary, and Dr. H. Van Heurck, Vice-President. It is proposed to organize an historical exhibition of microscopes, and an exhibition of the instruments of all makers, and of accessory apparatus and photomicrography. At the conferences the following subjects will be discussed and illustrated :—The history of the microscope ; the use of the microscope ; the projecting microscope and photomicrography ; the microscopical structure of plants ; the microscopical structure of man and of animals ; microbes ; the adulteration of food-substances, &c. Communications are to be addressed to M. Ch. de Bosschere, Lierre, Belgium.

WE regret to have to record the death of Mr. Daniel Adamson, well known from his connection with the iron and steel industries. He died on Monday at the age of 71. Mr. Adamson was President of the Iron and Steel Institute in 1887, and was a member of other mechanical and scientific associations.

DR. F. HAUCK, the eminent algologist, died at Trieste on December 21, 1889, at the early age of forty-four. He was the author of the volume on marine Algæ in the new edition of Rabenhorst's "Cryptogamic Flora of Germany."

THE December number of the *American Geologist* contains an interesting paper, by William Upham, on the late Prof. Henry Carvill Lewis, who, it will be remembered, died at Manchester on July 21, 1888, a day or two after his arrival in this country from America. He became ill during the voyage,

and it seems that the immediate cause was the contamination of the water supply of Philadelphia, where he had been living, and where about a thousand cases of typhoid fever appeared at nearly the same time. Prof. Lewis was only in his thirty-fifth year. An excellent portrait of him accompanies Mr. Upham's paper.

AT the meeting of the University Experimental Science Association, Dublin, on December 13, Mr. J. Joly read a paper on a resonance method of measuring the constant of gravitation. A simple pendulum of small mass is hung in a tall glass tube, rendered vacuous. In close proximity two massive pendulums, one at either side, are maintained in a state of vibration for any desired period of time. The times of vibration of all these pendulums are alike. The observations consist in observing the amplitude, or the increase of amplitude, of the central pendulum, after a known number of vibrations executed by the exterior pendulums. Several modifications, carrying out the same principle, were suggested. It is proposed to test the method in the vaults of the physical laboratory.

THE Central Meteorological Observatory of Mexico, which is situated at 7489 feet above the sea, has published a summary of meteorological results for each month of twelve years ending 1888 (excepting January and February 1877). The coldest month is January, the mean temperature of which is 54°, and the warmest month is April, the mean temperature of which is 64°. The absolute maximum in the shade was 89°, and the minimum 28°·9. The wettest month is August, in which the mean rainfall is 5·4 inches, and the driest month is February, with an average of 0·4 inch. The greatest fall at one time was 2·5 inches. The prevalent direction of the wind is north-west.

THE *Essex County Chronicle* of January 10 says that on Tuesday, the 7th inst., two slight shocks of earthquake were noticed at Chelmsford. The first occurred at 12.30, when a low rumbling sound like thunder in the distance was heard, accompanied by a vibration of the ground and a rattling of the windows. The shock was observed in several parts of the town. The more pronounced shock was, however, at 1.25 p.m., when the rumbling, moaning sound was intensified, there being a heavy throbbing in the air like the pulsation of an engine. At many houses there was a violent shaking of the windows, and two cases are reported of things trembling on the tables. Some men working for Mr. Norrington heard the sound, took it to be the rumble of a heavy waggon, and went out to see it. Nothing was in sight. Several people recognized the shock as being similar to the forerunner of the 1884 earthquake, and rushed out of their houses. Mr. Arthur E. Brown, writing to us from Brentwood, says that the shocks were noticed there. They were attributed by the people in his house to the firing of guns at Woolwich. They rattled the doors violently.

A CORRESPONDENT writes that during the thunderstorm which prevailed over the greater part of Scotland early on Monday morning, January 6, a slight shock of earthquake was felt in a district of Perthshire. "This," he says, "is somewhat similar to what took place at Argyll on the evening of July 15 last year, and might lead one to suppose that atmospheric influence has something to do with the production of seismic disturbances."

AT a meeting of the Royal Botanic Society on Saturday, attention was called to a specimen of the double cocoanut, or *cocoa de mer*, now known to come from the Seychelles. For some hundreds of years these nuts have been occasionally found washed up by the sea, and their extraordinary appearance, large size, and mysterious origin have given rise to many stories of miraculous virtue in the cure of diseases. Some are even said to have been sold for their weight in gold. This specimen belonged to General Gordon, and was given by him to General Gerald Graham, by whom it has been presented to the Society.

THE Transactions of the Congrès pour l'Utilisation des Eaux fluviales, held last summer in Paris, have just been issued. The volume contains a great number of engravings.

A BOOK on the Congo State, by E. Dupont, the Director of the Natural History Museum of Brussels, has just been published. He presents the scientific results of his travels, devoting especial attention to geological questions.

MESSRS. GEORGE PHILIP AND SON have published the second issue of their valuable "Educational Annual." The work has been enlarged, revised, and to some extent rearranged; and it ought to be of great service to all who are for any reason especially interested in educational institutions.

MESSRS. PERKEN, SON, AND RAYMENT have produced a projecting optical lantern, which is likely to be of considerable service. When enlargements are required, a condenser of 10-inch diameter is available; but when a magic-lantern entertainment is to be provided, a condenser of 4-inch diameter can be substituted. The apparatus consists of a mahogany-body lantern with a long bellows-camera adjusted by the patent quick-action rack and pinion, and lighted by the refulgent three-wick lamp.

ON January 21, and the three following evenings, Dr. E. Symes Thomson will deliver, at Gresham College, a course of lectures on influenza or epidemic catarrh. In the first lecture he will present a historical sketch of the subject. The remaining lectures will be on influenza as it affects the lower animals, the causes and consequences of influenza, and diagnosis and management.

THE additions to the Zoological Society's Gardens during the past week include four Leopard Tortoises (*Testudo pardalis*), three Well-marked Tortoises (*Homopus signatus*), a Rufous Snake (*Ablabes rufulus*), six Gray's Frogs (*Rana grayi*) from South Africa, presented by the Rev. G. H. R. Fisk, C.M.Z.S.; two Spur-winged Geese (*Plectropterus gambensis*) from West Africa, presented by Mr. C. B. Mitford; six Red-bellied Waxbills (*Estrelda rubriventris*), five Crimson-eared Waxbills (*Estrelda phœnicotis*), seven Grenadier Waxbills (*Uræginthus grantinus*, 6 ♂ 1 ♀), three Paradise Whydah Birds (*Vidua paradisea*), three —— Weaver Birds (*Euplectes* ——) from Benguela, West Africa, presented by Mr. T. W. Bacon; a Bluish Finch (*Spermophila cærulescens* ♂) from Brazil, presented by Mrs. Mayne; a Green Turtle (*Chelone viridis*) from the West Indies, presented by Mrs. Harris; a Chattering Lory (*Lorius garrulus*) from Moluccas, presented by Captain Bason, P. and O. s.s. *Bombay*; three Yellow-winged Sugar Birds (*Cœrea cyanea*), two Yellow-fronted Tanagers (*Euphonia flavifrons*) from South America, deposited; four Tufted Umbres (*Scopus umbretta*) from Africa, a Geoffroy's Terrapin (*Hydraspis hilarii*) from the Argentine Republic, purchased; a Koala (*Phascolarctus cinereus* ♀) from Australia, two Indian Cobras (*Naia tripudians*), an Indian Python (*Python molurus*) from India, received in exchange.

OUR ASTRONOMICAL COLUMN.

OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at Greenwich at 10 p.m., January 16 = 5h. 45m. 8s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G.C. 1185	—	—	5 30 7	− 5 20
(2) 119 Tauri	4	Reddish-yellow.	5 25 46	+18 31
(3) σ Orionis	4	Whitish-yellow.	5 33 12	− 2 40
(4) γ Orionis	2	White.	5 19 12	+ 6 15
(5) 64 Schj.	8	Very red.	5 38 29	+24 22
(6) R Ceti	Var	Yellowish-red.	2 20 24	− 0 40
(7) U Ceti	Var.	Reddish.	2 28 26	−13 37

Remarks.

(1) This is described in Herschel's general catalogue as "a remarkable object, very large, round, with tail, much brighter in the middle." The spectrum has not yet been recorded, but it promises to be one of great interest, as the nebula is apparently one of the cometic ones. The meteoritic hypothesis suggests that these are produced by a condensed swarm moving at a high velocity through a sheet of meteorites at rest, or a swarm almost at rest surrounded by a moving sheet. In the former case the collision region would be behind the swarm, and would be spread out like a comet's tail, the angle of the fan and length of "tail" depending upon the velocity of the moving swarm. Observations for variations of spectrum between nucleus and tail will also be valuable.

(2) This is a typical example of stars of Group II. Observations similar to those suggested for 20 Leporis, U.A., last week, are required.

(3) Konkoly classes this with stars of the solar type. The usual differential observations, as to whether the star belongs to Group III. or to Group V., are required.

(4) In Gothard's list of star spectra this is described as Group IV. The usual observations are suggested.

(5) Dunér describes the spectrum of this star as Group VI., but his description is not complete. The characters of the different bands, especially of Band 6, require further observation. It may be remarked in connection with these stars of small magnitude, that the observations are by no means so difficult as in the case of small stars with spectra consisting of fine lines. The bands are broad and generally dark, so that the continuous spectrum is broken up into zones.

(6) This variable has a period of 167 days, and ranges in magnitude from about 8 at maximum to 13 at minimum. The spectrum is of the Group II. type, and, as in other variables of the same group, bright lines may appear at maximum. Dunér states that the bands are very wide and dark, but he does not state what bands are present. Maximum on January 18.

(7) The spectrum of this variable has not yet been recorded, but the colour indicates that it is probably either Group II. or Group VI. The period is 228 days, and the range from 7 at maximum to 10 at minimum. The maximum will occur on January 18.

A. FOWLER.

THE TEMPERATURE OF THE MOON.—Prof. Langley, by means of the bolometer, made some measurements of the heat from different parts of the eclipsed moon on the night of September 23, 1885 (*Phil. Mag.*, January, 1890). These measurements were made in connection with a much more extended study on the temperature of our satellite. The following particulars are given:—The diameter of the lunar image was 28·3 millimetres, and of this only a limited portion (0·08 of the whole) fell upon the bolometer. As the penumbra came on, the diminution of heat was marked, being measured by the bolometer even before the eye had detected any appearance of shadow. The heat continued to diminish rapidly with the progress of the immersion in the penumbra. At one hour before the middle of the total eclipse, the deflection in the umbra was 3·8 divisions. Fifty minutes after the middle of the eclipse, it had diminished to approximately 1·3 divisions, this being less than 1 per cent. of the heat from a similar portion of the uneclipsed moon. The rise of the temperature after the passage of the umbra was apparently nearly as rapid as the previous fall. The most important conclusion drawn by Prof. Langley from his researches is that the mean temperature of the sunlit lunar soil is most probably not greatly above zero Centigrade.

ON THE ORBIT OF STRUVE 228.—The *Monthly Notices* of the Royal Astronomical Society, December 1889, contains a note, communicated by Mr. J. E. Gore, on this binary star. Recent measures show that, since Struve discovered the star in 1829, it has described about 120° of its apparent orbit. The following provisional elements have been computed:—

Elements of Σ 228.

P = 88·73 years.	☊ = 84° 49′
T = 1906·03	λ = 51 36
e = 0·5311	a = 0″·98
i = 70° 59′	μ = +4°·057

According to this orbit, the distance between the components will gradually increase during the next few years up to a maximum of about 0″·55, and then diminish again as the companion approaches the periastron. The minimum distance will not be reached until the position angle is 180° (after the periastron passage), when the components will probably be separated by less than 0″·2. The binary lies a little preceding 62 Andromedæ, the position for 1890·0 being approximately—

R.A. 2h. 6m. 59s., Decl. + 46° 58′·4.

The magnitudes of the components are about 6·7 and 7·6.

ORBIT OF SWIFT'S COMET (V. 1880).—The orbit of this comet has been computed, by Gibbs's vector method, by Messrs. W. Beebe and A. W. Phillips (*Astr. Journ.*, Nos. 207, 208). This method is found to possess advantages over those of Gauss and Oppolzer. Below are given elements which have been computed from eight observations ranging from October 25, 1880, to January 7, 1881, and compared with these are the elements computed from three observations by Gibbs's method. Both are referred to the ecliptic and mean equinox of 1880·0:—

Eight observations.	Three observations.
i = 5° 23′ 3″·8	i = 5° 22′ 2″·03
π − ☊ = 106 13 4·1	π − ☊ = 106 13 19·17
☊ = 296 42 55·1	☊ = 296 52 2·09
log e = 9·8163726	log e = 9·8146985
log a = 0·4905937	log a = 0·4873065
T = 1880 Nov. 7·786610	T = 1880 Nov. 7·782810
Periodic time = 1988·33 days.	Periodic time = 1965·88 days.

ON THE VARIABILITY OF R VULPECULÆ.—Schönfeld, from a discussion of the observations from 1859 to 1874, found that a uniform period left systematic deviations outstanding which exceeded seven or eight times the uncertainty of the single maxima, but that a quadratic term, corresponding to a shortening of 0·12 days from epoch to epoch, brought them within the range of the probable errors. The divergence from observation, however, soon began, and rapidly widened, until in 1885 it amounted to 106·5 days. Mr. Chandler (*Astr. Journ.*, No. 208) gives a table showing the maxima and minima observed since 1807, with the deviations from the elements of his catalogue. It is seen that, whereas the difference between the observed and the calculated maxima and minima, using Schönfeld's elements, are very considerable, the elements given by the author differed from those observed only in a very slight degree.

ON THE ROTATION OF MERCURY.—Nearly a century has elapsed since Schröter published his first observation of the physical aspect of Mercury, and assigned to the planet a period of rotation; but it has been left to that perspicacious observer, Signor Schiaparelli, to demonstrate the fact by a series of remarkable observations given by him in *Astronomische Nachrichten*, No. 2944. The observations extend from 1882 to the end of last year. As many as 150 drawings have been made of the markings upon the planet with respect to the best positions for observation. It is noted that one of the finest drawings was made on August 11, 1882, when Mercury was only 3° 2′ from the sun's limb. The markings that are visible on Mercury when observed at the same hour on consecutive days are identical in their aspect, and this being so, three hypotheses have been propounded (*Astr. Nach.*, 2479) regarding the rotation of the planet, viz.:—

That (1) the time of rotation is about 24 hours.

(2) The planet makes two or more rotations in the same interval.

(3) The time of rotation is so slow as to be inappreciable when observing the markings during a few days.

Schröter decided in favour of the first hypothesis, and Bessel, from a discussion of this observer's data, determined the time of rotation to be 24h. 0m. 52·97s. Schiaparelli's observations support the last of these hypotheses, and are opposed to the rotation period determined by Schröter.

Following a series of dark markings, shown in the figure which accompanies the article, it was found that—

Mercury revolves round the sun in the same manner that the moon revolves round the earth, always presenting to it the same hemisphere; hence, since the planet's periodic time is 87·9693 days, this must be the time of rotation on its axis.

The dark markings observed appear extremely faint, and are not easily recognized. On good occasions the colour may be seen to be reddish-brown, and always differs from the general colour of the planet's disk, which is a bright rose changing to copper.

This most interesting and important communication from Milan Observatory must be read in detail in order that it may be appreciated.

ON CERTAIN APPROXIMATE FORMULÆ FOR CALCULATING THE TRAJECTORIES OF SHOT.

IN the postscript to a paper by Mr. W. D. Niven, "On the Calculation of the Trajectories of Shot," which is published in the Proceedings of the Royal Society, vol. xxvi. pp. 268–287, I have given, without demonstration, some convenient and not inelegant formulæ applicable to a limited arc of a trajectory when the resistance is supposed to vary as the nth power of the velocity.

In these formulæ, the angle between the chord of the arc and the tangent at any point is supposed to be always small. The index n is not restricted to integral values, but may take any value whatever.

As the proof of these formulæ is not altogether obvious, and a similar method of treatment may be found useful in other problems, I think it may not be unacceptable to your readers if I show here how the formulæ may be demonstrated.

Analysis.

Investigation of formulæ applicable to a small arc of a trajectory, when the resistance varies as the nth power of the velocity.

Let x and y denote the horizontal and vertical co-ordinates at time t, u the horizontal velocity, and ϕ the angle which the direction of motion makes with the horizon at the same time.

Hence the velocity at time t is $u \sec \phi$, and we may denote the resistance by $ku^n(\sec\phi)^n$, where k is constant throughout the small arc in question.

Also let p and q denote the values of u at the beginning and end of the arc, α and β the corresponding values of ϕ, g the force of gravity, T the time taken to describe the arc, X and Y the corresponding total horizontal and vertical motion.

Making ϕ the independent variable, the fundamental formulæ are—

$$(1)\quad \frac{du}{d\phi} = \frac{ku^{n+1}}{g}(\sec\phi)^{n+1};$$

$$(2)\quad \frac{dx}{d\phi} = -\frac{u^2}{g}(\sec\phi)^2;$$

$$(3)\quad \frac{dy}{d\phi} = -\frac{u^2}{g}(\sec\phi)^2\tan\phi;$$

$$(4)\quad \frac{dt}{d\phi} = -\frac{u}{g}(\sec\phi)^2.$$

From the first of these equations—

$$\frac{1}{u^{n+1}}\frac{du}{d\phi} = \frac{k}{g}(\sec\phi)^{n+1};$$

and therefore, by integration between the limits $\phi = \alpha$ and $\phi = \beta$,

$$\frac{1}{q^n} - \frac{1}{p^n} = \frac{kn}{g}\int_\beta^\alpha (\sec\phi)^{n+1}d\phi.$$

Also, we have—

$$X = \frac{1}{g}\int_\beta^\alpha u^2(\sec\phi)^2 d\phi;$$

$$Y = \frac{1}{g}\int_\beta^\alpha u^2(\sec\phi)^2\tan\phi\, d\phi;$$

and

$$T = \frac{1}{g}\int_\beta^\alpha u(\sec\phi)^2 d\phi;$$

and we wish to compare the two former of these definite integrals with the following known one, viz. :—

$$\frac{1}{q^{n-2}} - \frac{1}{p^{n-2}} = (n-2)\int_\beta^\alpha \frac{1}{u^{n-1}}\frac{du}{d\phi}d\phi = \frac{k(n-2)}{g}\int_\beta^\alpha u^2(\sec\phi)^{n+1}d\phi;$$

and the last with—

$$\frac{1}{q^{n-1}} - \frac{1}{p^{n-1}} = (n-1)\int_\beta^\alpha \frac{1}{u^n}\frac{du}{d\phi}d\phi = \frac{k(n-1)}{g}\int_\beta^\alpha u(\sec\phi)^{n+1}d\phi.$$

This may be done by means of the following lemma, which follows immediately from Taylor's theorem :—

Lemma.

If $F(\phi)$ be any function either of ϕ only, or of ϕ and u, where u is a function of ϕ given by the above differential equation (1), and if α and β be the limiting values of ϕ in the integral and $\gamma = \frac{1}{2}(\alpha+\beta)$, then, putting for a moment $\phi = \gamma + \omega$,

$$\int_\beta^\alpha F(\phi)d\phi = \int_{-\frac{1}{2}(\alpha-\beta)}^{\frac{1}{2}(\alpha-\beta)} F(\gamma+\omega)d\omega = \int_{-\frac{1}{2}(\alpha-\beta)}^{\frac{1}{2}(\alpha-\beta)}\left\{F(\gamma) + F'(\gamma)\omega + F''(\gamma)\frac{\omega^2}{2} + F'''(\gamma)\frac{\omega^3}{6} + F''''(\gamma)\frac{\omega^4}{24} + \&c.\right\}d\omega$$

$$= (\alpha-\beta)\left\{F(\gamma) + \frac{1}{24}(\alpha-\beta)^2F''(\gamma) + \frac{1}{1920}(\alpha-\beta)^4F''''(\gamma) + \&c.\right\}$$

where $F'(\phi) = \frac{dF(\phi)}{d\phi}$, $F''(\phi) = \frac{d^2F(\phi)}{d\phi^2}$, &c., and $F(\gamma)$, $F'(\gamma)$, $F''(\gamma)$, &c., are what $F(\phi)$, $F'(\phi)$, $F''(\phi)$, &c., become when γ is substituted for ϕ, and the corresponding value of u (u_0 suppose) is put for u.

In what follows, the last of the terms above written, which is of the 5th order in $(\alpha - \beta)$, is neglected, together with all terms of the same order of small quantities.

All the definite integrals with which we are here concerned are included in the two forms

$$\int_\beta^\alpha u^l(\sec\phi)^m d\phi, \text{ and } \int_\beta^\alpha u^l(\sec\phi)^m\tan\phi\, d\phi.$$

In the first place, we will apply the above formula to the case in which $F(\phi)$ is a function of ϕ only, viz. when $F(\phi) = (\sec\phi)^{n+1}$.

Hence

$$F'(\phi) = (n+1)(\sec\phi)^{n+1}\tan\phi;$$

$$F''(\phi) = (n+1)[(n+1)(\sec\phi)^{n+1}(\tan\phi)^2 + (\sec\phi)^{n+3}]$$

$$= (n+1)[\overline{n+2}(\sec\phi)^{n+3} - \overline{n+1}(\sec\phi)^{n+1}];$$

and therefore,

$$\int_\beta^\alpha(\sec\phi)^{n+1}d\phi = (\alpha-\beta)(\sec\gamma)^{n+1}\left\{1 + \frac{n+1}{24}(\alpha-\beta)^2[\overline{n+2}(\sec\gamma)^2 - \overline{n+1}]\right\}, \text{ to the 4th order inclusive.}$$

Hence

$$\frac{1}{q^n} - \frac{1}{p^n} = \frac{kn}{g}(\alpha-\beta)(\sec\gamma)^{n+1}\left\{1 + \frac{n+1}{24}(\alpha-\beta)^2[\overline{n+2}(\sec\gamma)^2 - \overline{n+1}]\right\},$$

which gives q when p is known.

In the next place, let $F(\phi) = u^l(\sec\phi)^m$.

Hence

$$F'(\phi) = \frac{dF\phi}{d\phi} = lu^{l-1}\frac{du}{d\phi}(\sec\phi)^m + mu^l(\sec\phi)^m\tan\phi$$

$$= F(\phi)\left[\frac{l}{u}\frac{du}{d\phi} + m\tan\phi\right],$$

or

$$F'(\phi) = F(\phi)\left[\frac{kl}{g}u^n(\sec\phi)^{n+1} + m\tan\phi\right];$$

and $F''(\phi) = F'(\phi)\left[\frac{kl}{g}u^n(\sec\phi)^{n+1} + m\tan\phi\right] + F(\phi)\left[\frac{kln}{g}u^{n-1}\frac{du}{d\phi}(\sec\phi)^{n+1} + \frac{kl}{g}(n+1)u^n(\sec\phi)^{n+1}\tan\phi + m(\sec\phi)^2\right]$,

or

$$F''(\phi) = F(\phi)\left[\frac{k^2l^2}{g^2}u^{2n}(\sec\phi)^{2n+2} + 2\frac{klm}{g}u^n(\sec\phi)^{n+1}\tan\phi + m^2(\sec\phi)^2 - m^2\right]$$
$$+ F(\phi)\left[\frac{k^2ln}{g^2}u^{2n}(\sec\phi)^{2n+2} + \frac{kl}{g}(n+1)u^n(\sec\phi)^{n+1}\tan\phi + m(\sec\phi)^2\right]$$
$$= F(\phi)\left\{\frac{k^2l}{g^2}(l+n)u^{2n}(\sec\phi)^{2n+2} + \frac{kl}{g}(2m+n+1)u^n(\sec\phi)^{n+1}\tan\phi + m(m+1)(\sec\phi)^2 - m^2\right\}.$$

Since

$$\frac{du}{d\phi} = \frac{k}{g}u^{n+1}(\sec\phi)^{n+1},$$

this last expression may be put under the form—

$$F''(\phi) = F(\phi)\left\{l(l+n)\left(\frac{du}{ud\phi}\right)^2 + l(2m+n+1)\left(\frac{du}{ud\phi}\right)\tan\phi + m(m+1)(\sec\phi)^2 - m^2\right\}.$$

Hence, by the above lemma,

$$\int_\beta^\alpha u^l\sec\phi^m d\phi = (\alpha-\beta)F(\gamma)\left\{1 + \tfrac{1}{24}(\alpha-\beta)^2\left[l(l+n)\left(\frac{du}{ud\phi}\right)_0^2 + l(2m+n+1)\left(\frac{du}{ud\phi}\right)_0\tan\gamma + m(m+1)(\sec\gamma)^2 - m^2\right]\right\}$$
$$= (\alpha-\beta)u_0^l(\sec\gamma)^m\left\{1 + \tfrac{1}{24}(\alpha-\beta)^2(\text{as before})\right\}$$

where $\left(\frac{du}{ud\phi}\right)_0$ denotes what $\frac{du}{ud\phi}$ becomes when $\omega = 0$, or when γ is substituted for ϕ, and u_0 for u, that is—

$$\left(\frac{du}{ud\phi}\right)_0 = \frac{k}{g}u_0^n(\sec\gamma)^{n+1}.$$

The factor u_0^l may be eliminated from this expression, and the expression itself simplified, by means of the formula—

$$\frac{1}{q^{n-l}} - \frac{1}{p^{n-l}} = (n-l)\int_\beta^\alpha\frac{1}{u^{n-l+1}}\frac{du}{d\phi}d\phi = \frac{k(n-l)}{g}\int_\beta^\alpha u^l(\sec\phi)^{n+1}d\phi,$$

for, putting $m = n + 1$ in the above expression, we have—

$$\int_\beta^\alpha u^l(\sec\phi)^{n+1}d\phi = (\alpha-\beta)u_0^l(\sec\gamma)^{n+1}\left\{1 + \tfrac{1}{24}(\alpha-\beta)^2\left[l(l+n)\left(\frac{du}{ud\phi}\right)_0^2 + 3l(n+1)\left(\frac{du}{ud\phi}\right)_0\tan\gamma + \overline{n+1}\;\overline{n+2}\,(\sec\gamma)^2 - (n+1)^2\right]\right\}.$$

Hence

$$\int_\beta^\alpha u^l(\sec\phi)^m d\phi \div \int_\beta^\alpha u^l(\sec\phi)^{n+1}d\phi,\ \text{or}\ \int_\beta^\alpha u^l(\sec\phi)^m d\phi \div \frac{g}{k(n-l)}\left(\frac{1}{q^{n-l}} - \frac{1}{p^{n-l}}\right)$$
$$= (\sec\gamma)^{m-n-1}\left\{1 + \tfrac{1}{24}(\alpha-\beta)^2\left[2l(m-n-1)\left(\frac{du}{ud\phi}\right)_0\tan\gamma + \overline{m-n-1}\;\overline{m+n+2}\,(\sec\gamma)^2 - \overline{m-n-1}\;\overline{m+n+1}\right]\right\}.$$

It will be noticed that the term involving $\left(\frac{du}{ud\phi}\right)_0^2$ has disappeared by this division.

Now make $m = 2$, and this formula becomes—

$$\int_\beta^\alpha u^l(\sec\phi)^2 d\phi = \frac{g}{k(n-l)}\left(\frac{1}{q^{n-l}} - \frac{1}{p^{n-l}}\right)(\cos\gamma)^{n-1}\left\{1 - \tfrac{1}{24}(\alpha-\beta)^2\left[2l(n-1)\left(\frac{du}{ud\phi}\right)_0\tan\gamma + \overline{n-1}\;\overline{n+4}\,(\sec\gamma)^2 - \overline{n-1}\;\overline{n+3}\right]\right\}.$$

Divide throughout by g, and put $l = 2$, then, from before,

$$X = \frac{1}{k(n-2)}\left(\frac{1}{q^{n-2}} - \frac{1}{p^{n-2}}\right)(\cos\gamma)^{n-1}\left\{1 - \frac{n-1}{24}(\alpha-\beta)^2\left[4\left(\frac{du}{ud\phi}\right)_0\tan\gamma + (n+4)(\sec\gamma)^2 - \overline{n+3}\right]\right\}.$$

Similarly, divide throughout by g, and put $l = 1$, then—

$$T = \frac{1}{k(n-1)}\left(\frac{1}{q^{n-1}} - \frac{1}{p^{n-1}}\right)(\cos\gamma)^{n-1}\left\{1 - \frac{n-1}{24}(\alpha-\beta)^2\left[2\left(\frac{du}{ud\phi}\right)_0\tan\gamma + (n+4)(\sec\gamma)^2 - \overline{n+3}\right]\right\}.$$

Lastly, let

$$F(\phi) = u^l(\sec\phi)^m\tan\phi = f(\phi)\tan\phi\ \text{suppose},$$

so that

$$f(\phi) = u^l(\sec\phi)^m;$$

then

$$F'(\phi) = f'(\phi)\tan\phi + f(\phi)(\sec\phi)^2,$$

and

$$F''(\phi) = f''(\phi)\tan\phi + 2f'(\phi)(\sec\phi)^2 + 2f(\phi)(\sec\phi)^2\tan\phi.$$

Hence

$$\int_\beta^\alpha F(\phi)d\phi = (\alpha-\beta)\{F(\gamma) + \tfrac{1}{24}(\alpha-\beta)^2F''(\gamma)\}\ \text{approximately},$$
$$= (\alpha-\beta)\left\{f(\gamma)\tan\gamma + \tfrac{1}{24}(\alpha-\beta)^2[f''(\gamma)\tan\gamma + 2f'(\gamma)(\sec\gamma)^2 + 2f(\gamma)(\sec\gamma)^2\tan\gamma]\right\};$$

also

$$\int_\beta^\alpha f(\phi)d\phi = (\alpha-\beta)\{f(\gamma) + \tfrac{1}{24}(\alpha-\beta)^2f''(\gamma)\}\ \text{approximately};$$

and therefore

$$\int_\beta^\alpha F(\phi)d\phi \div \int_\beta^\alpha f(\phi)d\phi = \tan\gamma + \tfrac{1}{12}(\alpha-\beta)^2\left[\frac{f'(\gamma)}{f(\gamma)}(\sec\gamma)^2 + (\sec\gamma)^2\tan\gamma\right];$$

in which the term involving $f''(\gamma)$ has disappeared.

Now, since $f(\phi) = u^l(\sec\phi)^m$, we have, as before

$$f'(\phi) = f(\phi)\left[l\left(\frac{du}{ud\phi}\right) + m\tan\phi\right];$$

and therefore—

$$\frac{f'(\gamma)}{f(\gamma)} = l\left(\frac{du}{ud\phi}\right)_0 + m\tan\gamma.$$

Hence—

$$\int_\beta^\alpha F(\phi)d\phi \div \int_\beta^\alpha f(\phi)d\phi = \tan\gamma + \tfrac{1}{12}(\alpha-\beta)^2(\sec\gamma)^2\left[l\left(\frac{du}{ud\phi}\right)_0 + m + 1\tan\gamma\right];$$

and in the particular case where $l = 2$, and $m = 2$, we have—

$$\frac{Y}{X} = \tan\gamma + \tfrac{1}{12}(\alpha-\beta)^2(\sec\gamma)^2\left[2\left(\frac{du}{ud\phi}\right)_0 + 3\tan\gamma\right]$$

$$= \tan\left\{\gamma + \tfrac{1}{12}(\alpha-\beta)^2\left[2\left(\frac{du}{ud\phi}\right)_0 + 3\tan\gamma\right]\right\}.$$

Hence the angle which the chord of the arc makes with the axis of x is—

$$\gamma + \tfrac{1}{12}(\alpha-\beta)^2\left[2\left(\frac{du}{ud\phi}\right)_0 + 3\tan\gamma\right] = \bar{\gamma},\text{ suppose.}$$

Multiplying by the value of X found above, we have—

$$Y = \frac{1}{k(n-2)}\left(\frac{1}{q^{n-2}} - \frac{1}{p^{n-2}}\right)(\cos\gamma)^{n-1}\left\{\tan\gamma - \tfrac{1}{24}(\alpha-\beta)^2\left\{\left(\frac{du}{ud\phi}\right)_0\left[4\overline{n-1}(\tan\gamma)^2 - 4(\sec\gamma)^2\right] + \tan\gamma\left[\overline{n-1}\,\overline{n+4}(\sec\gamma)^2 - 6(\sec\gamma)^2 - \overline{n-1}\,n+3\right]\right\}\right\};$$

or

$$Y = \frac{1}{k(n-2)}\left(\frac{1}{q^{n-2}} - \frac{1}{p^{n-2}}\right)(\cos\gamma)^{n-1}\left\{\tan\gamma - \tfrac{1}{24}(\alpha-\beta)^2\left\{\left(\frac{du}{ud\phi}\right)_0\left[4\overline{n-2}(\sec\gamma)^2 - 4\overline{n-1}\right] + \tan\gamma\left[\overline{n-2}\,\overline{n+5}(\sec\gamma)^2 - \overline{n-1}\,n+3\right]\right\}\right\}.$$

Considering $\frac{1}{q^{n-2}} - \frac{1}{p^{n-2}}$, $\frac{1}{q^{n-1}} - \frac{1}{p^{n-1}}$, and $\alpha - \beta$ to be small quantities of the first order, the above expressions for $\frac{1}{q^n} - \frac{1}{p^n}$, X, Y, and T are true to the fourth order.

The quantity $\left(\frac{du}{ud\phi}\right)_0$ which occurs as a factor in some of the terms of the third order may be put under a very convenient form in the following manner.

We have, by Taylor's theorem,

$$u = (u_0) + \left(\frac{du}{d\phi}\right)_0\omega + \left(\frac{d^2u}{d\phi^2}\right)_0\frac{\omega^2}{2} + \&c.$$

In this make $\omega = \frac{1}{2}(\alpha-\beta)$ and $-\frac{1}{2}(\alpha-\beta)$ successively; therefore

$$p = u_0 + \tfrac{1}{2}(\alpha-\beta)\left(\frac{du}{d\phi}\right)_0 + \tfrac{1}{8}(\alpha-\beta)^2\left(\frac{d^2u}{d\phi^2}\right)_0 + \&c.,$$

and

$$q = u_0 - \tfrac{1}{2}(\alpha-\beta)\left(\frac{du}{d\phi}\right)_0 + \tfrac{1}{8}(\alpha-\beta)^2\left(\frac{d^2u}{d\phi^2}\right)_0 - \&c.$$

Hence we have to the first order of small quantities—

$$\frac{p-q}{\alpha-\beta} = \left(\frac{du}{d\phi}\right)_0,$$

and

$$\tfrac{1}{2}(p+q) = u_0;$$

and therefore

$$\left(\frac{du}{ud\phi}\right)_0 = \frac{2(p-q)}{(p+q)(\alpha-\beta)}\text{ to the first order.}$$

Making this substitution for $\left(\frac{du}{ud\phi}\right)_0$ the expressions for X, Y, and T become—

$$X = \frac{1}{k(n-2)}\left(\frac{1}{q^{n-2}} - \frac{1}{p^{n-2}}\right)(\cos\gamma)^{n-1}\left\{1 - \frac{n-1}{3}\cdot\frac{p-q}{p+q}(\alpha-\beta)\tan\gamma - \frac{n-1}{24}(\alpha-\beta)^2\left[n+4(\sec\gamma)^2 - \overline{n+3}\right]\right\};$$

$$Y = \frac{1}{k(n-2)}\left(\frac{1}{q^{n-2}} - \frac{1}{p^{n-2}}\right)(\cos\gamma)^{n-1}\left\{\tan\gamma - \tfrac{1}{3}\cdot\frac{p-q}{p+q}(\alpha-\beta)\left[\overline{n-2}(\sec\gamma)^2 - \overline{n-1}\right] - \tfrac{1}{24}(\alpha-\beta)^2\tan\gamma\left[\overline{n-2}\,\overline{n+5}(\sec\gamma)^2 - n-1\,\overline{n+3}\right]\right\};$$

$$T = \frac{1}{k(n-1)}\left(\frac{1}{q^{n-1}} - \frac{1}{p^{n-1}}\right)(\cos\gamma)^{n-1}\left\{1 - \frac{n-1}{6}\frac{p-q}{p+q}(\alpha-\beta)\tan\gamma - \frac{n-1}{24}(\alpha-\beta)^2\left[\overline{n+4}(\sec\gamma)^2 - \overline{n+3}\right]\right\};$$

and these values are still true to the fourth order, considering $\frac{p-q}{p+q}$ and $\alpha - \beta$ to be small quantities of the first order as before.

The angle which the chord of the arc makes with the axis of x becomes, in like manner—

$$\bar{\gamma} = \gamma + \tfrac{1}{3}\frac{p-q}{p+q}(\alpha-\beta) + \tfrac{1}{4}(\alpha-\beta)^2\tan\gamma,$$

which is true to the third order.

The above expressions for X and Y may be transformed by introducing this angle $\bar{\gamma}$ into them instead of γ, thus—

$$(\cos\bar{\gamma})^{n-1} = (\cos\gamma)^{n-1} - (n-1)(\cos\gamma)^{n-2}\sin\gamma\left[\tfrac{1}{3}\frac{p-q}{p+q}(\alpha-\beta) + \tfrac{1}{4}(\alpha-\beta)^2\tan\gamma\right]$$

$$= (\cos\gamma)^{n-1}\left\{1 - \frac{n-1}{3}\frac{p-q}{p+q}(\alpha-\beta)\tan\gamma - \frac{n-1}{4}(\alpha-\beta)^2(\tan\gamma)^2\right\}.$$

Hence we find—

$$X = \frac{1}{k(n-2)}\left(\frac{1}{q^{n-2}} - \frac{1}{p^{n-2}}\right)(\cos\bar{\gamma})^{n-1}\left\{1 - \frac{n-1}{24}(\alpha-\beta)^2[\overline{n-2}\,(\sec\gamma)^2 - \overline{n-3}]\right\},$$

and

$$Y = X\tan\bar{\gamma} = \frac{1}{k(n-2)}\left(\frac{1}{q^{n-2}} - \frac{1}{p^{n-2}}\right)(\cos\bar{\gamma})^{n-2}\sin\bar{\gamma}\left\{1 - \frac{n-1}{24}(\alpha-\beta)^2[\overline{n-2}\,(\sec\gamma)^2 - \overline{n-3}]\right\};$$

or

$$X = \frac{1}{k(n-2)}\left(\frac{1}{q^{n-2}} - \frac{1}{p^{n-2}}\right)(\cos\bar{\gamma})^{n-1}Q;$$

$$Y = \frac{1}{k(n-2)}\left(\frac{1}{q^{n-2}} - \frac{1}{p^{n-2}}\right)(\cos\bar{\gamma})^{n-2}\sin\bar{\gamma}\,Q;$$

Q being $= 1 - \dfrac{n-1}{24}(\alpha-\beta)^2[\overline{n-2}\,(\sec\gamma)^2 - \overline{n-3}]$.

Similarly, if

$$\bar{\gamma}' = \gamma + \tfrac{1}{6}\frac{p-q}{p+q}(\alpha-\beta) + \tfrac{1}{4}(\alpha-\beta)^2\tan\gamma,$$

we have

$$(\cos\bar{\gamma}')^{n-1} = (\cos\gamma)^{n-1} - (n-1)(\cos\gamma)^{n-2}\sin\gamma\left[\tfrac{1}{6}\frac{p-q}{p+q}(\alpha-\beta) + \tfrac{1}{4}(\alpha-\beta)^2\tan\gamma\right];$$

$$= (\cos\gamma)^{n-1}\left\{1 - \frac{n-1}{6}\frac{p-q}{p+q}(\alpha-\beta)\tan\gamma - \frac{n-1}{4}(\alpha-\beta)^2(\tan\gamma)^2\right\};$$

and therefore

$$T = \frac{1}{k(n-1)}\left(\frac{1}{q^{n-1}} - \frac{1}{p^{n-1}}\right)(\cos\bar{\gamma}')^{n-1}\left\{1 - \frac{n-1}{24}(\alpha-\beta)^2\left[\overline{n-2}\,(\sec\gamma)^2 - \overline{n-3}\right]\right\}$$

$$= \frac{1}{k(n-1)}\left(\frac{1}{q^{n-1}} - \frac{1}{p^{n-1}}\right)(\cos\bar{\gamma}')^{n-1}Q,$$

where Q has the same value as before.

Hence the values of X, Y, and T are as stated in my postscript to Mr. Niven's paper.

Although the method of finding the expressions for X and T given above, is perhaps the plainest and most straightforward that can be taken, the following leads to simpler operations.

Let $f(\phi) = u^l(\sec\phi)^{n+1}$.

Then $\displaystyle\int f(\phi)d\phi = \int u^l(\sec\phi)^{n+1}d\phi = \frac{g}{k}\int u^{l-n-1}\frac{du}{d\phi}d\phi$ by equation (1)

$$= \frac{g}{k(l-n)}u^{l-n} + \text{const.}$$

Hence

$$\int_\beta^\alpha f(\phi)d\phi = \frac{g}{k(l-n)}(p^{l-n} - q^{l-n})$$

Now let

$$F(\phi) = f(\phi)(\sec\phi)^m = u^l(\sec\phi)^{m+n+1},$$

then

$$F'(\phi) = f'(\phi)(\sec\phi)^m + mf(\phi)(\sec\phi)^m\tan\phi,$$

and

$$F''(\phi) = f''(\phi)(\sec\phi)^m + 2mf'(\phi)(\sec\phi)^m\tan\phi] + mf(\phi)[m(\sec\phi)^m(\tan\phi)^2 + (\sec\phi)^{m+2}]$$

$$= f''(\phi)(\sec\phi)^m + 2mf'(\phi)(\sec\phi)^m\tan\phi + mf(\phi)[\overline{m+1}(\sec\phi)^{m+2} - m(\sec\phi)^m].$$

Hence, by the lemma,

$$\int_\beta^\alpha F(\phi)d\phi = (\alpha-\beta)\{F(\gamma) + \tfrac{1}{24}(\alpha-\beta)^2F''(\gamma)\}$$

$$= (\alpha-\beta)\left\{f(\gamma)(\sec\gamma)^m + \tfrac{1}{24}(\alpha-\beta)^2(\sec\gamma)^m[f''(\gamma) + 2mf'(\gamma)\tan\gamma + mf(\gamma)[\overline{m+1}(\sec\gamma)^2 - m]]\right\}$$

$$= (\alpha-\beta)(\sec\gamma)^m\left\{f(\gamma) + \tfrac{1}{24}(\alpha-\beta)^2[f''(\gamma) + 2mf'(\gamma)\tan\gamma + mf(\gamma)[\overline{m+1}\,(\sec\gamma)^2 - m]]\right\}$$

But from above

$$\frac{g}{k(l-n)}(p^{l-n} - q^{l-n}) = \int^\alpha f(\phi)d\phi,$$

$$= (\alpha-\beta)\{f(\gamma) + \tfrac{1}{24}(\alpha-\beta)^2f''(\gamma)\}.$$

Hence, by division,

$$\int^\alpha F(\phi)d\phi \div \frac{g}{k(l-n)}(p^{l-n} - q^{l-n}) = (\sec\gamma)^m\left\{1 + \tfrac{1}{24}(\alpha-\beta)^2\left[2m\frac{f'\gamma}{f\gamma}\tan\gamma + m(\overline{m+1}(\sec\gamma)^2 - m)\right]\right\}.$$

It will be noticed that in this division the quantity $f''(\gamma)$ has disappeared.

Now, from above,

$$f(\phi) = u^l(\sec\phi)^{n+1},$$

and therefore

$$\frac{f'(\phi)}{f(\phi)} = l\frac{du}{ud\phi} + (n+1)\tan\phi,$$

and

$$\frac{f'(\gamma)}{f(\gamma)} = l\left(\frac{du}{ud\phi}\right)_0 + (n+1)\tan\gamma.$$

Hence

$$\int_\beta^\alpha F(\phi)d\phi \div \frac{g}{k(l-n)}(p^{l-n} - q^{l-n}) = (\sec\gamma)^m\left\{1 + \tfrac{1}{24}(\alpha-\beta)^2\left[2lm\left(\frac{du}{ud\phi}\right)_0 \tan\gamma + 2m\,\overline{n+1}(\tan\gamma)^2 + m\left(\overline{m+1}(\sec\gamma)^2 - m\right)\right]\right\}$$

$$= (\sec\gamma)^m\left\{1 + \tfrac{1}{24}(\alpha-\beta)^2\left[2lm\left(\frac{du}{ud\phi}\right)_0 \tan\gamma + m(m+2n+3)(\sec\gamma)^2 - m(m+2n+2)\right]\right\}.$$

Now make $m + n + 1 = 2$,

or $m = -(n-1)$, and we have

$$\int_\beta^\alpha u^l(\sec\phi)^2 \div \frac{g}{k(l-n)}(p^{l-n} - q^{l-n}) = (\cos\gamma)^{n-1}\left\{1 - \tfrac{1}{24}(\alpha-\beta)^2\left[2l(n-1)\left(\frac{du}{ud\phi}\right)_0 \tan\gamma + (n-1)(n+4)(\sec\gamma)^2 - (n-1)(n+3)\right]\right\}.$$

In this make $l = 2$, and $l = 1$, successively, and we obtain the same expressions for X and T as before.

The case thus treated is not one of mere curiosity, but is practically important. From theoretical considerations, Newton concluded that the resistance of the air to the motion of projectiles is proportional to the square of the velocity, and very little progress has been made in the theory of the subject since his time. Experiments have shown that the relation between the velocity of a projectile and the resistance offered by the air to its motion is far from being so simple as that given by the theory. The most extensive and accurate series of such experiments which we ~~have~~ are those made by Mr. Bashforth by means of his chronograph, which measures with the greatest precision the times taken by the same projectile in passing over several successive arcs in the course of its flight. In a summary of his results for ogival-headed shot, struck with a radius of $1\frac{1}{2}$ diameters, given in NATURE (vol. xxxiii. pp. 605, 606), Mr. Bashforth concludes that the resistance may be approximately represented by supposing it to vary as one power of the velocity when that velocity lies between certain limits, as another power when the velocity lies between certain other limits, and so on.

Thus, if v denote the velocity expressed in feet per second,

d the diameter of the shot in inches,

and w its weight in pounds,

and if $\frac{d^2}{w} = c$,

then, when v lies between 430 f.s. and 850 f.s.,

the resistance is nearly $= 61{\cdot}3\,c\left(\frac{v}{1000}\right)^2$;

when v lies between 850 f.s. and 1040 f.s.,

the resistance is nearly $= 74{\cdot}4\,c\left(\frac{v}{1000}\right)^3$;

when v lies between 1040 f.s. and 1100 f.s.,

the resistance is nearly $= 79{\cdot}2\,c\left(\frac{v}{1000}\right)^6$;

when v lies between 1100 f.s. and 1300 f.s.,

the resistance is nearly $= 108{\cdot}8\,c\left(\frac{v}{1000}\right)^3$;

and lastly, when v lies between 1300 f.s. and 2700 f.s.,

the resistance is nearly $= 141{\cdot}5\,c\left(\frac{v}{1000}\right)^2$.

Hence the resistance varies nearly as the square of the velocity both when the velocity is less than 850 f.s., and when it is greater than 1300 f.s., but the coefficient increases from 61·3 in the former case, to 141·5 in the latter. Also, the resistance varies nearly as the cube of the velocity, both when v lies between 850 f.s. and 1040 f.s., and also when it lies between 1100 f.s. and 1300 f.s., but the coefficient increases from 74·4 in the former to 108·8 in the latter case. Again, for velocities which are nearly equal to that of sound in air, the proportionate increase of the resistance is much greater than that of the velocity.

Mr. Bashforth remarks that the points of transition from one law of resistance to another, as stated above, are somewhat arbitrary, but that, if they were changed a little in either direction, the practical error would not be large.

Of course, if we had at our disposal much more numerous and still more accurate observations, it would be possible to represent the experimental results with any degree of exactness that might be desired, by subdividing the observations into a larger number of groups, so that the limiting velocities in any one group should be closer together, and that the change of the index of the power of the velocity in passing from one group to the next should be less abrupt.

J. C. ADAMS.

SOCIETIES AND ACADEMIES.

LONDON.

Chemical Society, December 19, 1889.—Dr. W. J. Russell, F.R.S., in the chair.—The following papers were read:—Frangulin, by Prof. T. E. Thorpe, F.R.S., and Mr. H. H. Robinson. The authors prepared the glucoside frangulin from the bark of the alder buckthorn (*Rhamnus frangula*), and find its formula to be $C_{22}H_{22}O_9$. On hydrolysis it yields a yellow product, $C_{15}H_{10}O_5$, which agrees in its properties with emodin, and a sugar which has the power of reducing Fehling's solution, and is not identical with dextrose.—Arabinon, the saccharon of arabinose, by Mr. C. O'Sullivan, F.R.S. The substance having an optical activity "well above $[\alpha]_j = 140$," obtained by the author by the hydrolysis of arabic acid, and described under the name of α-arabinose (Chem. Soc. Trans., 1884, 55), yields arabinose on hydrolysis, and appears to bear to this carbohydrate a relation similar to that which saccharon (cane sugar) bears to dextrose: the author therefore terms it arabinon. It has the formula $C_{10}H_{18}O_9$, and on hydrolysis gives a yield of arabinose agreeing very closely with that required by the equation $C_{10}H_{18}O_9 + H_2O = 2C_5H_{10}O_5$. As yet it has not been obtained in a crystalline state; it has a specific rotatory power of $[\alpha]_D = 198°{\cdot}8$, and 100 parts have the same cupric reducing power as 58·8 parts of dextrose.—On the identity of cerebrose and galactose, by Mr. H. T. Brown, F.R.S., and Dr. G. H. Morris. The authors give the results of an examination of a specimen of cerebrose, prepared from phrenosin, which was placed in their hands early in 1888 by Dr. Thudichum, who first isolated and crystallized this substance. They show that its specific rotatory power, cupric reducing power, and molecular weight as determined by Raoult's method, are identical with those of galactose, thus confirming the recent work of Thierfelder, *Zeit. Physiol. Chem.*, 14, 209) who has proved the sugar produced by the action of acid on cerebrin to be identical with galactose. In the discussion which followed the reading of the paper, Dr. Thudichum said that phrenosin, $C_{41}H_{79}NO_8$, consisted of the sugar now shown to be identical with galactose, $C_6H_{12}O_6$, of neurostearic acid, $C_{18}H_{36}O_2$, an isomeride of stearic acid, fusing at 84°, and of sphingosine, an

alkaloid of the formula $C_{17}H_{35}NO_2$. Some human brains contained as much as 4 per cent. of phrenosin in addition to other glucosides. The crystallized sugar (galactose) from phrenosin was always accompanied by an almost equal weight of uncrystallizable sugar, of which the nature was not yet ascertained.—The action of chloroform and alcoholic potash on hydrazines, Part 3, by Dr. S. Ruhemann. The products formed by the action of chloroform and alcoholic potash on hydrazines are to be regarded as derivatives of tetrazine, $N \langle \begin{smallmatrix} CH.NH \\ NH.CH \end{smallmatrix} \rangle N$; and in the present communication the author describes the di-paratolyl,-orthotolyl, and -pseudocumyl derivatives of this base (cf. Chem. Soc. Trans., 1889, 242).

Royal Microscopical Society, December 11, 1889.—The Rev. Dr. Dallinger, F.R.S., Vice-President, in the chair.—Mr. E. M. Nelson read a short paper descriptive of a new semi-apochromatic objective which he exhibited.—Mr. C. Rousselet exhibited a small tank for Rotifers which could be readily moved about in such a way as to render an examination of the contents very easy, so that any desired specimens could be easily picked out. The lens used was a Zeiss's No. 6 Steinheil, the focussing being done by rackwork.—Mr. Crisp called attention to a number of stereoscopic photomicrographs of embryos, by Prof. Fol. They afforded a conclusive answer to the question brought forward at their meeting as to whether stereoscopic photomicrographic slides had been produced before that time.—Mr. Crisp read some extracts from a paper by Mr. Gill, which he was sorry to say was only handed in at the conclusion of their last meeting, as otherwise it could have been read then, and would have added to the interest of the specimens exhibited at the *conversazione*, which seemed almost conclusively to prove that the "markings" on certain diatoms were apertures.—Mr. A. W. Bennett gave a *résumé* of his paper on the freshwater Algæ and Schizophyceæ of Hampshire and Devon. It was the result of collections made, during his summer holidays, in the New Forest and on Dartmoor, many of the species being not only interesting, but also new to science.—Mr. Crisp reminded the Fellows present that at the last meeting mention was made of a new objective with an aperture of 1·60, the price of which was said to be £400. Some doubt was expressed at the time as to whether the account was true, but since then they had received several communications about it. A letter from Prof. Abbe, describing the principles of its construction, was read. Letters were also read from Dr. van Heurck, describing the performance of the lens, and inclosing a series of remarkable photomicrographs of diatoms taken with it, with magnifying powers of 10,000 and 15,000 diameters.

Paris.

Academy of Sciences, January 6.—M. Hermite in the chair.—State of the Academy on January 1. Full lists are given of the Members of the various Sections. Amongst the foreign Associates and Correspondents occur the following English and American names:—*Associates:* Sir Richard Owen, Sir George Biddell Airy, and Sir William Thomson. *Correspondents: Geometry*—James Joseph Sylvester and George Salmon; *Astronomy*—John Russell Hind, J. C. Adams, Arthur Cayley, Joseph Norman Lockyer, William Huggins, Simon Newcomb, Asaph Hall, Benjamin Apthorp Gould, and Samuel Langley; *Geography and Navigation*—Rear-Admiral George Henry Richards; *General Physics*—George Gabriel Stokes; *Chemistry*—Edward Frankland and Alexander William Williamson; *Mineralogy*—James Hall and Joseph Prestwich; *Botany*—Joseph Dalton Hooker and Maxwell Tylden Masters; *Rural Economy*—John Bennet Lawes and Joseph Henry Gilbert; *Anatomy and Zoology*—James Dwight Dana, Thomas Henry Huxley, and Alexander Agassiz; *Medicine and Surgery*—Sir James Paget.—M. Duchartre was elected Vice-President for the year 1890.—Analogy of diamantiferous matrix in South Africa to meteorites, by M. Daubrée. It is argued that the South African diamonds were not formed *in situ*, but were erupted from great depths together with the fragmentary materials in which they are embedded. The presence of the diamond in the normal state and as carbonado, as well as transformed from graphite in various types of meteorites, is now placed beyond reasonable doubt. Attention is here called to the analogous conditions of association under which this crystal occurs in South Africa and in meteorites. M. Daubrée incidentally infers that the diamond is not, as is generally supposed, of vegetable origin, but is of inorganic nature, as is also the graphite occurring in analogous beds.—On some new fluorescent materials, by M. Lecoq de Boisbaudran. The author describes some new fluorescent appearances which he has obtained by employing samaria and the earths $Z\alpha$ and $Z\beta$ as agents, and calcined silica and zircon as solid solvents. Mr. Crookes's failure to obtain any fluorescences from samaria with SiO_2 and ZrO_2, he considers was probably due to their having been calcined at too low a temperature.—Observations of Borrelly's comet made at the Observatory of Algiers, by MM. Trépied, Ramraud, and Renaux. The observations are for the period December 23–30, when the nebulosity was somewhat elongated, and about 2′ in extent.—Observations of Brooks's comet (July 6, 1889) made at the Observatory of Nice with the 0·38m. equatorial, by M. D. Eginitis.—On the elliptic functions, by M. Paul Appell. It is shown that the representation of the elliptic functions by the quotient of Θ functions may be justified *a priori* by considerations which seem capable of being extended to the functions of two variables with four groups of periods.—On the rational integrals of equations of the first order, by M. P. Painlevé. Given a differential equation of any order, it is shown that the polynomes may always be found which verify the equation by determining a higher limit of their degree.—On the absolute value of the magnetic elements on January 1, 1890, by M. Th. Moureaux. These values are deduced from the mean of the horary observations taken at the Parc Saint-Maur on December 31, 1889, and January 1, 1890, and at Perpignan from the twenty-four horary observations taken on January 1.—On the refracting powers of the simple salts in solution, by M. E. Doumer. Owing to Mr. B. Walter's recent note in *Wiedemann's Annalen* (1889, No. 9, p. 107), M. Doumer here publishes somewhat prematurely the researches on this subject, which he has carried on for over five years, and during which he has dealt with 90 salts. He concludes that all salts formed by the same acid have the same molecular refracting power when they are constructed on the same type; that the refracting powers of salts belonging to different types are approximately multiples of the same number; lastly, that the molecular refracting powers of all salts are functions of the number of valencies of the metallic element entering into their construction.—Papers were read by M. Georges Vogt, on the composition of the rocks employed in China for the manufacture of porcelain; by M. Charles Combes, on matezite and matezo-dambose; by M. E. Guinochet, on the carbally lates; by M. A. Lacroix, on the mineral-bearing cipoline marbles and the wernerite rocks of Ariège; and by M. Thoulet, on the sub-lacustrine relief, geology, and temperature of Lake Longemer (Vosges).

Berlin.

Physiological Society, December 13, 1889.—Prof. du Bois-Reymond, President, in the chair.—Prof. Moebius spoke on a "drumming" fish (*Balistes aculeatus*) from Mauritius. During a recent visit to this island he observed a bright blue-coloured fish in the shallow waters of the harbour; when caught and held in the hand this fish emitted from its interior a most striking noise, like that of a drum. A careful examination of the animal failed to reveal any obvious movements, with the exception of one part of the skin, lying just behind the gill-slit, which was in continuous vibration. Nothwithstanding prolonged endeavours he had not been able to secure a second living example of this fish, and had hence been able to carry out his investigations on the cause of the drumming noise only on dead specimens. The portion of the skin (membrana supra-axillaris) which vibrates stretches from the clavicle to the branchial arch: it is provided with four large bony plates, and lies over the swim-bladder, which in this fish for the most part projects out of the trunk-muscles. Behind the clavicle lies a curiously-shaped long bone, which is attached to the clavicle at one point in such a way as to form a lever with two arms. The long arm of this bony lever (os post-claviculare) is embedded in the ventral trunk-muscles, and is capable of easy movement to and fro. The short arm slides during this movement over the rough inner side of the clavicle, and gives rise to a crackling noise, and this noise is then intensified by the swim-bladder, which lies in close proximity to the short arm of the lever, and acts as a resonator. When the trunk-muscles contract the body cavity is diminished in size, the air in the swim-bladder is driven forward, and the bladder then communicates the vibrations of the bony lever to the membrana

supra-axillaris, and the latter communicates them to the air. The speaker was of opinion that the above was the explanation of the "drumming" of this fish; he was, at all events, unable to find any other organ in it which could account for the noise. This noise is not known to be emitted by other species of Balistes, although it is known to occur in other groups.—Prof. Fritsch spoke on the anatomy of *Torpedo marmorata*. In opposition to the revolutionary views of many recent investigators, who deny the nervous nature of the ganglion-cells, he laid great stress upon the extremely close relationship which exists between the ganglia and end-organs, and is so strikingly shown in Torpedo. A thick nerve-fibre runs from each ganglion-cell to the electrical-organ, divides into twelve to twenty-three fibrils before it reaches the organ, and each of these fibrils is connected up with some one special plate of the organ. Now, since each plate, which is of hexagonal shape, owing to the close juxtaposition of the columns, receives one nerve-fibre at each of its angles, it hence follows that the number of the plates must be, on the average, three times as great as the number of the ganglia. The fibres of one ganglion supply eighteen plates, the latter (being hexagonal) require six times eighteen fibres for their supply, and since on an average eighteen fibres run out from each ganglion, it requires six ganglia to supply eighteen plates with nerves. The speaker had counted the plates of an electrical-organ in Torpedo, and obtained a number corresponding closely with an older enumeration of Valentin's made on a Torpedo of the same size; the number of plates he found to be 179,625. He had further counted the ganglion-cells which supply the plates with nerves and found them to number 53,739; this corresponds closely with the enumeration of Boll, who counted 53,760. The counting of ganglion-cells is subject to much uncertainty, chiefly owing to the fact that in sections of the central nervous system many cells are cut through, and are thus liable to be counted twice: hence the speaker had enumerated, most readily by means of photographs, the axis-cylinders of the nerves which supply the electric-organ; he found them to number 58,318, corresponding to the same number of ganglion-cells. The last number is nearly one-third the number of plates in the electrical-organ, and corresponds closely to the number which should be found if the older view is the correct one, that the ganglion-cells are the centres for the nervous end-organs.

DIARY OF SOCIETIES.

LONDON.

THURSDAY, JANUARY 16.

ROYAL SOCIETY, at 4.30.—On the Chief Line in the Spectrum of the Nebulæ: Prof. J. N. Lockyer, F.R.S.—Observations on the Excretion and Uses of Bile: A. W. Mayo Robson.—On the Theory of Free Stream Lines: J. H. Michell.

LINNEAN SOCIETY, at 8.—Life-History of a Remarkable Uredine on Jasminum grandiflora: A. Barclay.—Certain Protective Provisions in some Larval British Teleosteans: E. Prince.

CHEMICAL SOCIETY, at 8.—On a New Method of estimating the Oxygen dissolved in Water: Dr. J. C. Thresh.

ZOOLOGICAL SOCIETY, at 4.

FRIDAY, JANUARY 17.

SOCIETY OF ARTS, at 8.

PHYSICAL SOCIETY, at 5.—On a Carbon Deposit in a Blake Telephone Transmitter: F. B. Hawes.—On Electric Splashes: Prof. S. P. Thompson.—On Galvanometers: Prof. W. E. Ayrton, F.R.S., T. Mather, and W. E. Sumpner.

SUNDAY, JANUARY 19.

SUNDAY LECTURE SOCIETY, at 4.—How I crossed Africa from the Indian Ocean to the Atlantic (with Oxyhydrogen Lantern Illustrations): Commander V. L. Cameron, R.N.

MONDAY, JANUARY 20.

ROYAL GEOGRAPHICAL SOCIETY, at 8.30.—Mr. J. R. W. Pigott's Journey to the Upper Tana in 1889: E. G. Ravenstein.—The Mouths of the Zambezi: Daniel J. Rankin.

SOCIETY OF ARTS, at 8.—The Electromagnet; Dr. Silvanus P. Thompson.

ARISTOTELIAN SOCIETY, at 8.—The Universals: M. H. Dziewicki.

VICTORIA INSTITUTE, at 8.—Ancient Eastern Laws in Regard to Land: Rev. J. Neil.

TUESDAY, JANUARY 21.

SOCIETY OF ARTS, at 5.—Tea, Coffee, and Cocoa Industries of Ceylon: John Loudoun Shand.

INSTITUTION OF CIVIL ENGINEERS, at 8.—Recent Dock Extensions at Liverpool: George Fosbery Lyster. (Discussion.)

ROYAL STATISTICAL SOCIETY, at 7.45.

ROYAL INSTITUTION, at 3.—The Post-Darwinian Period: Prof. G. J. Romanes, F.R.S.

UNIVERSITY COLLEGE BIOLOGICAL SOCIETY, at 5.15.—Vegetarianism: W. North.

WEDNESDAY, JANUARY 22.

SOCIETY OF ARTS, at 8.—Vision-testing for Practical Purposes: R. Brudenell Carter.

GEOLOGICAL SOCIETY, at 8.—On the Crystalline Schists and their Relation to the Mesozoic Rocks in the Lepontine Alps: Prof. T. G. Bonney, F.R.S.—The Variolitic Rocks of Mont Genèvre: Grenville A. J. Cole and J. W. Gregory.

THURSDAY, JANUARY 23.

ROYAL SOCIETY, at 4.30.

INSTITUTION OF ELECTRICAL ENGINEERS, at 8.

ROYAL INSTITUTION, at 3.—Sculpture in Relation to the Age: Edwin Roscoe Mullins.

FRIDAY, JANUARY 24.

INSTITUTION OF CIVIL ENGINEERS, at 7.30.—The Up-keep of Metalled Roads in Ceylon: Thos. H. Chapman.

ROYAL INSTITUTION, at 9.—The Scientific Work of Joule: Prof. Dewar F.R.S.

SATURDAY, JANUARY 25.

ROYAL BOTANIC SOCIETY, at 3.45.

ROYAL INSTITUTION, at 3.—The Natural History of the Horse, and of its Extinct and Existing Allies: Prof. Flower, F.R.S.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

A Search for Knowledge, and other Papers: A. N. Pearson (Melbourne).—The Magic Lantern (Perkin).—The Fauna of British India, including Ceylon and Burma; Birds, vol. i.: E. W. Oates (Taylor and Francis).—A Text-book of Animal Physiology: Dr. W. Mills (Appleton).—Our Earth and its Story, vol. iii.: edited by Dr. R. Brown (Cassell).—Geological and Natural History Survey of Canada; Annual Report, vol. iii., Parts 1 and 2, Maps, &c., to accompany ditto (Montreal).—Stanley's Explorations in Africa; a new Map (Philip).—The Scenery of the Heavens: J. E. Gore (Roper and Drowley).—Graphical Statics: L. Cremona; translated by T. H. Beare (Oxford, Clarendon Press).—Annuaire de l'Académie Royale de Belgique, 1890 (Bruxelles).

CONTENTS.

THURSDAY, JANUARY 23, 1890.

THE FUTURE INDIAN CIVIL SERVICE EXAMINATIONS.

THE importance of obtaining a satisfactory position for future science candidates in these examinations is now very great. We have not only to consider the need there is that the men selected should represent every side of modern thought and culture, but also to bear in mind the influence of such examinations on the development of education at home. It is unfortunately notorious that candidates offering science in the examinations conducted by the Civil Service Commission stand, as a rule, at a great disadvantage. The marks allotted to science subjects have often been relatively small, and even when outside pressure has secured the allotment of a fair proportion of marks to science, the methods adopted in conducting the examinations have, as has been pointed out in our columns and elsewhere, frequently been such as to prevent good candidates from actually obtaining an equitable proportion of them.

Now as the Commissioners, year by year, deal with thousands, we might say with tens of thousands, of candidates of various types and ages; and as their influence is by no means confined to the actual candidates examined, it is plain that we have in this organization a body whose influence, for good or ill, on education in this country is enormous. Therefore we regard it as most urgent that those who are familiar with this question should press the facts of the present case not only on the attention of the Civil Service Commission, but also at the India Office and on the notice of the public. We are happy to know, indeed, that the subject is being energetically taken up by a number of distinguished graduates of Cambridge. But the forces on the other side are very strong, and past experience of the action of the Commission has made it plain that the representatives of science have a serious task before them.

In their Report for 1888, the Commissioners have been at some pains to convince the public that their examinations have had a minimum disturbing effect on the ordinary course of education. For example, they show that at several recent examinations for Class I. clerkships in the home services, all, or nearly all, the successful candidates have been men of University education. The Commissioners should carry their investigations somewhat deeper, and ascertain how far these selected candidates represent all classes of University graduates. We have done this so far as opportunity has permitted; and the results of our investigation in the case of the Class I. clerkships (which alone we have at present examined, as it only affects the present question) do not bear out the contention of the Commissioners, but go to show that the examinations concerned are very distinctly calculated either to disturb the course of education or to fail to select men representing all the chief types of University culture.

From our results, which are given below, it is easy to foresee what it is that is to be feared under the coming scheme. For in the competition for Class I. clerkships, the major limit of age, twenty-four, is not far removed from that about to be adopted for future Indian Civil servants of the highest class. And in them, as we learn will be the case in the future examinations for the Indian service, no limit is placed on the number of subjects that may be selected from those which are examined.

We have before us the results of a number of these competitions held during the last ten or eleven years, and they show, as might have been expected from the scheme of marks, that science men are practically excluded. We have ascertained as far as possible the degrees taken by the successful candidates, and out of thirty we find that twenty-two have taken their degrees in classics, seven in mathematics, and one in natural science; whilst the marks of forty others, whose degrees could not be ascertained, show a similar preponderance of classical men. Now, when it is remembered that many men take honours in science at Oxford, that the number who do so at Cambridge is approaching that of those who take classical honours, and that scholarships are now given for science in considerable numbers at both Universities, it is plain that a scheme which is likely to produce such results as those we have quoted ought on no account to be adopted for the Indian Civil Service. Such a one-sided system of selection is not fair to the various classes of candidates, and it is not fair to the dependency which they will be charged to administer. The plain fact is that in the competition for the home services, the marks assigned to classics, mathematics, and science respectively are scarcely fair to mathematics, and very distinctly unfair to science. These branches of learning have been placed upon a far more equal footing at our Universities, and science candidates may fairly claim more equal treatment from the Commissioners in competitions such as those which we are now considering. In the examinations for first-class appointments in the home services, there is the enormous difference of 375 marks against science, out of 1250 in the effective mark values of classics and science. On a recent occasion the difference between the highest and lowest on the list of successful candidates was no more than 158, and although this is indeed a very exceptional case, it shows how enormous the effect of such a difference may be when the candidates are at all evenly matched.

Such a boycotting of the men of scientific training is deplorable enough in the selecting of men for the home services, but in the case of the future administrators of our Indian dependency it would be far more unfortunate. There, if anywhere, men of every type should play their part in the national work. The Cambridge men of science are doing their best to avert the catastrophe that we fear. We hope they will be supported promptly, universally, and energetically by their scientific brethren, both great and small.

THE SHAN STATES.

A Thousand Miles on an Elephant in the Shan States. By Holt S. Hallett. (London and Edinburgh: William Blackwood and Sons, 1889.)

MR. HALLETT'S journeys in Burmah, Siam, and the Shan States, in search of the best path to connect Burmah with China and Siam, were performed partly by boat, and partly on the back of elephants.

The problem before him was a difficult one, owing to the geography of Central Indo-China being unknown at the time of his visit. He has filled up a great blank in the map of this interesting region, and has proved that a practicable route for the railway exists, chiefly through great and fertile plains, to the populous parts of the Chinese province of Yunnan, and thence through Southern into Central China. The project has been for some years before the public, and has received the unanimous support of the manufacturing and mercantile communities, who have constantly, through the Chambers of Commerce, pressed the matter upon the attention of the Government. The Siamese section of the line, and several important branches, are now being surveyed and estimated for the King of Siam by English engineers, and are likely soon to be taken in hand.

The handsome volume before us contains an excellent index map of Southern China and Indo-China, five route maps, and nearly a hundred original illustrations. The index map shows clearly the projected Anglo-Siamese system of lines, and its continuation into Central China, together with the proposed branch to Pakhoi, the Southern Chinese seaport. On the same map are shown the rival lines which the French propose to construct in order to draw the trade of Southern and Central China and of the Shan States to a French port in Tonquin. The route maps, which are beautifully executed from Mr. Hallett's survey, have the population, geology, and height above sea-level of the country noted on them, which greatly increases their value. Apart from its commercial and geographical aspect, the book will prove of great interest to the politician and the general reader. It gives the account of an able, intelligent, and careful inquirer on the spot, concerning the position of the frontier of the British and Siamese Shan States at the time we annexed Upper Burmah, and it indicates the districts claimed by our new subjects which were then forcibly occupied by the Siamese. It describes the mode of government and the condition of the people in Siam and its Shan States, countries which are now being brought into close political and commercial relations with us. It treats of the threatened absorption of Siamese territory by the French, and shows how vast is our present stake in the country. It points out how imperative it is that we should pay close attention to the proceedings of the French, and safeguard our interests, which include the only known practicable route for the railway connection of Burmah with the populous and fertile regions of Southern and Western China.

The author expresses himself fluently and concisely. His descriptions of scenery, people, and wayside incidents, are extremely good, and the story of the journey is lightly, brightly, and amusingly told. He was exceptionally fortunate in his companions, and had no trouble in gaining the good-will and assistance of everyone he met during his travels. Dr. Cushing and Dr. McGilvary, who joined the party as interpreters, were masters of the Shan language, and, being missionaries, took a great interest in the welfare of the people. They had made a careful study of their manners and customs, and, having previously traversed the Shan States in various directions, were well known to the chiefs, nobles, and officials of the country. Another missionary, Mr. Wilson, who had resided at Zimmé for several years, afforded Mr. Hallett great assistance in collecting statistics and particulars of the trade of the country, and information about the religions, superstitions, and folk-lore of the various races. In the preface, Mr. Hallett gives an interesting history of the races found in Indo-China, and during his travels he collected several of their vocabularies. The aborigines of Lower Indo-China appear to have been Negritos, probably akin to those of the Andaman Islands and the hills of the Malay Peninsula. Other dwarf races of Negrito origin were met with on the journey, belonging to the Ka tribes in the neighbourhood of Luang Prabang. These are probably of the same stock as the Trao in Cochin China and one of the native races in Formosa, and are, in all likelihood, akin to the Tiao, a race of pygmies with whom the Chinese became acquainted when they entered North-Eastern China more than 4000 years ago. The Bau Lawa tribes met by him in the Shan States, and found in the hills as far south as the latitude of Bangkok, as well as the Mon race in Lower Burmah and the Cham or people of Cambodia, migrated into their present habitat at an early period, and are Mongoloid tribes of a race with Malaysian affinities. This Mon race is represented in Western Bengal and Central India by the Kolarian tribes. They are probably descendants of the Ngu stock, including the Pang, Kuei, and Miao tribes, who, with the Shan, Yang or Karen, and King or Chin tribes, formed the chief part of the population of Central and Southern China during the struggle for empire—604–220 B.C.

Other interesting tribes, known as La-Hu and Kiang Tung La-Wa, were met with by Mr. Hallett; and these are said to belong to the same white race as ourselves. They had already settled about the southern bend of the Hoang-ho at the time when the Chinese tribes arrived on the borders of China after their long journey from the neighbourhood of Chaldæa. Part of these various races have been gradually amalgamated with the Chinese, who have doubtless received from them and other races much of their folk-lore and superstitions. It may therefore prove highly interesting to compare the habits, customs, folk-lore, and superstitions of these early inhabitants of China with those of the Chinese. Many of the customs and superstitions must have been widespread at an early date. Mr. Hallett notices the strong similarity between some of the customs and superstitions of the Finnish tribes and those of the Shans. The book is rich in legends connected with various events which are said to have happened in the country. Some of these relate to the time when the Lawa were conquered or driven into the hills by the Shans; others relate to events which have since happened in the country; and the remainder are adaptations from Buddhistic stories, or refer to the guardian spirits of the country, or to romantic incidents that have occurred. The guardian spirits universally worshipped by the Shans are, strange to say, the spirits of ancient Lawa kings and queens reigning in the country at the time when wars were carried on between the Lawa and the Shans. Some of these local Sivas are believed to have ogre propensities, and formerly human sacrifices were offered up to them. Even the year previous to Mr. Hallett's visit, the execution of several criminals was hurried on in order to appease the local

Lawa spirits, so that they might be induced to allow the water needed for the irrigation of the fields to flow down from the hills. Human sacrifices at the obsequies of their chiefs were offered by the Shans up to the middle of the sixteenth century, when the States became feudatory to Burmah. At the time the chiefs were buried, elephants, ponies, and slaves were interred with them. The continuance of this custom was strictly prohibited by the Burmese Emperor Bureng Naung. Besides the legends, many humorous stories and fables are current amongst the people, specimens of which are given in the book.

Buddhism, with the Shans, as with the Chinese, is merely a cloak covering the belief in ancient superstitions, ancestral worship, and spirit worship of the people. Even the images of Buddha in the temples are believed to be inhabited by the spirits of deceased monks, and when an abbot, celebrated for his learning and virtue, dies, it is the custom for those who have spent their monastic life under his instruction to prepare a shrine for him in some part of their house, or, if still in the monastery, in their dormitory, where flowers and food are placed for the acceptance of the spirit of their deceased teacher. If he is treated with neglect or disrespect, he may become a spirit of evil towards his former pupils. This custom probably arises from the monks being celibate, and therefore having no children to carry on the ancestral worship of the family. Another peculiar practice in relation to the images of Buddha is the transferring to him of some of the attributes of the Kwan-yin, the Chinese Goddess of Mercy, the offspring of the lotus flower, who terminates the torment of souls in purgatory by casting a lotus flower on them. In China, miniature offerings are laid before this goddess as a hint for her to convey the articles implied by their likenesses to the spirits of friends or relations. The offerings, frequently accompanied by a scroll stating who the articles are for, consist of miniatures—cut out of paper—of money, houses, furniture, carts, ponies, sedan-chairs, pipes, male and female slaves, and all that one on this earth might wish for in the way of comfort. In Siam and the Shan States, there being no temple of this goddess, Buddha, who is generally depicted as sitting on a lotus flower, is besought to do her work, and similar things are heaped on his altar, but cut out of wood, or formed of rags or any kinds of rubbish, as paper is not easily obtainable. The whole country outside the villages is, according to the Shans, infested with jungle demons, the spirits of human beings who have died when absent from their homes. These endeavour to cause the death of others by the same means as caused their own. Their victims have to join the company or clan of demons to which the successful demon belongs. Thus the clan increases in numbers, and is ever becoming more potent for mischief. The people believe in divination, charms, omens, exorcism, sorcery, mediums, witchcraft, and ghosts. Witch-hunting rages throughout the country, and villages are set apart in which those accused of witchcraft must reside. Mr. Hallett noticed that the elephant-drivers every evening placed pieces of lattice-work on tall sticks stuck in the ground on the paths leading to and from the camp; and on inquiry he learned they were to entangle any evil spirit that might wish to enter the camp and injure the party. The Shans consider such precautions fully sufficient to ward off their malignant foes. The spirits, in their opinion, have as little intelligence as the birds of the air, and any scarecrow device will keep them at a distance. The spirits of those who die from abortion, miscarriage, or childbirth are much dreaded by the widower. If the child dies with the mother, its spirit joins hers in its rambles, endeavouring to harm the living. The first object of their search is the husband and father, whose death they do all they can to accomplish. Sometimes the man endeavours to escape by becoming a monk in a monastery far from his home. This belief, like most of the superstitions in Indo-China, is also current in China.

With reference to the condition of the people in the Shan States, Mr. Hallett says:—

"Nowhere in the Shan States is misgovernment and oppression of the people so rampant as in Siam. Taxation in the Shan States is exceedingly light; and the people are not placed under grinding Government masters, but have the power to change their lords at their will; they are not compelled to serve for three months in the year without receiving either wages or food; amongst them gamblers, opium-smokers, and drunkards are looked down upon and despised, and libertinism is nearly unknown. The only loose women seen by me in the Shan States were a few Siamese, who had taken up their quarters at Zimmé, the head-quarters of the Siamese judge."

Referring to Siam, he gives a fearful description of the oppression ruling in the country, and he says:—

"If it were not for slavery, serfdom, vexatious taxation and for the vices of the people, the Siamese might be a happy race. Living as they do chiefly upon vegetables and fish; in a country where every article of food is cheap; where a labourer's wages are such as to enable him to subsist upon a fourth of his earnings; where a few mats and bamboos will supply him with materials for a house sufficient to keep out the rays of the tropical sun and the showers in the rainy season; where little clothing is needed, and that of a cheap and simple kind; where nine-tenths of the land in the country is vacant, without owners or inhabitants—surely such a people might be contented and happy. The land is so fertile and the climate is so humid, that every cereal and fruit of the tropics grows there to perfection. Yet among the common people it is seldom a man or woman can be found who is not the slave of the wealthy or the noble. The Government battens on the vices of the people by granting monopolies for gambling, opium, and spirits. Government places the people under unscrupulous and tyrannical Government masters—merciless, heartless, and exorbitant leeches—who, unless heavily bribed, force the peasantry to do their three months' *corvée* labour at times and seasons that necessarily break up all habits of industry, and ruin all plans to engage in successful business. Government imposes taxes upon everything grown for human requirements in the country: fishing-nets, stakes, boats, spears, and lines, are all taxed. The Government net is so small that even charcoal and bamboos are taxed to the extent of one in ten, and firewood one in five, in kind. Fancy the feelings of an old woman, after trudging for miles to market with a hundred sticks of firewood, when twenty of the sticks are seized by the tax-gatherer as his perquisite! There is a land-tax for each crop of annuals sown, and paddy and rice are both subject to tax; so that three taxes can thus be reaped from one cereal. The burdensome taxation is levied in the most vexatious manner that can be conceived; for the taxes are let out to unscrupulous Chinamen, who are thus able to squeeze, cheat, and rob the people mercilessly. It is no use appealing from the tax-gatherer to the officials.

Money wins its way, and justice is unknown in Siam. Everyone who has not a friend at Court is preyed upon by the governors and their rapacious underlings. Such being the present state of Siam, one is not surprised to learn that the majority of its inhabitants, besides being slaves and selling their children, are libertines, gamblers, opium smokers or eaters, and given to intoxicating beverages."

Mr. Alabaster, the confidential adviser of the King of Siam, told Mr. Hallett that nine-tenths of the non-Chinese inhabitants of Bangkok were slaves; that 'squeezing" was so universal amongst the nobility, officials, and monopolists, that no man could become rich in the country unless he purchased an appointment, and thus became one of the rulers; and that justice in the courts was a farce—the heaviest purse, or the most powerful person, invariably winning the case; besides which, if a man was believed to be in possession of money, false charges were brought against him, directly or indirectly, by the officials, in order to wring the money out of him. Everyone that he questioned in Bangkok was of opinion that the state of the people could not be much worse than it was at the time of his visit. According to an inspector of police in Siamese employ, the magistrates in that city have the reputation of being the biggest liars in the country, and the police are said to be the greatest thieves, and so unsafe are the people from false charges and lawsuits, that they willingly become the slaves of the powerful in order to gain their protection.

The whole volume is replete with interesting information; we heartily commend it to the attention of our readers.

THE LESSER ANTILLES.

The Lesser Antilles. A Guide for Settlers in the British West Indies and Tourists' Companion. By Owen T. Bulkeley. (London: Sampson Low, Marston, Searle, and Rivington, Limited, 1889.)

SINCE Mr. Froude wrote on the West Indies, numerous books and pamphlets have been produced, either to show he was entirely wrong, or to supplement in some important particular the information he gave respecting these islands. The author of the little book before us took note of Mr. Froude's lament that all hand-books to the West Indies "were equally barren" of facts connected with the higher interest which the islands possess for Englishmen, and he seeks to supply the deficiency.

Although it is evident that Mr. Bulkeley has not an intimate knowledge of all the islands concerned, this is no great disparagement—especially when we recall their comparative isolation, and the general ignorance which exists even in the West Indies themselves in regard to the affairs of their neighbours.

The facts stated are generally trustworthy, and the hints given to visitors and intending settlers are likely to be useful. There are a moderately good map and some twenty illustrations, most of which, however, are already familiar to us. Although usually grouped together, the several islands in the Lesser Antilles differ much more from each other than is usually supposed. One end of the chain, at the Virgin Islands, touches 19° N. lat., while the other end at Trinidad is in 10° N. lat. Hence, the extreme points of the Lesser Antilles are about six hundred miles apart, and there is such a diversity of soil and climate that each island really requires separate treatment.

There is still much misconception in the mind of the British public as regards the healthiness of these islands, and also as regards their suitability for settlers with a small capital. If there were someone in this country whose business it was to give accurate information respecting the West Indies, they would probably be greatly benefited.

The revival of interest in these islands, and the large number of people who annually visit them, are facts which have naturally led to the production of a guide-book. Mr. Bulkeley has, however, aimed at producing something more than a guide-book. The greater part of the volume is devoted to a minute description of the physical features, and the circumstances of the several islands, and this is followed by information for intending settlers, with the view of inducing those who have capital to invest to make their homes in these islands. While we cannot endorse all Mr. Bulkeley's statements on this latter point, it is only right to say that none of them are positively misleading, and at all times they are discussed with a modesty, and an evident desire to arrive at a right conclusion, that disarms criticism.

Besides the sugar-cane and cocoa-nut palms, there are industries connected with fruits, fibres, spices, annatto, arrow-root, pepper, maize, medicinal plants, scent-producing plants, coca, ramie, tea, tobacco, and many others well suited to the soil and climate.

It is well known that in former days large fortunes were made by sugar planters in the West Indies. Now, however, even the best estates do little more than give a small return on the capital invested, while many cannot even do this. It would be unwise, therefore, for the West Indies to confine their attention exclusively, or, indeed, largely, to the sugar-cane. Already a change is taking place. Jamaica has pimento, coffee, tropical fruits, cinchona, dye-woods, annatto, cacao; Trinidad has cacao, cocoa-nuts; Grenada is almost exclusively cacao and spices; Montserrat is noted for its lime plantations and lime-juice; while Dominica exports concentrated lime-juice, cacao, cocoa-nuts, as well as oranges to the neighbouring islands. The tendency is for the cultivation of the West Indies to become more and more diversified, and it is well it should be so.

With such good markets for produce of all kinds in the United States and Europe, it is evident that West Indian planters could regain much of their former prosperity if only they adapted themselves to the new order of things. To assist them in the development of new industries, Government botanical gardens are in course of being established, under the auspices of Kew, in every island, and from these new plants and information respecting their cultivation are being widely distributed. In such a work enterprising governors, such as the late Sir Anthony Musgrave, and the present Governor of Trinidad, Sir William Robinson, and others, have taken an active part. It is not, however, as regards industrial subjects only that interest in the West Indies has revived of late. The publication of Grisebach's "Flora of the British West Indian Islands" in 1864 (one of the series of colonial

flora projected by the late Sir William Hooker) was for a long time the only effort made in the cause of botanical science in this part of the world. Since that time, both the fauna and flora have received systematic attention in this country and in the United States, and after a lapse of nearly two hundred years we are beginning to have a clear idea of the distribution of life in the Caribbean Archipelago.

A Joint Committee of the Government Grant Committee of the Royal Society and of the British Association, has been engaged for the last three years in investigating ascertained deficiencies in the fauna and flora. Almost every page of Mr. Bulkeley's work affords ample evidence of the aid he has received, directly or indirectly, from the botanical efforts of recent years. More, however, might have been said of the special plants which are characteristic of the several islands, and which contribute so large a share to the interest of daily life in them.

It is to be hoped the day is not far distant when this first unpretentious guide-book to the Lesser Antilles will be followed by others, not less interesting, but still more fully meeting the requirements of those who may visit them for pleasure, or go to them in the hope of pursuing some of the numerous industries opened to settlers in these beautiful islands.

D. M.

A TEXT-BOOK OF HUMAN ANATOMY.

A Text-book of Human Anatomy, Systematic and Topographical. Including the Embryology, Histology, and Morphology of Man, with special reference to the requirements of Practical Surgery and Medicine. By Alex. Macalister, M.A., M.D., F.R.S., Professor of Anatomy in the University of Cambridge. (London: Charles Griffin and Co., 1889.)

WHEN it was announced some time ago that the Professor of Anatomy in the University of Cambridge was engaged in writing a systematic work on Human Anatomy, its publication was looked for with anticipation and interest. Prof. Macalister deservedly enjoys a high reputation as a man of remarkable culture in many branches of knowledge, and as an anatomist in the comprehensive meaning of the term. Curiosity was excited, therefore, as to the mode in which he would treat the subject: whether he would follow the old lines pursued by so many of those who have preceded him in the writing of text-books, or if he would strike out a new path for himself.

In his preface he tells us that he has endeavoured to give a comprehensive account of the Anatomy of Man studied from the Morphological standpoint. Accordingly, we find that, after a few explanatory paragraphs on the meaning of terms used in description, he proceeds to state his conception of a Cell. His definition is so comprehensive that he regards it in its simplest form as a minute speck of protoplasm without either nucleus or cell-wall; and, in this respect, he may be said to coincide with the view held by Stricker in his well-known article on the Cell. He then briefly describes the process of Karyokinesis, and very properly states that the study of the specialization of the products of cell multiplication is the only trustworthy guide to the solution of the many morphological problems which Human Anatomy presents. This very naturally leads to an account of the Development of the Embryo, which is, however, compressed into so few pages that we doubt whether a beginner can derive from it a clear conception of the very elaborate set of changes which lead from the simple laminated blastoderm to the form of the fœtus at the time of birth.

A chapter on Histology or tissue-anatomy comes next in order. He groups the tissues into five classes—epithelial or surface limiting; connective or skeletal; nervous or sensory; muscular or contractile; blood and lymph or nutritive. This classification is both simple and convenient, and is much to be preferred to the grouping into cellular, fibrous, membranous and tubular tissues, sometimes adopted. In the course of this chapter he in part fills up some of the gaps in the section on embryology, by describing the development of the nervous and vascular systems.

The skeleton is next described, and following the plan pursued by Prof. Humphry in his well-known treatise, and by Hyrtl, Gegenbaur, Krause, and others in their systematic works, he describes the joints and ligaments along with the bones with which they are associated. This arrangement, undoubtedly, has certain advantages more especially in the direction of economizing space in description.

About one-third of the work, extending to 248 pages, is occupied with the chapters to which we have just referred, and the remaining two-thirds is devoted to an account of the soft parts, including the anatomy of the brain and organs of sense. In this, the larger division of his text-book, Prof. Macalister alters his mode of treating the subject, and departs from the method which systematic writers are in the habit of pursuing.

The rule, almost without exception, has been to describe in separate chapters the muscular, vascular, nervous, alimentary, respiratory, and genito-urinary systems, so as to bring before the student in a continuous series all those organs which possess corresponding properties. To some extent, therefore, the arrangement adopted in our text-books of systematic anatomy has had a physiological basis.

Dr. Macalister has not followed this plan. He has adopted an arrangement on a topographical basis, *i.e.* according to the method pursued in the dissecting-room, in which the student works out for himself the constituent parts of the body as he displays them in the course of his dissections. This method of studying the anatomy of the human body is, as everyone will admit, of enormous importance—indeed, we may say of primary value—to the practitioner of medicine and surgery. But it is the custom of the schools to distinguish between the analytical or dissecting-room method, in which the body is picked to pieces by the dissector himself, and the synthetical or systematic method, in which the body is, as it were, built up by the teacher for the student. This custom is the fruit of long experience, for whilst giving full value to the topographical or regional aspect of anatomy, it enables the teacher to show to the student the continuousness of such systems as the vascular, nervous, and alimentary, and to point out their physiological relations. For it should be kept in mind that anatomy is the basis of physiology, as well

as the foundation of that side of medical and surgical practice which is based on a sound knowledge of regional anatomy. The incomplete recognition of the physiological aspect of anatomy is, we think, the weak part of the book, and it is especially shown in the scanty notice which is taken of the action of the muscles and their association with the movements of the joints.

To enable both these lines of anatomical study to be pursued, the student is accustomed to employ at least two text-books; the one in connection with his systematic work, the other as a guide to the dissection of the body. Prof. Macalister apparently expects, as, indeed, he states in his preface, that his text-book should stand in the place of the two customarily employed. We doubt, however, whether this expectation will be fulfilled. For his text-book, in addition to what is essential in topographical description, by containing an account of the microscopic structure of tissues and organs, a section on embryology, and a detailed description of the bones, is necessarily a work of considerable size and weight, and too cumbersome to be conveniently carried to and fro by the student, as is required with a dissecting-room manual. On the whole, therefore, we prefer the old and well-accustomed lines on which text-books have for so long been written, to Prof. Macalister's modified plan.

But whilst expressing our inability to regard the method which has been followed in the descriptive anatomy of the soft parts as an improvement on the customary arrangement of systematic text-books, we recognize with pleasure the clearness of the descriptions and the many suggestive hints, both morphological and practical, which the book contains. The volume is profusely illustrated with upwards of eight hundred wood-cuts, about one half of which are original figures.

OUR BOOK SHELF.

A Treatise on Ordinary and Partial Differential Equations. By W. W. Johnson. (London: Macmillan, 1889.)

WE have read Prof. Woolsey Johnson's work with some interest: his style is clear, and the worked-out examples well adapted to elucidate the points the writer wishes to bring out. He appears to recognize Boole, but, so far as the text is concerned, does not acknowledge the existence of Mr. Forsyth's fine work. We do not say that he was under any obligation to do so, but nowadays we are so accustomed to see a list of authors upon whom a writer has drawn that we missed it here. "An amount of space somewhat greater than usual has been devoted to the geometrical illustrations which arise when the variables are regarded as the rectangular co-ordinates of a point. This has been done in the belief that the conceptions peculiar to the subject are more readily grasped when embodied in their geometric representations. In this connection the subject of singular solutions of ordinary differential equations, and the conception of the characteristic in partial differential equations may be particularly mentioned." This is certainly the most prominent feature of the early chapters, and it is, to our mind, clearly put before the student. Reference is duly made to Prof. Cayley's work in the *Messenger of Mathematics* (vol. ii.), which initiated the present mode of treatment of the subject, but not to Dr. Glaisher's "Illustrative Examples" (vol. xii.), nor to Prof. M. J. M. Hill's paper (London Math. Soc. Proc., vol. xix.), in which the theorems *stated* by Prof. Cayley are proved. This paper, though read before the Society, June 14, 1888, may not have reached the author before his work was in the printer's hands: we do not say that a perusal of it would have called for any further notice than a reference. Symbolic methods come in for their due meed of recognition and employment. The author satisfies himself with referring the student to the table of contents for the topics included and the order pursued in treating them. The work consists of twelve chapters divided up into twenty-four sections: i. (1) discusses the nature and meaning of a differential equation between two variables; ii. (2, 3, 4,) equations of the first order and degree; iii. equations of the first order, but not of the first degree, (5) singular solutions (discriminant, cusp-, tac-, and node-loci), (6) Clairaut's equation, (7) geometrical applications, orthogonal trajectories; iv. (8) equations of the second order; v. (9, 10) linear equations with constant coefficients, in (10) symbolic methods are employed; vi. (11–13) linear equations with variable coefficients; vii. (14, 15) solutions in series; viii. (16) the hypergeometric series; ix. (17) special forms of differential equations, as Riccati's equation (due reference is made to Dr. Glaisher's classical paper in the Phil. Trans. for 1881), Bessel's equation, and Legendre's equation (reference is made to text-books and memoirs); x. (18–20) equations involving more than two variables; xi. (21, 22) partial differential equations of the first order; xii. (23, 24) partial differential equations of higher order. Examples for practice are added at the end of each section. Though Prof. Johnson cannot lay claim to have made here any additions to our knowledge of the subject, he has produced an excellent introductory hand-book for students, and this, we expect, was the object he proposed to himself in its compilation. We have omitted to state that all use of the complex variable is eschewed.

The Land of an African Sultan: Travels in Morocco, 1887, 1888, and 1889. By Walter B. Harris, F.R.G.S. (London: Sampson Low and Co., 1889.)

A GOOD deal has been written about Morocco lately, and Mr. Harris's volume is an interesting, although not a very important, contribution to the literature of the subject. He describes first a journey through northern Morocco, then a journey with H.B.M. Special Mission to the court of the Sultan at Morocco city, next a visit to Wazan and a ride to Sheshuan; and in a final chapter he sums up the impressions produced upon him by the Moors and their country. In the chapter on his ride to Sheshuan, he describes a place which had been "only once before looked upon by Christian eyes." Mr. Harris does not pretend to have produced an exhaustive work on Morocco; but he presents clearly what he himself has had opportunities of observing.

Wayside Sketches. By F. Edward Hulme, F.L.S., F.S.A. (London: Society for Promoting Christian Knowledge, 1889.)

THIS is a pleasantly conversational book on all sorts of subjects more or less connected with natural history or country life: birds, caterpillars, flowers, snow-crystals, and the forms of clouds, all come in for a share of attention. Without having any scientific pretensions of its own, the book may well serve to rouse a first interest in many branches of science. The numerous illustrations are very good indeed.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Influenza.

THE following paragraph, taken from Sir David Brewster's "Life of Sir Isaac Newton," is not uninteresting at the present time:—

"Some light has been recently thrown on the illness of Newton by Dr. Dowson, of Whitby, who, at a meeting of the Philosophical Society there on the 3rd of January, 1856, read a paper 'On the Supposed Insanity of Sir Isaac Newton,' in which he has shown that the malady with which he was afflicted in September 1693 was probably influenza or epidemic catarrhal fever, which prevailed in England, Ireland, France, Holland, and Flanders in the four last months of 1693. This distemper, which lasted from eight or ten days to a month, was so general, that 'few or none escaped from it'; and it is therefore probable, as Dr. Dowson believes, that Newton's mental disorder was merely the delirium which frequently accompanies a severe attack of influenza. See Dr. Theophilus Thomson's 'Annals of Influenza or Epidemic Catarrh in Great Britain,' published in 1852 by the Sydenham Society. See also the Philosophical Transactions for 1694, vol. xviii. pp. 105–115." W. GREATHEED.

ABOUT forty-five years ago I paid a visit with a friend to the laboratory of the celebrated chemist Prof. Schonbein, the discoverer of ozone in the atmosphere and the cause of influenza. Just prior to our visit the Professor had obtained some ozone, and had inhaled it for the purpose, as he said, of giving himself influenza, in order to ascertain how it would affect him. We both distinctly observed most of the ordinary symptoms of the malady. AUGUSTUS HARVEY.

12 Landridge Road, Fulham, January 17.

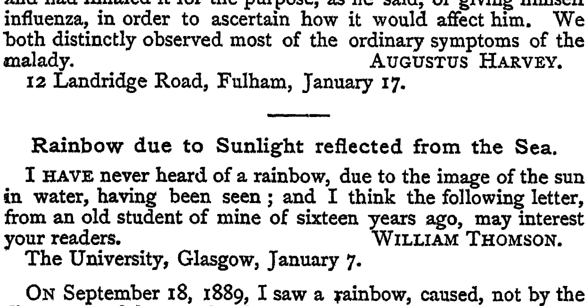
Rainbow due to Sunlight reflected from the Sea.

I HAVE never heard of a rainbow, due to the image of the sun in water, having been seen; and I think the following letter, from an old student of mine of sixteen years ago, may interest your readers. WILLIAM THOMSON.

The University, Glasgow, January 7.

ON September 18, 1889, I saw a rainbow, caused, not by the direct rays of the sun, but by their reflection from the sea.

We were at the height of 900 feet; the sky was all clouded except along the western horizon; the sun, an hour before setting, was hidden; but its rays were reflected from the sea. A drizzle was falling, and my companion was remarking how strong the light from the sea was, when it occurred to me that it might give a bow. And there it was behind us—not the usual recumbent bow, less than a semicircle, but an overhanging one, greater than a semicircle. The clouds were drifting from the west, so that the sun came into view; and the usual rainbow became visible with its secondary bow; *so that three rainbows were seen at once.* The sea-bow and the usual bow were identical at the horizon. The angle between them was greater than the sun's



angular height, but not double. It seemed as if the complementary segment of the rim had been folded up from beneath into view, but that the colours were not reversed. The sea-bow was just as bright as the secondary bow, which it intersected.

From the fact that the three were seen together, for over 3 minutes, at least in part, I would argue that it is no unusual sight, and that in Scotland, where bows are so frequent, and plenty of comparatively smooth water available, this sea-bow may be looked for and seen.

I may mention, also, that I saw a fourth bow that evening. After the sun had set, a bow of one colour, an orange-pink, took the place of the usual bow. The source of light, I thought, was a cloud just over the place where the sun had set.

WILLIAM SCOULLER.

86 Calle de la Independencia, Valparaiso, November 9, 1889.

Osteolepidæ.

YOUR reviewer R. L. is mistaken in condemning so absolutely the above form. The word "Osteolepus" would be a legitimate adjective expressing the same idea as the substantive Osteolepis; and the patronymic of the "Osteolepi" would be simply "Osteolepidæ," and not "Osteolepididæ."

It may be useful for R. L. and some others to apprehend this principle in word-building—viz. that compound Greek adjectives do *not* take the lengthened genitive as root; thus the correct Latin equivalent for the corresponding Greek adjective is *not* "echinodermatus" but "echinodermus," not "distomatus" but "distomus." Hence, the correct form for the neuter plural of the former is "Echinoderma;" and for the neuter singular of the latter is Distomum. And it would be wrong to write "Distomatidæ" as the family name, and correct to write "Distomidæ." Hence Osteolepidæ and the like are admissible, since they may be considered as formed from adjectives, and not from the substantive (of questionable form itself) in -is.

R. L. + E.

Exact Thermometry.

SINCE the publication of my letter in NATURE (December 19, 1889, p. 152) on the cause of the rise of the zero-point of a thermometer when exposed for a considerable time to a high temperature, two letters on the same subject have appeared, one from Mr. Herbert Tomlinson (January 2, p. 198), the other from Prof. E. J. Mills (January 9, p. 227), who replies to my objections to the plastic theory.

Mr. Tomlinson considers that my experiments seem to leave no doubt that compression, due to the plasticity of the glass, is not the main cause of the rise of the zero-point, but he considers that it is not merely the prolonged heating, but also the change of temperature (heating or cooling), that is effective in bringing about the change. I have not yet had time to make any special experiments to test this point, but I may perhaps mention that such data as I possess seem rather to point to the conclusion that long-continued steady heating is more effective than alternate heating or cooling. As the following experiment, made about a year ago, seems to bear on the point, I give the results:—

									Total rise of zero
Approximate time in hours.	6	3	6	6	6	31	6	6	
Rise of zero-point	$1^\circ \cdot 6$	$0^\circ \cdot 15$	$0^\circ \cdot 85$	$0^\circ \cdot 5$	$0^\circ \cdot 1$	$1^\circ \cdot 2$	0°	0°	$4^\circ \cdot 4$

Two other thermometers, heated each day for about six hours, showed after nine days rises of zero-point of $3^\circ \cdot 8$ and $4^\circ \cdot 1$ respectively, but in these cases the change was apparently not quite complete. The temperature was in each case 280°, and all these thermometers belonged to the same batch as those employed in my experiments already described in NATURE.

Prof. Mills does not regard the experiments as conclusive, but criticizes my results in the following words: "The zero movement, however, only ranged from 1° to $1^\circ \cdot 2$—small readings which might very possibly have been obtained, or not, on either of the thermometers at other times." This criticism, in striking contrast to the rest of the letter, appears to be rather unkind either to me or to my thermometers, I hardly know which. I sincerely hope that none of my thermometers are capable of such erratic behaviour as to show changes of zero-point of 1° (or even twice this amount if the plastic theory is correct) without extraordinary treatment, or that my readings of temperature are reliable only to within 1° or so. But to make the matter more certain, I will continue the heating of the two thermometers, A and C, under the same conditions as before, and will also heat two more thermometers under similar conditions to about 360°.

Prof. Mills mentions the very curious behaviour of lead-glass thermometers at different temperatures, but his objection on that score to the temperature 280° does not seem to apply, as my thermometers are all made of soft German soda-glass. It may, however, be useful to heat two more thermometers to a temperature of about 220° in order to compare the total rise with that at 280° and 360°.

With regard to the statement that the final state of a thermometer kept at the ordinary temperature for an infinite time would differ from that of the same thermometer after being subjected to prolonged heating at a high temperature, I am not prepared to give a decided opinion either one way or the other, but it does appear to me to be rather a daring procedure to make observations of the minute changes of zero-point over a few years, and to extrapolate from a decade or so to eternity.

I am also quite willing to admit that there may be other causes tending to raise the zero-point besides the equalization of tension, such, for instance, as the chemical changes alluded to by Prof. Mills; but I should like to ask, as I am ignorant on the point, whether there is any experimental evidence of their nature or existence. SYDNEY YOUNG.

University College, Bristol, January 11.

Foreign Substances attached to Crabs.

IN your issue of December 26, and also in exhibiting his collection of crabs before the Linnean Society, Mr. Pascoe cast some doubt on the function of the two pairs of modified legs of *Dromia vulgaris*, which are usually supposed to be adapted to the retention of the sponge with which it covers its carapace.

That these legs were really used for this purpose I was enabled to observe, during my stay at the zoological station in Naples last winter. I had in my tank several specimens, in some of which the sponge had also extended on to the ventral surface, over the edge of the carapace, thus securing a firm hold apart from the action of the legs. In all specimens, however, there are seen, when the sponge is removed, which requires considerable force, two oblique depressions into which the legs fit, giving them thus a distinct hold on the sponge.

If the latter be, however, removed from the animal but left in the tank, the crab soon sets to work to regain possession of its covering, and can be seen to use its modified hinder pairs of legs most effectually for this purpose. It would seem therefore beyond doubt that these modified legs serve not only for holding on the sponge, but also for getting hold of a new sponge, should the old one get injured or die, as must happen not unfrequently.

F. ERNEST WEISS.

The Zoological Laboratory, University College, January 6.

Galls.

I AM sorry if I unintentionally misrepresented the opinions of Prof. Romanes and Dr. St. George Mivart in suggesting that they wished to assail the theory of natural selection in their recent communications to NATURE on this subject. They must, however, pardon me for saying that I still think the extract to which I alluded in my note admits this interpretation. As my views of the relations of gall-formation to the theory of natural selection are clearly at variance with those of your correspondents, perhaps you will allow me space to give briefly the grounds upon which I base my conclusions.

There are in England about ninety well-known varieties of galls, and of this number fully a third are found in the oak. About half the oak-galls are formed on growing leaves. In nearly one-third of the total number the grub is hatched, and the gall is fashioned in a developing bud. We can readily imagine, in the case of a tree with deciduous leaves, that the presence of a few galls upon its foliage would not greatly affect its chances of survival, if its fitness was in other respects complete. It is otherwise when a gall occupies the position of a developing bud, especially when the bud is a terminal one. In this case there occurs coincidently with, and as a result probably of, the adventitious formation, an arrest of normal development and growth. Indeed, I believe "the gnarled and twisted oak" owes many of its gnarls and most of the twists to the common oak-apple and other bud-galls. If a tree endowed with less developmental vigour and with fewer supplementary buds than the oak had been exposed to the repeated attacks of the insects for many generations in a struggle for existence, it would doubtless have long ago succumbed, and it would have done so by a process of natural selection operating in the ordinary manner, and not "at the end of a long lever of the wrong kind," whatever that may mean. This selective process in the case of gall-bearing trees has left possible traces of its action to-day, for I am unaware that any other English tree than the oak is attacked by terminal bud-galls. The terminal leaf-galls of certain Salices and Conifers can scarcely affect their growth and development to the same extent as the bud-galls.

When we compare pathological tumours in the higher animals with these vegetable excrescences, we must make due allowances for the different conditions under which each lives. I cannot then see that the "morphological specialization" of galls, which, for the most part, are composed of hypertrophied reproductions of the simpler vegetable tissues, is greater than that exhibited by man himself, when, for instance, he becomes the involuntary host of Dr. Lewis's *Filariæ*, and his leg the seat of *Elephantiasis lymphangiectodes*, accompanied by hypertrophy of many integumentary structures of the limb. Oak-spangles, on the other hand, are to my mind comparable to the circular nests of ringworm, or to the sprouting epithelium of a *Verruca necrogenetica*. Such comparisons may be of little scientific value, yet I take it they are as useful in their place as attempts to gauge the amount of "disinterestedness" shown by a cabbage when it becomes the unwilling host of the gall-producing *Ceuthorhynchus sulcicollis*. W. AINSLIE HOLLIS.

Brighton, December 30, 1889.

The Evolution of Sex.

THE interesting note of Mr. M. S. Pembrey in your issue of January 2 (p. 199), induces me to draw the attention of your correspondent to a short paper of mine just published (or in course of publication) in the *Ibis*, where I communicated the experiences of a friend, who had hatched a series of parrot eggs, belonging to the genus *Eclectus*, in which the young males are green, the young females red. It is remarkable that by far the larger number of the birds hatched were males. In each case only two eggs were laid, and the breeder himself, without being able to tell why, is of opinion that nearly all his hatches consisted of male birds. As there are still many embryos of those *Eclectus* in my hands, the sex of which is not yet determined, I hope to be able to make known the result of my investigation later, whether the pairs are always males, or always females, or consist of a male and a female bird, at least sometimes. Meanwhile, I should be glad to hear if anything more is known about the sexes of birds which lay only two eggs at a time.

A. B. MEYER.

Royal Zoological Museum, Dresden, January 5.

"Manures and their Uses."

ALLOW me to thank the well-known writer "W." for his review of the above-mentioned book. "W." does not hold with the view that "farmyard manure is erroneously supposed to contain *all* the necessary plant-foods required for the growth of plants." I believe, with M. Ville and others, that "the farmer who uses nothing but farmyard manure exhausts his land." "W." speaks of this as an "obvious fallacy." If the statement is wrong, would "W." kindly answer the quotation given on p. 76 of the book in question. The quotation "runs" as follows:—

"M. Grandeau (the French agricultural authority) recently estimated that one year's crop in France represents 298,200 tons of phosphoric acid, of which only 151,200 tons were recovered from the stable dung, thus leaving a deficit of 147,000 tons, equal to over one million tons of superphosphate, to be made good by other means.

"M. Grandeau also estimated that the entire number of farm animals in France in 1882, representing a live weight of 6,240,430 tons, had accumulated from their food 193,453 tons of mineral matter containing 76,820 tons of phosphoric acid. These figures give some idea of the enormous quantities of phosphoric acid required to restore to the soil what is continually being carried away by the crops sold off the farm."

It must be borne in mind that in the above estimates, M. Grandeau includes the purchase of oil-cakes and other feeding stuffs. Therefore, if farmyard manure only contains about half the amount of phosphoric acid (to say nothing of nitrogen, potash, &c.) required to retain the land in a fertile condition, how can I have attached "too much prominence to chemical manures, and too little importance to stock-feeding as a manurial agency"? A. B. GRIFFITHS.

[DR. GRIFFITHS assumes that because, as asserted by M. Grandeau, the balance of fertilizing matter in France is against the land, "the farmer who uses nothing but farmyard manure exhausts his land." This is arguing from general principles to special cases, and there is no sequence in his reasoning. A nation may be rushing to ruin, but that does not prevent an individual from growing rich. Phosphates and nitrates may be diminishing, but that does not prevent them from accumulating on any particular farm. We traverse Dr. Griffiths's statement without qualification, that the farmer who uses nothing else but farmyard manure exhausts his land. We believe he improves his land.—THE REVIEWER.]

MAGNETISM.[1]

II.

WHEN one considers that the magnetic property is peculiar to three substances—that it is easily destroyed by the admixture of some foreign body, as manganese—one would naturally expect that its existence would depend also on the temperature of the body. This is found to be the case. It has long been known that iron remains magnetic to a red heat, and that then it somewhat suddenly ceases to be magnetic, and remains at a higher temperature non-magnetic. It has long been known that the same thing happens with cobalt, the temperature of change, however, being higher; and with nickel, the temperature being lower. The magnetic characteristics of iron at a high temperature are interesting. Let us return to our ring, and let us suppose that the coils are insulated with a refractory material, such as asbestos paper, and that the ring is made of the best soft iron. We are now in a position to heat the ring to a high temperature, and to experiment upon it at high temperatures in exactly the same way as before. The temperature can be approximately determined by the resistance of one of the copper coils. Suppose, first, that the current in the primary circuit which we use for magnetizing the ring is small; that from time to time, as the ring is heated and the temperature rises, an experiment is made by reversing the current in the primary circuit, and observing the deflection of the galvanometer needle. At the ordinary temperature of the air the deflection is comparatively small; as the temperature increases the deflection also increases, but slowly at first; when the temperature, however, reaches something like 600° C., the galvanometer deflection begins very rapidly to increase, until, with a temperature of 770° C., it attains a value of no less than 11,000 times as great as the deflection would be if the ring had been made of glass or copper, and the same exciting current had been used. Of course, a direct comparison of 11,000 to 1 cannot be made: to make it, we must introduce resistance into the secondary circuit when the iron is used; and we must, in fact, make use of larger currents when copper is used. However, the ratio of the induction in the case of iron to that in the case of copper, at 770° C., for small forces is no less than 11,000 to 1. Now mark what happens. The temperature rises another 15° C.: the deflection of the needle suddenly drops to a value which we must regard as infinitesimal in comparison to that which it had at a temperature of 770° C.; in fact, at the higher temperature of 785° C. the deflection of the galvanometer with iron is to that with copper in a ratio not exceeding that of 1·14 to 1. Here, then, we have a most remarkable fact: at a temperature of 770° C. the magnetization of iron 11,000 times as great as that of a non-magnetic substance; at a temperature of 785° C. iron practically non-magnetic. These changes are shown in Fig. 8. Suppose now that the current in the primary circuit which serves to magnetize the iron had been great instead of very small. In this case we find a very different order of phenomena. As the temperature rises, the deflection on the galvanometer diminishes very slowly till a high temperature is attained; then the rate of decrease is accelerated until, as the temperature at which the sudden change occurred for small forces is reached, the rate of diminution becomes very rapid indeed, until, finally, the magnetism of the iron disappears at the same time as for small forces. Instead of following the magnetization with constant forces for varying temperatures, we may trace the curve of magnetization for varying forces with any temperature we please. Such curves are given in Diagrams 9 and 10. In the one diagram, for the purpose of bringing out different points in the curve, the scale of abscissæ is 20 times as great as in the other. You will observe that the effect of rise of temperature is to diminish the maximum magnetization of which the body is capable, slowly at first, and rapidly at the end. It is also very greatly to diminish the coercive force, and to increase the facility with which the body is magnetized. To give an idea of the magnetizing forces in question, the force for Fig. 8 was 0·3; and as you see from Figs. 9 and 10, the force ranges as high as 60. Now the earth's force in these latitudes is 0·43, and the horizontal component of the earth's force is 0·18. In the field of a dynamo machine the force is often more than 7000. In addition to the general characteristics of the curve of magnetization, a very interesting, and, as I take it, a very important, fact comes out. I have already stated that if the ring be submitted to a great current in one direction, which current is afterwards gradually reduced to zero, the ring is not in its non-magnetic condition, but that it is, in fact, strongly magnetized. Suppose now we heat the ring, whilst under the influence of a strong magnetizing current, beyond the critical temperature at which it ceases to have any magnetic properties, and that then we reduce the current to

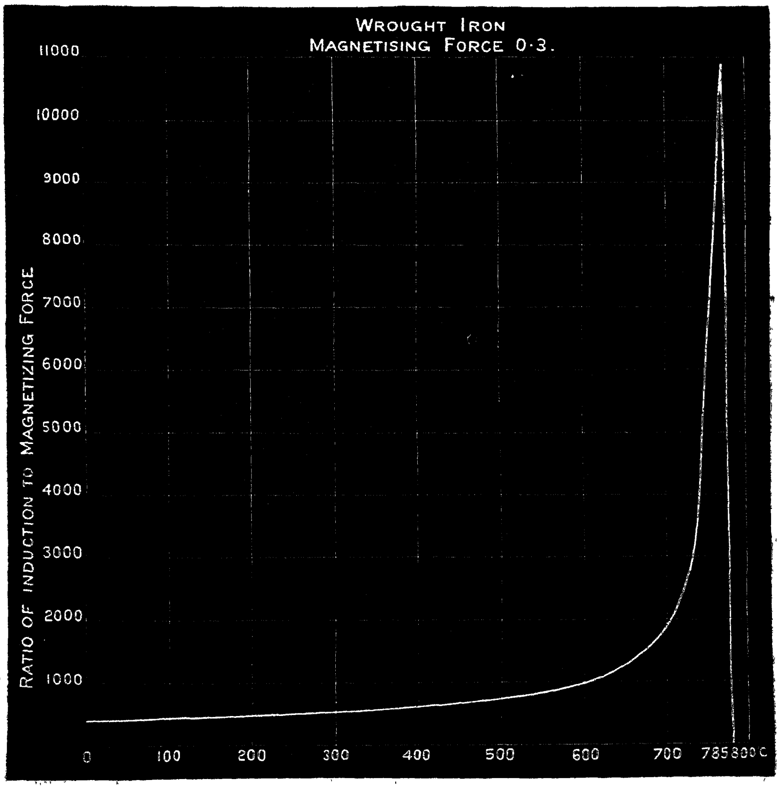


FIG. 8.

[1] Inaugural Address delivered before the Institution of Electrical Engineers, on Thursday, January 9, by J. Hopkinson, M.A., D.Sc., F.R.S., President. Continued from p. 254.

zero, we may in this state try any experiment we please. Reversing the current on the ring, we shall find that it is in all cases non-magnetic. Suppose next that we allow the ring to cool without any current in the primary, when cold we find that the ring is magnetized; in fact, it has a distinct recollection of what had been done to it before it was heated to the temperature at which it ceased to be magnetic. When steel is tried in the same way with varying temperatures, a similar sequence of phenomena

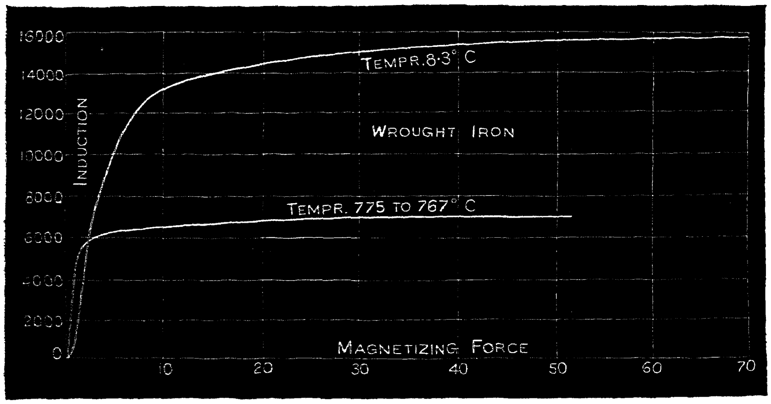


FIG. 9.

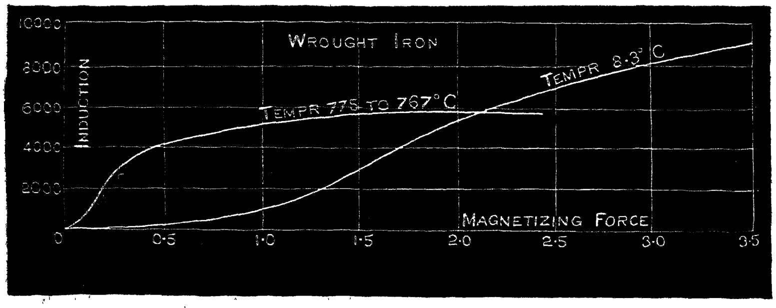


FIG. 10.

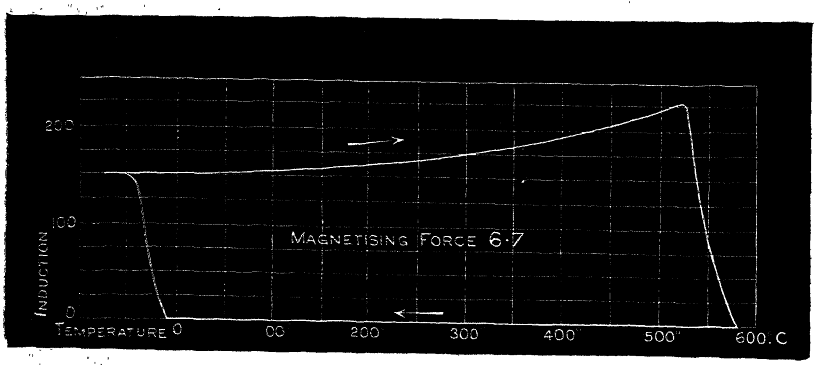


FIG. 11.

is observed; but for small forces the permeability rises to a lower maximum, and its rise is less rapid. The critical temperature at which magnetism disappears changes rapidly with the composition of the steel. For very soft charcoal iron wire the critical temperature is as high as 880° C.; for hard Whitworth steel it is 690° C.

The properties of an alloy of manganese and iron are curious. More curious are those of an alloy of nickel and iron. The alloy of nickel and iron containing 25 per cent. of nickel is non-magnetic as it is sure to come from the manufacturer; that is to say, a substance compounded of two magnetic bodies is non-magnetic. Cool it, however, a little below freezing, and its properties change: it becomes very decidedly magnetic. This is perhaps not so very remarkable: the nickel steel has a low critical temperature—lower than we have observed in any other magnetizable body. But if now the cooled material be allowed to return to the ordinary temperature it is magnetic; if it be heated it is still magnetic, and remains magnetic till a temperature of 580° C. is attained, when it very rapidly becomes non-magnetic, exactly as other magnetic bodies do when they pass their critical temperature. Now cool the alloy: it is non-magnetic, and remains non-magnetic till the temperature has fallen to below freezing. The history of the material is shown in Fig. 11, from which it will be seen that from −20° C. to 580° C. this alloy may exist in either of two states, both quite stable—a magnetic and a non-magnetic—and that the state is determined by whether the alloy has been last cooled to −20° C. or heated to 580° C.

Sudden changes occur in other properties of iron at this very critical temperature at which its magnetism disappears. For example, take its electrical resistance. On the curve, Fig. 12, is shown the electrical resistance of iron at various temperatures, and also, in blue, the electrical resistance of copper or other pure metal. Observe the difference. If the iron is heated, its resistance increases with an accelerating velocity, until, when near the critical temperature, the rate of increase is five times as much as the copper; at the critical temperature the rate suddenly changes, and it assumes a value which, as far as experiments have gone, cannot be said to differ very materially from a pure metal. The resistance of manganese steel shows no such change; its temperature coefficient constantly has the value of 0·0012, which it has at the ordinary temperature of the air. The electrical resistance of nickel varies with temperature in an exactly similar manner. Again, Prof. Tait has shown that the thermo-electric properties of iron are very anomalous—that there is a sudden change at or about the temperature at which the metal becomes non-magnetic, and that before this temperature is reached the variations of thermo-electric property are quite different from a non-magnetic metal.

Prof. Tomlinson has investigated how many other properties of iron depend upon the temperature. But the most significant phenomenon is that indicated by the property of recalescence. Prof. Barrett, of Dublin, observed that if a wire of hard steel is heated to a very bright redness, and is then allowed to cool, the wire will cool down till it hardly emits any light at all, and that then it suddenly glows out quite bright again, and afterwards finally cools. This phenomenon is observed with

great difficulty in the case of soft iron, and is not observed at all in the case of manganese steel. A fairly approximate numerical measurement may be made in this way: Take a block of iron or steel on which a groove is cut, and in this groove wind a coil of copper wire insulated with asbestos; cover the coil with many layers of asbestos; and finally cover the whole lump of iron or steel with asbestos again. We have now a body which will heat and cool comparatively slowly, and which will lose its heat at a rate very approximately proportional to the difference of temperature between it and the surrounding air. Heat the block to a bright redness, and take it out of the fire and observe the resistance of the copper coil as the temperature falls, due to the cooling of the block. Plot a curve in which the abscissæ are the times, and the ordinates the logarithms, of the increase of resistance of the copper coil above its resistance at the temperature of the room. If the specific heat of the iron were constant, this curve would be a straight line; if at any particular temperature latent heat were liberated, the curve would be horizontal so long as the heat was being liberated. If now a block be made of manganese steel, it is found that the curve is very nearly a straight line, showing that there is no liberation of latent heat at any temperature. If it is made of nickel steel with 25 per cent. of nickel, in its non-magnetic state, the result is the same—no sign of liberation of heat. If now the block be made of hard steel, the temperature diminishes at first; then the curve (Fig. 13) which represents the temperature bends round: the temperature actually rises many degrees whilst the body is losing heat. The liberation of heat being completed, the curve finally descends as a straight line. From inspection of this curve it is apparent why hard steel exhibits a sudden accession of brightness as it yields up its heat. In the case of soft iron the temperature does not actually rise as the body loses heat, but the curve remains horizontal, or nearly horizontal, for a considerable time. This, again, shows why, although a considerable amount of heat is liberated at a temperature corresponding to the horizontal part of the curve, no marked recalescence can be obtained. From curves such as these it is easy to calculate the amount of heat which becomes latent. As the iron passes the critical point it is found to be about 200 times as much heat as is required to raise the temperature of the iron 1 degree Centigrade. From this we get a very good idea of the importance of the phenomenon. When ice is melted and becomes water, the heat absorbed is 80 times the heat required to raise the temperature of the water 1 degree Centigrade, and 160 times the heat required to raise the temperature of the ice by the same amount. The temperature of recalescence has been abundantly identified with the critical temperature of

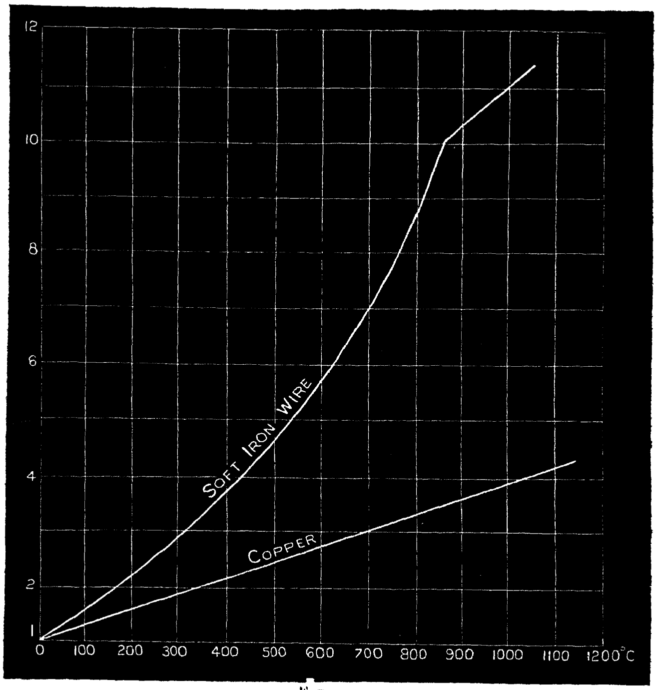


Fig. 12.

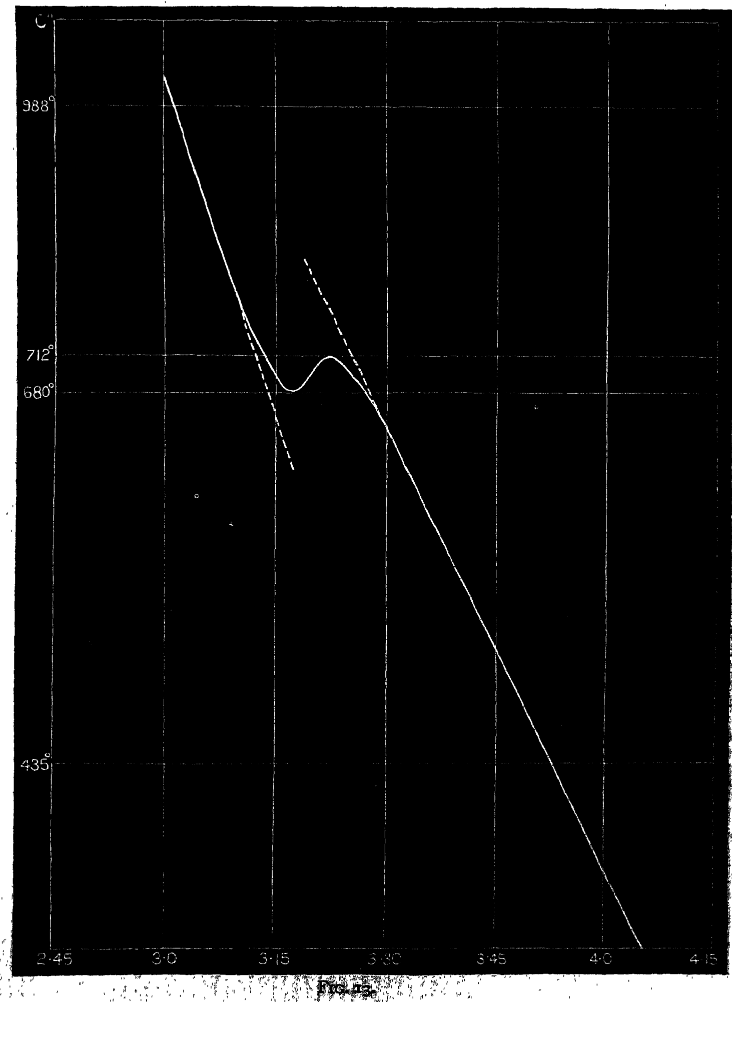


Fig. 13.

magnetism.[1] I am not aware that anything corresponding with recalescence has been observed in the case of nickel. Experiments have been tried, and gave a negative result, but the sample was impure; and the result may, I think, be distrusted as an indication of what it would be in the case of pure nickel. The most probable explanation in the case of iron, at all events, appears to be that when iron passes from the magnetic to the non-magnetic state it experiences a change of state of comparable importance with the change from the solid to the liquid state, and that a large quantity of heat is absorbed in the change. There is, then, no need to suppose chemical change; the great physical fact accompanying the absorption of heat is the disappearance of the capacity for magnetization.

What explanations have been offered of the phenomena of magnetism? That the explanation must be molecular was early apparent. Poisson's hypothesis was that each molecule of a magnet contained two magnetic fluids, which were separated from each other under the influence of magnetic force. His theory explained the fact of magnetism induced by proximity to magnets, but beyond this it could not go. It gave no hint that there was a limit to the magnetization of iron—a point of saturation; none of hysteresis; no hint of any connection between the magnetism of iron and any other property of the substance; no hint why magnetism disappears at a high temperature. It does, however, give more than a hint that the permeability of iron could not exceed a limit much less than its actual value, and that it should be constant for the material, and independent of the force applied. Poisson gave his theory a beautiful mathematical development, still useful in magnetism and in electrostatics.

Weber's theory is a very distinct advance on Poisson's. He supposed that each molecule of iron was a magnet with axes arranged at random in the body; that under the influence of magnetizing force the axes of the little magnets were directed to parallelism in a greater degree as the force was greater. Weber's theory thoroughly explains the limiting value of magnetization, since nothing more can be done than to direct all the molecular axes in the same direction. As modified by Maxwell, or with some similar modification, it gives an account of hysteresis, and of the general form of the ascending curve of magnetization. It is also very convenient for stating some of the facts. For example, what we know regarding the effect of temperature may be expressed by saying that the magnetic moment of the molecule diminishes as the temperature rises, hence that the limiting moment of a magnet will also diminish; but that the facility with which the molecules follow the magnetizing force is also increased, hence the great increase of μ for small forces, and its almost instantaneous extinction as the temperature rises. Again, in terms of Weber's theory, we can state that rise of temperature enough to render iron non-magnetic will not clear it of residual magnetism. The axes of the molecules are brought to parallelism by the force which is impressed before and during the time that the magnetic property is disappearing; they remain parallel when the force ceases, though, being now non-magnetic, their effect is nil. When, the temperature falling, they become again magnetic, the effect of the direction of their axes is apparent. But Weber's theory does not touch the root of the matter by connecting the magnetic property with any other property of iron, nor does it give any hint as to why the moment of the molecule disappears so rapidly at a certain temperature.

Ampère's theory may be said to be a development of Weber's: it purports to state in what the magnetism of the molecule consists. Associated with each molecule is a closed electric current in a circuit of no resistance; each such molecule, with its current, constitutes Weber's magnetic molecule, and all that it can do they can do. But the great merit of the theory—and a very great one it is—is that it brings magnetism in as a branch of electricity; it explains why a current makes a magnetizable body magnetic. It also gives, as extended by Weber, an explanation of diamagnetism. It, however, gives no hint of connecting the magnetic proporties of iron with any other property. Another difficulty is this: When iron ceases to be magnetizable, we must assume that the molecular currents cease. These currents represent energy. We should therefore expect that, when iron ceased to be magnetic by rise of temperature, heat would be liberated; the reverse is the fact.

So far as I know, nothing that has ever been proposed even attempts to explain the fundamental anomaly. Why do iron, nickel, and cobalt possess a property which we have found nowhere else in nature? It may be that at lower temperatures other metals would be magnetic, but of this we have at present no indication. It may be that, as has been found to be the case with the permanent gases, we only require a greater degree of cold to extend the rule to cover the exception. For the present, the magnetic properties of iron, nickel, and cobalt stand as exceptional as a breach of that continuity which we are in the habit of regarding as a well-proved law of Nature.

[1] I have only recently become acquainted with the admirable work of M. Osmond on recalescence. He has examined a great variety of samples of steel, and determined the temperatures at which they give off an exceptional amount of heat. Some of his results are apparent on my own curves, though I had assumed them to be mere errors of observation. For example, referring to my Royal Society paper, there is, in Fig. 38, a hint of a second small anomalous point a little below the larger one. And, comparing Figs. 38 and 38A, we see that the higher the heating, the lower is the point of recalescence; both features are brought out by M. Osmond. The double recalescence observed by M. Osmond in steel with a moderate quantity of carbon I would explain provisionally by supposing this steel to be a mixture of two kinds which have different critical temperatures. Although M. Osmond's method is admirable for determining the temperature of recalescence, and whether it is a single point or multiple, it is not adapted to determine the quantity of heat liberated, as the small sample used is inclosed in a tube of considerable mass, which cools down at the same time as the sample experimented upon.

NOTES ON A RECENT VOLCANIC ISLAND IN THE PACIFIC.

In 1867, H.M.S. *Falcon* reported a shoal in a position in about 20° 20′ S., and 175° 20′ W., or 30 miles west of Namuka Island of the Friendly or Tonga Group.

In 1877 smoke was reported by H.M.S. *Sappho* to be rising from the sea at this spot.

In 1885 a volcanic island rose from the sea during a submarine eruption on October 14, which was first reported by the *Janet Nichol*, a passing steamer, to be 2 miles long and about 250 feet high.

The U.S.S. *Mohican* passed it in 1886, and from calculation founded on observations in passing, gave its length as $1\frac{4}{10}$ miles, height 165 feet. The crater was on the eastern end, and dense columns of smoke were rising from it.

In 1887 the French man-of-war *Decres* reported its height to be 290 feet.

In the same year an English yacht, the *Sybil*, passed it, and a sketch was made by the owner, H. Tufnell, Esq., which is here produced.

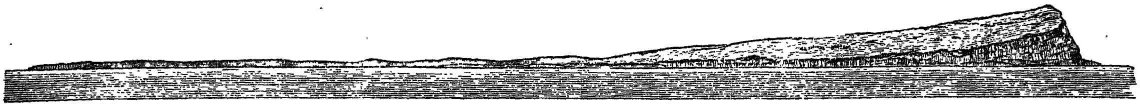
The island has now been thoroughly examined and mapped, and the surrounding sea sounded by H.M. surveying-ship *Egeria*, Commander Oldham.

It is now $1\frac{1}{10}$ mile long, and $\frac{8}{10}$ of a mile wide, of the shape given in the accompanying plan. The southern portion is high, and faced by cliffs on the south, the summit of which is 153 feet above the sea. A long flat stretches to the north from the foot of the hill.

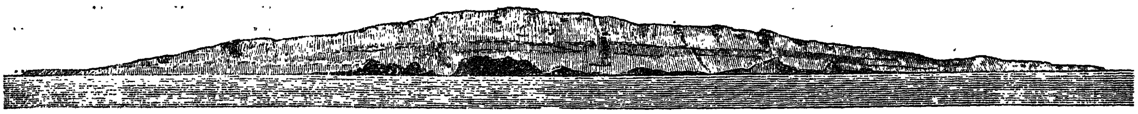
The island is apparently entirely formed of ashes and cinders, with a few blocks and volcanic bombs here and there, especially on the verge of the hill.

Under the action of the waves, raised by the almost constant south-east winds, this loose material is being rapidly removed; continual landslips take place, and Commander Oldham is of opinion that the original

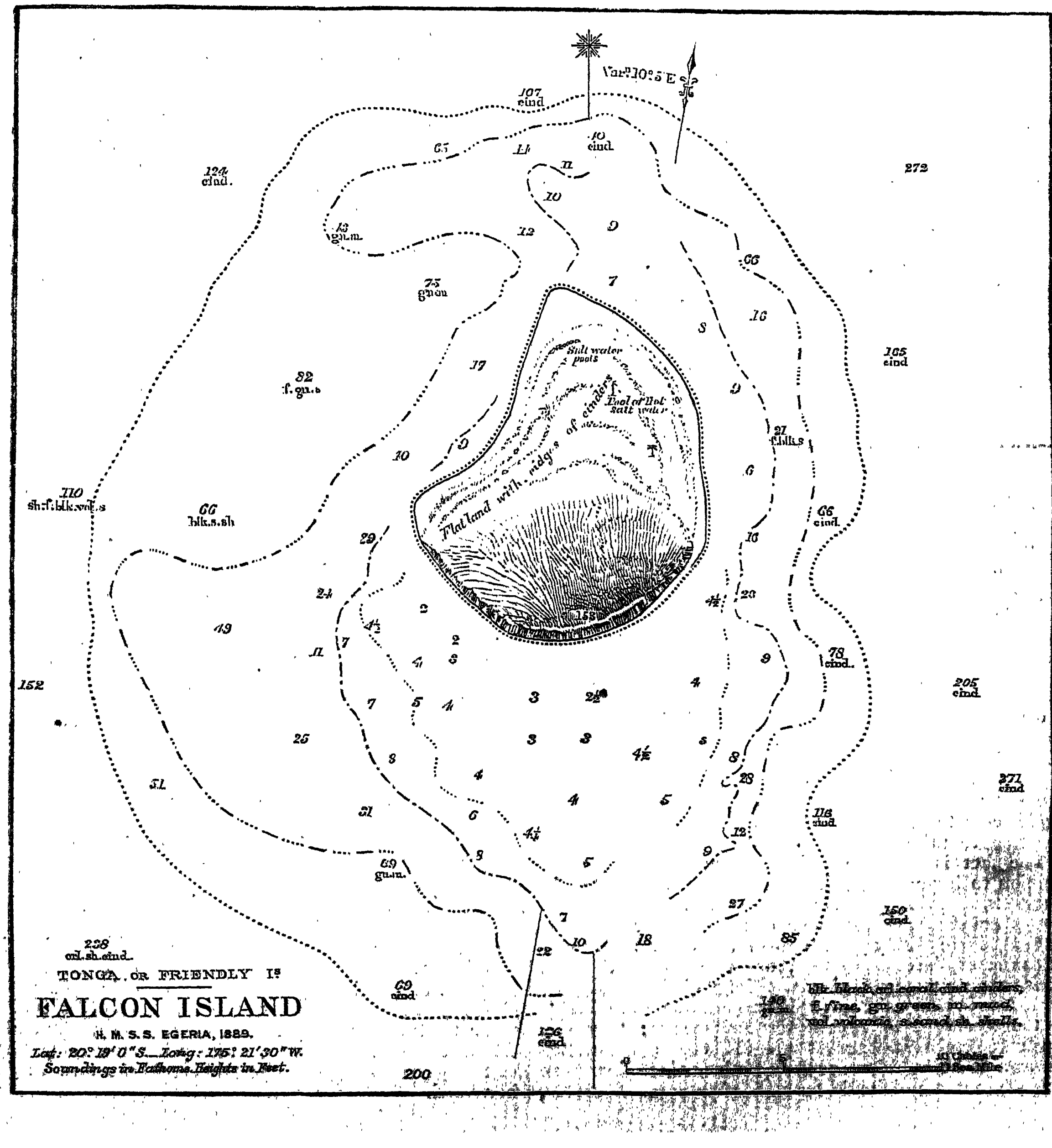
By H. Tufnell, Esq., 1887, *bearing S.E. about* 2 *miles.*



By "Egeria," 1889, *bearing E.* 1½ *mile.*



By "Egeria," 1889, *bearing N.N.W.* ½ *W.* 1 *mile.*



summit was some 200 or 300 yards southward of the present highest cliff, and that the shallow bank stretching to the south represents the original extension of the island.

As far as can be judged from Mr. Tufnell's sketch from the north-west and that of the *Egeria* from the south-south-east, considerable changes have taken place in two years, the different summits shown in the former having disappeared as the sea has eaten away the cliffs.

The flat to the north seems to be partly due to redistribution under the lee of the island of the material removed from the southern face. It is crossed by curved ridges from 3 to 12 feet high, which Commander Oldham considers to have been formed as high beaches during spring tides and strong winds, the flat ground between them, almost at the level of the water, being deposited under normal conditions of weather.

The island is thus gaining on one side, while losing on the other, but when the high part has gone, this partial recovery will probably cease.

A little steam issuing from cracks in the southern cliffs was the sole sign of activity, but a pool of water at a temperature of from 91° to 113° F., water which rose in a hole dug in the flat of a temperature of 128° F., and a temperature of 100° F. in a hole dug half-way up the slope, also show that the island still retains heat near the surface. The water is sea-water that has filtered through the loose ashes, and it rose and fell with the tide.

It appears by the condition of the flat that the island has neither risen nor subsided during the past two or three years.

It will be interesting to watch the ultimate fate of this last addition to the Pacific isles, but it seems probable that its existence as an island will be short unless a hard core is yet revealed.

The soundings between Falcon Island and Namuka show that they are separated by a valley 6000 feet deep.

Metis Island, 73 miles north-north-east of Falcon Island, is another volcanic cone that appeared a few years before the latter, but has not yet been examined.

W. J. L. WHARTON.

WEATHER FORECASTING.

POPULAR interest in weather prediction shows no sign of abating. The January number of the *Kew Bulletin* is devoted to an account of Herr Nowack's so-called "weather plant," and its failure as an indicator either of coming weather or of earthquakes. Very recently a lively correspondence has been carried on in the daily press on the merits or demerits of the forecasts issued by the Meteorological Office. Accordingly, some remarks on the subject in the columns of NATURE may not be out of place.

One critic says that the forecasts are little better than haphazard guesses, and that the money devoted to them would be better spent on an additional lifeboat or two on the coast. Another says the forecasts are not worth the paper they are printed on, and wishes that the Office published in the newspapers fuller accounts of the weather reported from the coasts.

The fact is that the Office is compelled by public opinion to issue forecasts. The public *will* have its forecasts, as in 1867 it would have its storm-warnings, notwithstanding the reluctance of meteorologists to issue either the one twenty years ago or the other at present. It can hardly be doubted that, for these islands at least, conscientious meteorologists would be disposed to agree with Arago, who said in 1846, and printed it in italics in the *Annuaire du Bureau des Longitudes*: "Jamais, quels que puissent être les progrès des sciences, les savants de bonne foi et soucieux de leur réputation ne se hasarderont à prédire le temps." We are, of course, speaking of forecasts based on telegraphic reports, and emanating from a central office. In every country, without exception, where forecasts for distant counties or provinces are issued from headquarters, the complaints from outlying stations, of occasional failure, are frequent enough.

The fact is that at individual stations the percentage of success may be highly satisfactory, as at Mr. C. E. Peek's observatory at Rousdon, Lyme Regis. The results for this point appeared in the *Times* of January 14, and are as follows:—

	1	2	3	4	5	6	7
1884	58·7	69·0	20·0	11·0	73·4	16·9	9·7
1885	70·0	80·0	12·0	8·0	80·0	12·0	8·0
1886	73·0	80·0	11·0	9·0	85·0	8·0	7·0
1887	75·0	83·0	9·0	8·0	82·0	11·0	7·0
1888	81·0	89·0	5·0	6·0	89·0	7·0	4·0

In this, Col. 1 is percentage of reliable wind and weather.
Col. 2 ,, ,, wind only.
Col. 3 ,, wind doubtful.
Col. 4 ,, wind unreliable.
Col. 5 ,, reliable weather.
Col. 6 ,, weather doubtful.
Col. 7 ,, ,, unreliable.

On the other hand, at other points the forecasts may be frequently unsuccessful.

In one important particular not only our own Office, but all other Offices in Europe, signally fail, and that is the quantitative prediction of rain. No one is able, apparently, to predict whether the amount of rainfall on the morrow will be a tenth of an inch or a couple of inches. No sudden floods have ever yet been foretold. By this we are not speaking of predicting the approach of floods to the lower valleys from rain which has already fallen on the upper reaches of a river, for that is not meteorological prediction at all.

With the necessarily incomplete character of the information reaching head-quarters, the wonder is that the Office can attain such success as it does. The main deficiency in the information is in its quantity, and this seems to lie at the door of the Postal Telegraph Office, which insists on being paid for its telegrams. If meteorological messages were transmitted gratis, we might expect to hear at frequent intervals from our outposts, instead of twice, or, at most, thrice in the twenty-four hours: in fact, from several stations we can only hear once, the cost of more telegrams being prohibitive. It is self-evident that such an amount of information is quite insufficient. The weather will not abstain from changing because the hour for a telegraphic report has not arrived.

The information contained in the telegrams is also deficient in quantity, for the reporters cannot, within the prescribed form of their messages, communicate all the impressions which the ever-varying appearance of the sky may have conveyed to their minds. A skilled cloud observer, who has leisure to practise his powers, is often able to form a very correct idea of what is coming for the region bounded by his own horizon, but he is quite unable to give the benefit of his observations and experience to a friend in another county by telegraphing the information.

The greatest want which the Office finds in its observers is skill in cloud observation, and it appears to be the case that a cloud observer *nascitur non fit*, and that it is next to impossible to teach the art to a new hand, at least by correspondence.

Instrumental records of the phenomena taking place in the higher strata of the atmosphere are of course unattainable, and it is only by carefully watching the upper clouds that we can gain any notion of changes taking place up there, but, by means of such watching, Mr. Clement Ley is able to predict with nearly perfect certainty the weather for the Midlands—his own neighbourhood.

It must always be remembered that the forecasts are drawn for districts, not for individual stations; and disregarding the amount of correctness claimed by the Office by its own checking of its work, they attain a very creditable amount of success when tested by independent observers. This happens even in the summer-time, the very season at which a recent critic said that the forecasts for one month, if shuffled about, and drawn at random from a bag, would suit just as well for the next! This is proved by the results of the hay harvest forecasts, which are deduced from the reports of the recipients, practical agriculturists.

The following is the table for the season of 1888, the latest for which the figures are available:—

Districts.	Names of stations.	Percentages.			
		Complete success.	Partial success.	Partial failure.	Total failure.
Scotland, N.	Golspie and Munlochy	48	34	17	1
„ E.	North Berwick, Glamis, Aberfeldy, and Rothiemay	43	41	11	5
England, N.E. ...	Chatton and Ulceby	50	27	17	6
„ E.	Thorpe and Rothamsted	48	39	10	3
Midland Counties..	Cirencester and East Retford ...	53	32	9	6
England, S.	Horsham, Maidstone, and Downton	52	40	6	2
Scotland, W.	Dumbarton, Islay, and Stranraer	45	41	8	6
England, N.W. ...	Leyburn and Prescot	57	24	11	8
„ S.W. ...	Bridgend (Glamorgan), Clifton, Glastonbury and Spring Park (Gloucestershire)	46	36	13	5
Ireland, N.	Moynalty and Hollymount	43	38	14	5
„ S.	Moneygall, Kilkenny, Ardfert Abbey	53	31	10	6

Every year the Office hears of farmers expressing their interest in these announcements, and sending daily to the places where they are exhibited, to learn what they contain.

To give an idea of the difficulty of obtaining accurate opinions from outsiders as to the value of storm-warnings, which are a class of forecasts, it may be interesting to give some specimens of reports.

Inquiries were made in 1882, from all the stations where signals are hoisted, as to their correctness and general utility. From Tynemouth the answer was that "these signals have been, and will be, an inestimable boon to our seafaring population." From South Shields, just opposite Tynemouth, the reply to a recent official inquiry was that "the warnings were not a ha'porth of use, and that no one minded them." Each answer merely represented the private opinion of the person who uttered it.

The reader can see that there is some difficulty in picking out the actual truth from such a heap of incongruous statements as the foregoing are certain to furnish.

R. H. S.

THE LABORATORIES OF BEDFORD COLLEGE.

BEDFORD COLLEGE, in York Place, Baker Street, which was one of the earliest institutions devoted to the higher education of women, is taking a leading part in providing facilities for their instruction in science. Founded long before Oxford and Cambridge condescended to the "weaker sex" (which has since proved strong enough to attain to the highest place in the Classical Tripos), it is the result of the work of enthusiasts who would not admit the possibility of defeat. It has had to struggle not only against the inevitable difficulties due to its early foundation, but against the apathy of London. Provincial towns feel that their honour is involved in the success of their institutions. The Colleges for women at Oxford and Cambridge share in the picturesque surroundings of those old homes of learning. They attract attention and interest by their situation amid scenes and traditions of which the whole English-speaking race is proud. Bedford College has had no such advantages. London institutions are regarded as either Imperial or parochial—as too large or too small to interest its citizens as such. Bedford Square compares unfavourably with the "backs," and it is impossible to regard York Place with that gush of emotion which "the High" sets free. Thus it is that, although Bedford College has been at work since 1849, and though one in every four of the whole number of women who have gained degrees of the University of London has been a student in its classes, the work of the College does not yet receive the meed of public appreciation which it has fairly earned. Bedford College is for women what University and King's Colleges are for men. It provides, within easy reach of all Londoners, an education which is tested by the severe standard of the University of London, and bears the hall-mark of success. One-third of its students are aiming at degrees, and their presence in the class-rooms, their work in the examination-hall, guarantees the quality of the teaching they receive to class-mates who do not intend to face the same ordeal. Science has for long been taught in Bedford College, but there has been a pressing need for better laboratories and class-rooms. These the Council has now provided. A new wing has been built, dedicated to the memory of the late Mr. William Shaen, who worked long and devotedly for the College. About £2000 is required to complete and fit up this building free of all debt, and Mr. Henry Tate, who had already given £1000 to the fund, has promised to supplement it by a like amount if the Council on its part can raise the other moiety of the deficit. It is too probable that this sum will only be obtained by an exhausting effort, but surely it is not too much to hope that the public may at last appreciate the importance of promoting the higher education of women in London. In a northern manufacturing town the money would be forthcoming in a week.

As regards the laboratories, it may be sufficient to say that Dr. W. Russell, F.R.S., is the Chairman of the Council, and that they have been built under his general supervision. They appear to be in all respects suited to the purposes for which they are intended. The physical laboratory and lecture-room are on the ground floor. The former has a concrete floor, and is well lighted, partly by windows, partly by a skylight. It looks out upon East Street, and is therefore removed as far as possible from the effects of the heavy traffic in Baker Street. The chemical laboratory is at the top of the house, and opens into a class-room which is fitted with all the usual conveniences for experimental illustration.

It is surely a hopeful sign that a College for the higher education of women should now be regarded as incomplete unless it controls physical and chemical laboratories specially designed and fitted for the delivery of lectures and the performance of experiments. These Bedford College now possesses. We can only hope that it may soon possess them free of debt. The Editor of NATURE will be happy to receive and forward to the College authorities any subscriptions which may be sent to him for that purpose.

STEPHEN JOSEPH PERRY, F.R.S.

ON the evening of January 4 a telegram from Demerara announced that there had been a successful observation of the eclipse of December 22, and that Father Perry had succumbed to dysentery.

Stephen Joseph Perry was born in London on August 26, 1833, and received his early education at Gifford Hall School. Having decided to enter the priesthood, he went

to the Catholic Colleges at Douai and Rome. While at Rome, he resolved to enter the Order of Jesuits; and, returning to England, he joined the English province of the Order on November 12, 1853. After two years' noviciate, he went to France for one year. He then returned to Stonyhurst for a course in philosophy. His inclination to mathematics was soon apparent, and his superiors in the Order decided to train him specially for this line of work. In 1858 he occupied the 6th place on the Mathematical Honours list of the London University. After attending lectures by De Morgan, he went to Paris for a year to finish his mathematical studies. On returning to Stonyhurst, he was appointed Professor of Mathematics and Director of the Observatory, succeeding Father Weld, who had for many years occupied the position. During the College year 1862-63, Father Perry taught one of the classes at Stonyhurst. In September 1863 he went to study divinity at St. Bueno's College, North Wales, and in 1866 he was ordained priest. Two years later he returned to Stonyhurst to resume his professorship and the charge of the Observatory. From this time he never left the College save to take part in some scientific expedition.

The work at Stonyhurst Observatory had been chiefly meteorological and magnetic before Father Perry's assumption of the directorship. In 1866 it was selected as one of the first-class meteorological stations. In 1867 the astronomical department of the Observatory was placed in a much more satisfactory position by the acquisition of an equatorial which originally belonged to Mr. Peters, and a small instrument destined for spectroscopic work. The first of these instruments was an 8-inch by Troughton and Simms, the second a 2¾-inch. The first spectroscope was procured in 1870 from Mr. Browning, and was used for preliminary work on star spectra, pending the construction of a larger instrument ordered from Troughton and Simms. In 1874 a large direct-vision spectroscope was ordered from Browning for use in observing the transit of Venus. Two years later a Maclean spectroscope was added, and in 1879 another by Browning containing 6 prisms of 60°; and more recently a Christie half-prism by Hilger.

With these instruments Father Perry has carried out systematic work of the highest class, his aim being to make Stonyhurst as efficient an observatory for solar physics as the means at his disposal would admit. His first communication to the Royal Astronomical Society indicates the policy he pursued—to undertake no work which was a mere duplication of that done at other places. His solar work during the last ten years formed the subject of a lecture at the Royal Institution on May 24. It may be divided into two classes—drawings and spectroscopic observations. For the drawings an image of the sun 10½ inches in diameter was projected on a sheet of drawing-paper affixed to a sketch-board carried by the telescope, and all markings on the sun traced. The drawing finished, the chromosphere and prominences were examined with the spectroscope. About 250 drawings were made every year from 1880. The results of the observations were published annually in a neat little volume, and also in various publications.

In addition to this work, regular observations of Jupiter's satellites, comets, &c., were made, as also spectroscopic observations of comets, stars, &c.

Father Perry's labours were not confined to the Observatory alone, and in fact the extraneous work which he undertook gave the world the best opportunities for studying his high character, and impressed astronomers with a sense of his great devotion to their science. The first occasion on which he left the Observatory for scientific work was in the autumn of 1868, when, accompanied by Father Sidgreaves, he made a magnetic survey of the west of France. In the following year the vacation was spent in a like work for the east of that country. In 1871, assisted by Mr. Carlisle, he made a similar survey of Belgium.

In 1870, Father Perry took part, for the first time, in an eclipse expedition, being stationed near Cadiz, whither he had taken the two spectroscopes acquired by the Observatory in 1870, and two telescopes—a Cassegrain of 9⅓ inches and a 4-inch achromatic. In 1874 he volunteered for the Transit of Venus expeditions, and was selected by Sir George Airy as chief of the Kerguelen party. Much tact and energy were required for the success of his party, who encountered several obstacles before arriving at the "Island of Desolation," as he termed Kerguelen. The spirit in which these obstacles were met is shown by his words—"We were determined that no consideration should make us flinch where the astronomical interests of the expedition were at stake." That this was no vain boast is proved by the evidence of those who were his colleagues in any excursions by water. His sufferings from sea-sickness were so fearful that everyone wondered that he cared to venture on even the most promising trip; and that he should have undertaken the terrible voyage to Kerguelen speaks volumes for his enthusiasm for science. "Four days and nights the mighty waves had been washing over the *Volage*." His patience in suffering on this and other occasions helped to win for him the esteem of the officers with whom he came in contact. Not one word of his discomfort is to be found in any of the journals kept by him. In addition to the work of the expedition, he took magnetic observations at the Cape, Kerguelen, Bombay, Aden, Port Said, Malta, Palermo, Rome, Naples, Florence, and Moncalieri, and lectured on the Transit of Venus at the Cape and Bombay, and, on his return, at the Royal Institution.

In 1882 he went to Madagascar for the Transit of Venus. For the eclipse of August 29, 1886, he went to Carriacou, for that of August 19, 1887, to Russia; and last November he sailed for Salut Isles on his final expedition. It is worthy of remark that the Archbishop of Demerara, who had been a pupil of his, went to Barbadoes in 1886 to see his old master; and on the present occasion the body of the master was taken to Demerara.

When at Stonyhurst, Father Perry, in addition to his Observatory work, carried out to the fullest extent his duties as a professor. He was very popular as a lecturer; and at Liverpool, Wigan, and neighbouring towns, he often delighted audiences, some of which numbered more than 3000 people. Father Perry but rarely occupied the pulpit of recent years, but he was much admired as a preacher. His sermons were marked by the earnestness which formed so distinguished a feature of his character.

To those who came in contact with him in connection with his scientific work, he endeared himself by his genial and retiring manner, retiring on all occasions save when some sacrifice was demanded for the science he loved so well, and for which he laid down his life on December 27.

In 1874, Father Perry was elected a Fellow of the Royal Society, and very shortly before his last voyage he was placed on its Council. He was a Fellow and Member of Council of the Royal Astronomical Society, and a Fellow of the Royal Meteorological Society, the Physical Society of London, and the Liverpool Astronomical Society. Of the last-named Society he was President at the time of his death. In 1886 he received the honorary degree of D.Sc. from the Royal University of Ireland, and at various dates he was elected by the Accademia dei Nuovo Lincei, the Société Scientifique de Bruxelles, and the Société Géographique d'Anvers. For several years preceding his death, he served on the Committee of Solar Physics, appointed by the Lords of the Committee of Council on Education, and also on the Committee for Comparing and Reducing Magnetic Observations, appointed by the British Association for the Advancement of Science. In April 1887 he took part in the International Astrophotography Congress held at Paris.

MR. DANIEL ADAMSON.

As a mechanical engineer and a metallurgist, Mr. Daniel Adamson must always maintain a foremost place, for he was in the van in the industrial progress of the century. He was born at Shildon, in the county of Durham, in 1818, and apprenticed to Mr. T. Hackworth, locomotive superintendent of the Stockton and Darlington Railway, with whom he remained from 1835 to 1841. He then held various stations in the same railway until 1850, and in 1851 he began business on his own account as an iron-founder, engineer, and boiler-maker.

From this time forward until quite recently Mr. Adamson has brought out many highly successful inventions in connection with the manufacture of boilers and the application of steam. The first of these was a flange seam for high-pressure boilers, patented by him in 1852, and well known as Adamson's flange seam. In 1856, Mr., now Sir Henry, Bessemer, read a paper before the British Association at Cheltenham describing his steel process, and one of the first to apply it was Mr. Adamson. Having satisfied himself by experimental trials of the quality of steel, he determined to use it for the manufacture of boilers; and Sir Henry Bessemer, when on May 9, 1888, he presented the Bessemer Medal to Mr. Adamson on behalf of the Council of the Iron and Steel Institute, referred with satisfaction to this circumstance, as being the turning-point in his own career, and as having given a start to the use of steel for general engineering purposes. Later on, when open-hearth steel was introduced by the late Sir William Siemens, Mr. Adamson made trial of it for boiler use, and was for years an upholder of the merits of steel. He wrote a comprehensive paper "On the Mechanical and other Properties of Iron and Mild Steel," which was brought before the Paris meeting of the Iron and Steel Institute in 1878, when it gave rise to a most interesting discussion. This paper is looked upon as a standard one on the subject of steel.

Mr. Adamson's inventions appear to have been all intimately connected with his business. In 1858 he applied hydraulic power for the riveting of steel structures, and in 1862 he brought out an invention for building steam boilers, the rivet holes being drilled through the plates when these were in position. He was entirely opposed to the punching of steel plates; he described it as a barbarous mode of treatment, as it tore the fibre of the material; and he would never allow it to be used in his own works. The important feature in all Mr. Adamson's work was its thoroughness; all the material used was subjected to chemical and mechanical tests, so that he obtained a reputation throughout the world for the soundness of everything he turned out.

Mr. Adamson was one of the first to show the superiority of compound engines. This class of engine had already been introduced by Mr. John Elder, of Glasgow, but to Mr. Adamson is greatly due the credit of the employment of triple and quadruple expansion engines. In 1874 he read a paper at Manchester, in which he maintained that pressures of 150 pounds on the square inch could be as safely applied as pressures of 50 pounds by a careful extension of the compound system. As far back as 1861 he patented and brought out a triple-expansion engine, and in 1873 a quadruple engine. In the paper to which we have just referred Mr. Adamson gave expression to the opinion that the consumption of coal per horse-power per hour should not exceed from 1 to $1\frac{1}{2}$ pounds of coal, whilst at that time $2\frac{1}{2}$ pounds per horse-power per hour was considered a very good result.

Besides these inventions, Mr. Adamson took out patents in connection with the manufacture of steel by the Bessemer process, with machinery for compressing steel, and for testing machines, as also improvements in guns and armour.

No account of his work would be complete without a reference to his connection with the Manchester Ship Canal. He was of an enthusiastic temperament, and this was made specially evident in connection with this great undertaking. A Manchester man, and thoroughly convinced of the benefit which would accrue to the surrounding manufacturing towns, Mr. Adamson set to work to effect what others had proposed. It is more than 65 years ago since it was proposed that Manchester should be connected with the sea by a ship canal, but it was Mr. Adamson's invitation to various persons to meet at his house on June 27, 1882, that really started the project. The proceedings then initiated resulted in the incorporation of the Manchester Ship Canal Company in 1885. Mr. Adamson's work in connection with international progress, and his labours to make Manchester an ocean steam port, will not readily be forgotten.

In September and October last he was engaged on an examination of the iron mines of the island of Elba, and he embodied the results in a report to the Italian Government. About two months ago he caught a cold on his Flintshire estate of Wepre Hall. He returned to his home at Didsbury, and died there on Monday, the 13th inst.

Quite recently Mr. Adamson was elected President of the Iron and Steel Institute. He was a member of the Institution of Civil Engineers, of the Institution of Mechanical Engineers, and of the Iron and Steel Institute, and to the proceedings of these Societies he presented many papers containing the results of his inquiries as to the properties and treatment of metals, especially iron and steel.

NOTES.

At a meeting of a Committee appointed by the Council of the Royal Society to set on foot a memorial to the late James Prescott Joule, held on November 30 last, at Burlington House, it was unanimously resolved that a fund should be raised for a memorial of an international character commemorative of the life-work of Joule. This memorial will have for its object the encouragement of research in physical science. It is proposed also that a tablet or bust shall be erected to his memory in London, a Manchester Memorial Committee having already taken steps to ensure a suitable monument in his native city. Joule's discoveries were of such commanding importance that there can be no doubt as to the success of this movement. The Committee feel confident not only that men of science will gladly contribute towards a fund to do honour to Joule's memory, and to assist others to follow in his footsteps, but that those who devote themselves to the practical application of scientific principles will also be anxious to aid in the promotion of a fitting memorial of one whose work has exerted so great an influence on industry.

We regret to announce the death of Gustave-Adolphe Hirn, the eminent physicist. He died at Colmar on January 14, in his seventy-fifth year.

Mr. Rooney, who accompanied the late Father Perry on the solar eclipse expedition to the Salut Isles, has arrived in England, bringing with him the plates successfully exposed during the totality of the eclipse by Father Perry and himself. Mr. Rooney has put himself in communication with the Astronomer Royal, and the plates will be handed over to the Royal Astronomical Society to be developed.

The Forth Bridge was tested by the engineers on Tuesday as a preliminary to the passage of the first train over it on Friday. The following is the official report:—"Sir John Fowler and Mr.

Baker, engineers of the Forth Bridge, have to-day tested the two 1700-feet spans by placing on the centres two trains, each made up of 50 loaded coal waggons and three of the heaviest engines and tenders, the total load thus massed upon the spans being the enormous weight of 1800 tons, which is more than double what the bridge will ever be called upon in practice to sustain. The observed deflections were in exact accordance with the calculations of the engineers, and the bridge exhibited exceptional stiffness in all directions." Every part of the bridge will be in perfect order for the visit of the Prince of Wales on March 4.

At the meeting of the Convocation of London University, on Tuesday, there was some discussion as to the question of the reconstitution of the University. Dr. F. J. Wood, who presided, said he was not in a position to help Convocation very much. As they were well aware, the Senate had drawn up a scheme which was intended to follow on the lines of the recommendations of the Royal Commission. That scheme had been submitted to the consideration of University College and King's College, and up to now those Colleges had arrived at no decision upon it, but requested a conference. That conference was about to take place, and, of course, until it was held it was impossible for any of them to say what shape the scheme would ultimately assume. Mr. T. Tyler moved a resolution declaring that "The proposal of the University for London Commission that, under a new charter for this University, special powers and privileges should be conferred on certain institutions in or near London is incompatible with the fair and just treatment of the provincial Colleges, and that the acceptance of this proposal would be detrimental alike to the interests of the provincial Colleges and to those of the University itself." This motion was unanimously adopted.

On Friday, January 24, at 4.30 p.m., Mr. Holland Crompton will begin a course of ten lectures at the Central Institution, Exhibition Road, on the theory of electrolysis and the nature of chemical change in solution. In this course an historical account will be given of the recent development of the Clausius dissociation hypothesis by Arrhenius, Ostwald, and others; of van't Hoff's extension of Avogadro's theorem to dilute solutions; and of the Raoult methods of determining the molecular weights of dissolved substances. On Monday, January 27, at 4.30 p.m., Prof. Armstrong, F.R.S., will begin a special course of ten lectures on methods of analysis as applied to the determination of the structure of carbon compounds. The object of this course will be to explain and experimentally demonstrate the methods adopted in determining the structure of the more important and typical compounds, including alkaloids, carbohydrates, and oils and fats.

The annual meeting of the Association for the Improvement of Geometrical Teaching was held last Friday morning in one of the theatres of University College, London, under the presidency of Prof. Minchin. While observing with pleasure that the Universities of Oxford and Cambridge had embodied in the printed regulations for various examinations some requests of the Association with regard to elementary geometry, the Council in their report expressed regret that the Euclid papers set for responsions at Oxford still consist exclusively of "book work." The response of the University of Dublin to the Society's petition is that they are not prepared to decide on such important questions without much consideration. At the afternoon meeting papers were read by the Master of St. John's College, Cambridge, on a new treatment of the hyperbole; by Mr. G. Heppel, on the teaching of trigonometry; by Mr. E. M. Langley, on some geometrical theorems; by Prof. Minchin, on statics and geometry; and by Mr. R. Tucker, on isoscelian hexagrams.

Fears having been expressed as to a possible connection between influenza and cholera epidemics, Dr. Smolenski publishes, in the Russian *Official Messenger*, an elaborate report upon the subject. He points out that the suspicion is not new, and that in 1837 it was discussed by Gluge ("Die Influenza"), and refuted. In fact, influenza or *grippe* epidemics have been known in Europe since 1173—that is, for more than seven hundred years; whilst the first cholera epidemic appeared in Europe in 1823, but did not spread, that time, further than Astrakhan. Six years later it broke out in Orenburg; next year in Caucasia and Astrakhan again, whence it spread over Russia, and, in 1831, reached Western Europe. As a rule, influenza spreads very rapidly, and in 1782, at St. Petersburg, no fewer than 40,000 persons fell ill of it on the same day (January 14). In 1833 its progress was also very rapid, and within a few days it appeared at places so far apart as Moscow, Odessa, Alexandria, and Paris, while cholera epidemics are usually slow in their migrations from one place to another. Moreover, influenza is chiefly a winter epidemic, while cholera prefers the spring and the summer. Dr. Smolenski has further tabulated all influenza and cholera epidemics which have broken out in the course of our century in Europe, and he comes to the following results:—Influenza broke out in 1816, in Iceland; 1827, in Russia and Siberia; 1830-33, in Europe generally; 1836-37, in Europe; 1838, in Iceland; 1841-48 and 1850-51, in Europe; 1853, in the Faroe Islands; 1854-55 and 1857-58, in Europe; 1856, in Iceland and the Faroe Islands; 1862, Holland and Spain; 1863-64, France and Switzerland; 1866, France and Great Britain; 1867, France, Germany, and Belgium; 1868, Turkey; and 1874-75, Western Europe. As to the cholera epidemics during the same period they were: 1823, Astrakhan and Caucasia (from Persia); 1829, Orenburg (from Turkestan); 1830, Russia (from Persia); 1831-37, various parts of Europe; the next epidemic appeared in 1846 in Transcaucasia (coming from Persia); in 1847 it spread over Siberia and Russia, and in 1848 it was in Europe; in 1849-52 it was followed by feeble outbreaks all over Europe. The third cholera epidemic came from Persia again in 1852, and it resulted in a severe outbreak during the years 1853-55 in Europe, followed by feebler outbreaks till 1861. The fourth cholera epidemic came through the Mediterranean ports in 1865, and lasted in Europe till 1868, with feebler epidemics in 1869-74. The latest invasion of cholera was in 1884, when it came again through the Mediterranean ports. As to the cholera epidemic which now begins to die out in Persia and Mesopotamia, it certainly is a danger—the more so as, out of the five epidemics of cholera which have visited Europe, three have come from Persia.

Attention has lately been called to the fact that anchovies are found off Torquay and other south coast fishing centres. Prof. Ewart, of Edinburgh, has written to the *Times* that during the present winter they have made their appearance in the Moray Firth. At the end of December they were abundant off Troup Head, where considerable numbers were captured in the herring nets by the Buckie fishermen. Prof. Ewart thinks that further inquiries may perhaps show that the northward migration of the anchovies is in some way related to the mildness of the winter. He points out that it is most desirable to ascertain whether they have reached the Moray Firth with the warm Atlantic water that during western winds rushes through the Pentland Firth, or by travelling along the east coast through the cold Arctic water that wells up from the bottom in the vicinity of the Dogger Bank.

The programme of the Royal Horticultural Society for the present year includes a daffodil exhibition and conference, to be held at Chiswick on four days of April; the great show in the Temple Gardens in May; an exhibition of tea roses, by the National Rose Society, in June; in July an exhibition of and

conferences upon carnations, ferns, and selaginellas; and in September, at Chiswick, exhibitions of and conferences upon dahlias and grapes. The drill-hall meetings began with one on the subject of winter gardening, introduced by the Rev. W. Wilks; and, after the annual meeting in February, there are to be papers and discussions upon hippeastrums (amaryllis), saladings, spring flower gardening, spring flowering shrubs and trees, herbaceous pæonies, lilies, fruit-drying, hollyhocks, crinums, trees and shrubs for large towns, and Chinese primulas. The accommodation at the drill-hall is not adequate to the wants of the Society, and the Council is considering whether it would not be possible to erect a suitable building on the Thames Embankment.

THE International Horticultural Exhibition to be held in Berlin under Royal and Imperial auspices, from April 25 to May 5, will be characterized by two special features—an exhibition of horticultural architecture, and one of horticultural models, apparatus, &c. It is requested that all exhibits or announcements of such should be promptly sent to the General Secretary of the Society for the Promotion of Horticulture, Prof. Dr. L. Wittmack, Invalidenstrasse 42, Berlin N., from whom all further information may be obtained. The Exhibition will be held in the Royal Agricultural Exhibition building, on the Lehrt Railway. The general organizer of the scientific department is Prof. Dr. Pringsheim; and the following gentlemen have undertaken the management of special branches:—For the geography of plants, Prof. Dr. Ascherson; for physiology, Prof. Dr. Frank; for seeds, Herr P. Hennings; for morphology, anatomy, and the history of development, Prof. Dr. Kny; for fungi, Prof. Dr. Magnus; for soils, Prof. Dr. Orth; for history, literature, and miscellaneous, Dr. Schumann; for officinal and technical objects, Dr. Tschirch. The Minister for Agriculture, Dr. Freiherr v. Lucius-Balhausen, will be the Honorary President of the Exhibition. The city of Berlin has granted the sum of 15,000 marks towards its expenses; and a guarantee fund of 80,000 marks has been raised.

THE Calcutta Herbarium contains a rich collection of Malayan plants, and Dr. King, the superintendent of the Calcutta Royal Botanic Garden, proposes to publish from time to time a systematic account of as many of them as are indigenous to British provinces, or to provinces under British influence. In addition to the States on the mainland of the Malayan penninsula, these provinces include the islands of Singapore and Penang, and the Nicobar and Andamán groups. The classification which Dr. King intends to follow is that of the late Mr. Bentham and Sir Joseph Hooker. The current number of the Journal of the Asiatic Society of Bengal contains the first of this proposed series of papers.

THE January number of the *Kew Bulletin* contains an able and most interesting report, by Dr. Francis Oliver, on the so-called weather plant. This plant is *Abrus precatorius*, Linn., a well-known tropical weed. Mr. Joseph F. Nowack claims to have discovered that its leaves have "the peculiar property of indicating by their position various changes in nature about forty-eight hours before the said changes occur." Numerous observations with hundreds of such plants have convinced him that "any given position of the leaves corresponds always to a certain condition of the weather forty-eight hours afterwards." Some time ago he devised an apparatus for the purpose of putting his supposed discovery to practical use. It consists of a "transparent vessel containing the weather plant, closed on all sides, protected against injurious external influences, and adapted to be internally ventilated and maintained at a temperature of at least 18° Reaumur, these being the conditions under which, in temperate climates, Nowack's weather plant answers the purpose of a weather indicator." Last year Mr. Nowack was anxious that his apparatus should be scientifically tested at Kew, but it would not have been easy for any member of the staff of the Royal Gardens to find time for the necessary observations. The task was undertaken by Dr. Francis Oliver, who now presents the results of his investigation. The following is a summary of the conclusions at which he has arrived:—"I contend that all the movements exhibited by the leaves of *Abrus precatorius* depend on causes not so far to seek as those suggested by Mr. Nowack. The ordinary movements of the leaflets, of rising and falling, are called forth in the main by changes in the intensity of the light. In a humid atmosphere they are more sluggish than in a relatively dry one. In other words, when the conditions are favourable for transpiration the movements are most active. The position for snow and hail is connected intimately, in the cases that have come under my observation, with a spotting or biting (by insects) of the leaflets, and is not due to any other external factor. The position for fog and mist, and for electricity in the air, is probably due to the disturbance caused by varying light, the rhythmical movements of the leaflets being temporarily overthrown. The position indicating thunder and lightning I take to be pathological from its tendency to recur on the same leaves. Daily movements of the rachis constitute a periodic function in this as in many other plants with pinnate leaves. The regularity of these oscillations is considerably influenced by both light and temperature."

ON Tuesday an Archæological Congress began its proceedings at Moscow. The sitting was attended by delegates from German, Austrian, and French Archæological Societies. The section of the Russian Imperial Historical Museum in Moscow allotted to the Moscow Archæological Society was formally opened on January 8, by Prince von Dolgoroukoff, the Governor-General. The collection consists of a variety of antiquities from the Caucasus, stone and glass ornaments, beautiful enamel work from various parts of Russia, ancient holy images, and antique garments and china. A correspondent of the *Times*, who gives an account of the exhibits, calls attention especially to a number of ancient gold ornaments from the Caucasus (described as Merovingian), contributed by the Countess Ouvarova, the President of the Society. He also refers to certain Osetinian copper pins, 18 inches long, found near some human skulls, and supposed to have been used for dressing the hair. A helmet of Assyrian form has attracted much notice.

IN one of the lectures he is delivering at Aberdeen, under the Gifford Bequest, Dr. E. B. Tylor offered a most interesting suggestion the other day as to the meaning of a well-known but puzzling Assyrian sculptured group. This group consists of two four-winged figures, with bodies of men and heads of eagles, standing opposite a tree-like formation, which is easily recognized as a collection of date-palms, or a conventionalized representation of a palm-grove. Each of the two figures carries in the left hand a bucket or basket, in the right a body which each seems to be presenting to the palm-tree. What is this body? It is usually described as a fir-cone, but some have regarded it as a bunch of grapes, others as a pine-apple. Dr. Tylor suggests that it should be connected with the most obvious point of interest for which the date-palm has been famous among naturalists since antiquity—namely, its need of artificial fertilization in order to produce a crop of edible dates. This process in its simplest form consists in shaking the pollen from the inflorescence of the male date-palm over the inflorescence of the female. The practice is mentioned by Theophrastus and Pliny, and in modern times in such works as Shaw's "Travels in Barbary." Dr. Tylor exhibited a drawing of the male palm inflorescence, and said it was hardly necessary to point out the resemblance to the object in the hand of the winged figure of the Assyrian sculpture. As the cultivator of the palm-tree has to ascend the tree in order to perform the process of fertilization,

he of course takes with him a supply of fresh flowers in a basket. Dr. Tylor's theory, therefore, is that the objects carried by the winged genii of the Assyrians are the male inflorescence of the date-palm in one hand, the basket with a fresh supply of inflorescence in the other, and that the function the genii are depicted in the sculptures as discharging is that of fertilizing the palm-groves of the country—a function which must have been held to denote their great beneficence, since it showed them fulfilling the great duty of providing the Assyrians with bread.

THE current quarterly statement of the Palestine Exploration Fund contains a brief review of the work done in connection with the Fund during 1889. It is stated that excavations on property belonging to a French gentlemen on the eastern slope of Zion have revealed a number of rock-hewn chambers, which appear to have been used in ancient times partly as dwellings and partly as storehouses. In describing them Herr Schick remarks that nearly all the ground covered by the city of Jerusalem is found on examination to be honeycombed with these rock-hewn chambers. It is not improbable that the Jebusites were to some extent troglodytes. In the Apocryphal Acts of the Apostles mention is made of a cave at Cyprus "where the race of the Jebusites formerly dwelt."

SEVERAL violent shocks of earthquake occurred in Carinthia on January 14, at 9.30 p.m., their direction being from south-east to north-west. In the theatre at Klagenfurt, which was densely packed, the seismic disturbance caused a panic, which was heightened by a false alarm of fire. The audience, however, soon became reassured, and there was no accident to life or limb.

THE Pilot Chart of the North Atlantic Ocean for the month of January states that December was notable for the severe storms that prevailed along the Transatlantic routes. A number of the depressions followed each other in rapid succession; the most notable of these was one on the 16th, in about lat. 51° N., long. 37° W. Gales of hurricane force, with mountainous seas, accompanied this disturbance, as it moved to the north-eastward, to the serious embarrassment of west-bound steamers. Two storms occurred to the eastward of Bermuda during the first week of the month. The first of these disturbances was central on the 4th, in about lat. 36° N., long. 55° W. After 16 hours the wind hauled to south-east and moderated. The south-east wind experienced after the passage of the storm was probably due to the approach of the second cyclone, which was central on the 5th in about lat. 31° N., long. 63° W., and was accompanied by severe hailstorms and heavy seas. Very little fog was reported. A dense fog along the coast of the United States on the 19th, 20th, and 21st, extended some distance inland; navigation in New York harbour was practically suspended on the 20th. Ocean ice was reported in the neighbourhood of lat. 48° N., long. 47° W.

WE referred lately to a new kind of butter which is now being made in Germany from cocoanut milk. The Calcutta Correspondent of the *Times* says that the cocoanuts required for this industry are imported in large numbers from India, chiefly Bombay, and that the trade seems likely to attain still greater importance.

ACCORDING to the *Perseveranza* of Milan, quoted in the current number of the *Board of Trade Journal*, important sponge-banks have lately been discovered close to the island of Lampedusa, on the southern coast of Sicily. These deposits of sponges extend for over a surface of from 15 to 18 marine leagues, and are situated about an equal distance from the south-eastern extremity of the island. The smallest depth above these banks is 20 ells; the greatest depth is from 30 to 31 ells. At the lesser depths rock is met with, on which the sponge grows; at greater depths a sandy soil is found. All varieties of sponge are discovered here, including those which are in the greatest commercial request, and they are easy to obtain. Greek and Italian vessels have already proceeded to Lampedusa to take advantage of this discovery.

AT the meeting of the Linnean Society of New South Wales, on November 27, Mr. K. H. Bennett read a paper on the breeding of the glossy ibis (*Ibis falcinellus*, Linn.). The unprecedented rainfall of the year on the Lower Lachlan induced several species of birds to breed in the district, contrary to the author's experience of previous years. Among these was the glossy ibis, two nests of which with eggs of a beautiful greenish-blue colour somewhat resembling those of *Ardea novæ-hollandiæ*, but much brighter, were found in October and November. At the same meeting Mr. J. H. Maiden communicated preliminary notes, by Dr. T. L. Bancroft, on the pharmacology of some new poisonous plants. Mr. T. P. Lucas read a paper on Queensland Macro-Lepidoptera, with localities and descriptions of new species. Forty-one species belonging to various families were proposed as new, and new localities were given for about ninety-five other species.

THE new number of "The Year Book of Pharmacy" (J. and A. Churchill) has been issued. It comprises abstracts of papers relating to pharmacy, materia medica, and chemistry, contributed by British and foreign journals from July 1, 1888, to June 30, 1889. It presents also the Transactions of the British Pharmaceutical Conference at the twenty-sixth annual meeting, held at Newcastle-on-Tyne, September 1889.

MESSRS. E. AND F. N. SPON have issued a third edition of "A Guide for the Electric Testing of Telegraph Cables," by Colonel V. Hoskiær, of the Royal Danish Engineers. The first edition appeared in 1873. The Congress of Electricians in 1881 made some alterations necessary, and the author explains that he has added a few methods of testing, in the hope of making the book more useful.

THE Society for Promoting Christian Knowledge has issued, in the series entitled "Chief Ancient Philosophies," a third edition of the Rev. I. Gregory Smith's "Aristotelianism," in which an attempt is made to tabulate from the "Ethics" the opinions of Aristotle on questions relating to what has been called "the scientific basis of morality." In the same volume is printed a treatise, by the Rev. W. Grundy, Head Master of Malvern College, on the more important of Aristotle's other works.

SOME interesting properties and reactions of the chlorides of selenium are described by M. Chabrié in the current number of the *Bulletin de la Société Chimique de Paris*. Selenium tetrachloride, $SeCl_4$, was obtained by Berzelius by passing a stream of chlorine over selenium at the ordinary temperature, a quantity of the reddish-brown liquid subchloride, Se_2Cl_2, being first formed, and eventually converted to the pale yellow solid tetrachloride. The tetrachloride was subsequently volatilized by heating and obtained in small white opaque crystals. By heating the crystals obtained by this method in one end of a sealed tube to 190°–200° C., M. Chabrié has obtained a sublimate of much larger and better formed crystals, presenting brilliant faces. With these crystals determinations of the vapour density of the tetrachloride were attempted by Victor Meyer's method at 360° in an atmosphere of nitrogen. The resulting numbers show that two molecules of $SeCl_4$ dissociate at 360° into one molecule of Se_2Cl_2 and three molecules of chlorine. The subchloride, Se_2Cl_2, is a very much more stable body, and may be distilled unchanged at 360°. Determinations of the density of its vapour yield values closely approximating to 7·95, the theoretical density of a molecule of the formula Se_2Cl_2. Among the numerous reactions of these compounds which M. Chabrié has studied, the most interesting are those between selenium tetrachloride and

benzene. It is curious that when pure benzene is allowed to react upon pure $SeCl_4$, the latter body undergoes precisely the same decomposition as when heated to 360°, the liberated chlorine reacting with the benzene to form several chlor-benzenes and all the selenium remaining in the form of Se_2Cl_2. If, however, the benzene and selenium tetrachloride are brought together in presence of that most useful of intermediate reagents, aluminium chloride, quite a different series of changes occur. On treating the mixture with water, and separating and distilling the oil obtained, three distinct fractions may be collected. The first, which passes over at 131°–133°, consists of monochlor benzene, C_6H_5Cl. The second, distilling at 227°–228° under a pressure of only a few millimetres of mercury, consists of phenyl selenide, $(C_6H_5)_2Se$, corresponding to phenyl sulphide, $(C_6H_5)_2S$, and phenyl oxide, $(C_6H_5)_2O$. It is a yellow oil of sp. gr. 1·45 at 19°·6. The third fraction, boiling between 245° and 250° under the same reduced pressure, consists of another new compound of the composition $Se_2(C_6H_5)_3C_6H_4Cl$. This substance is a red oil of sp. gr. 1·55 at 19°·6. On allowing this red oil to stand it deposits yellow crystals of a compound of powerful odour, which may be obtained recrystallized from alcohol in long rhombic prisms. On analysis this substance turns out to be seleno-phenol, C_6H_5SeH, analagous to thiophenol and mercaptan, both of evil odour. Like all the hitherto investigated mercaptans, its alcoholic solution readily reacts with salts of mercury and silver. Analysis of the silver salt leads to the formula C_6H_5SeAg. The reactions by which phenyl selenide and seleno-phenol are respectively produced are believed by M. Chabrié to be as follows:—

$$2C_6H_6 + SeCl_4 = (C_6H_5)_2Se + 2HCl + Cl_2,$$
$$C_6H_6 + SeCl_4 = C_6H_5SeH + 2Cl_2.$$

THE additions to the Zoological Society's Gardens during the past week include a Black-headed Gull (*Larus ridibundus*), British, presented by Mr. E. Hart, F.Z.S.; a Chinese Jay Thrush (*Garrulax chinensis*) from China, presented by Sir Harry B. Lumsden, C.B., K.C.S.I., F.Z.S.; a King Parakeet (*Aprosmictus scapulatus* ♂) from Australia, presented by the Rev. A. J. P. Matthews, F.L.S.; a Peregrine Falcon (*Falco peregrinus*) from Scotland, presented by Mr. Geo. W. Landels; a Vulturine Eagle (*Aquila verreauxi*), a Jackal Buzzard (*Buteo jacal*), a White-necked Raven (*Corvultur albicollis*) from South Africa, presented by Mr. Marshall; a Pigmy Cormorant (*Phalacrocorax africanus*), a Moorhen (*Gallinula chloropus*), two Shining Weaver Birds (*Hypochera nitens*), four Black-bellied Weaver Birds (*Euplectes afer* 2 ♂ 2 ♀), two Abyssinian Weaver Birds (*Ploceus abyssinicus* ♂ ♂), four Red-beaked Weaver Birds (*Quelea sanguinirostris* 2 ♂ 2 ♀), four Cutthroat Finches (*Amadina fasciata* 2 ♂ 2 ♀), four Orange-cheeked Waxbills (*Estrelda melpoda*), a Paradise Whydah Bird (*Vidua paradisea* ♂) from West Africa, an Indian Silver-Bill (*Munia malabarica*) from India, two Cardinal Grosbeaks (*Cardinalis virginianus* ♂ ♂) an Indigo Bird (*Cyanospiza cyanea* ♂) from North America, purchased.

OUR ASTRONOMICAL COLUMN.

OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at Greenwich at 10 p.m. on January 23 = 6h 12m. 44s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G.C. 1225	—	—	5 36 5	+ 9 2
(2) LL. 12169	7	Yellowish-red.	6 15 58	−11 46
(3) θ Canis Maj. ...	5	Yellow.	6 48 38	−11 53
(4) γ Geminorum ...	2	White.	6 31 24	+16 30
(5) 74 Schj.	6	Reddish-yellow.	6 19 12	+14 46
(6) U Cancri	Var.	Reddish.	8 29 28	+19 16
(7) R Draconis	Var.	Yellowish-red.	16 32 22	+66 59

Remarks.

(1) The General Catalogue description of this nebula is as follows: "Planetary nebula; pretty bright, very small, very little extended." So far as I know, the spectrum has not yet been recorded, but if it is of the same nature as other planetary nebulæ, bright lines may be expected. The character of the chief line, near λ 500, if visible, should be particularly noted.

(2) Dunér classes this with stars of Group II., but states that the type of spectrum is a little uncertain. He notes, however, that the bands 2, 3, and 7 are visible, so there seems to be no reasonable doubt about the type. The probability is that it is either an early or late star of the group, in which case we should not expect to find all the bands fully developed. The star has been provisionally placed in species 2 of the subdivision of the group, but further observations are at once suggested to determine whether this is right or wrong. If right, the bright flutings of carbon should be fairly prominent, as it is probably due to the masking effects of these flutings that some of the dark bands are absent. The carbon flutings near 517 and 474, seen in the spectrum of a bunsen or spirit-lamp flame, should therefore be particularly looked for. It is possible, too, that in the earlier stars of the group the hydrogen lines may appear bright, as the swarms are only a little more condensed than those constituting stars with bright lines, so that the interspacial radiation may more than balance the absorption.

(3) According to the observations of Konkoly, this is a good example of stars of the solar type. The usual observations, as to whether the star belongs to Group III. or to Group V., are required.

(4) A star of Group IV. (Gothard). The main point to be noted in stars of this class is the relative intensities of the lines of hydrogen and those of iron, magnesium, and sodium, for the purpose of arranging them in a line of temperature. If possible, the criterion lines which indicate increasing or decreasing temperature should also be noted, as in the stars which have hitherto been classed as of the solar type.

(5) This is a star of Group VI., showing the usual carbon flutings and the subsidiary bands 4 and 5 (Dunér). In some stars of the group of smaller magnitude, a greater number of secondary bands have been noted, and it seems possible, therefore, that 74 Schj. may not have been observed under the most favourable conditions. Further confirmatory observations are therefore necessary before conclusions as to the specific differences between the different stars of the group can safely be drawn.

(6) The spectrum of this variable has not yet been recorded. The period is 305·7 days, and the range from 8·2–10·6 at maximum to < 13 at minimum (Gore). The maximum occurs on January 23.

(7) This variable star has a period of 244·5 days, and ranges from 7–8·7 at maximum to < 13 at minimum. The spectrum is of the Group II. type, and the range of variability is such that the appearance of bright lines at maximum may be expected, as in R Leonis, &c., observed by Mr. Espin. The maximum occurs on January 25. A. FOWLER.

THE CLUSTER G.C. 1420 AND THE NEBULA N.G.C. 2237.—Dr. Lewis Swift, in the *Sidereal Messenger* for January 1890, calls attention to a wonderful nebulous ring entirely surrounding this cluster. The ring was discovered by Prof. Barnard last year (*Astr. Nach.*, 2918), and its average outer diameter estimated as not less than 40′, so that in comparison the ring nebula in Lyra is a pygmy. Although Dr. Swift discovered, in 1865, a large diffused nebula north-preceding the star-cluster G.C. 1420, his attention was first directed to the ring structure by Prof. Barnard in January 1889.

The nebula N.G.C. 2237 is in the constellation Monoceros; its position is R.A. 6h. 24m. 48s., Decl. + 5° 8′; hence it will soon be favourably situated for observation, and Dr. Swift hopes that Mr. Isaac Roberts will be induced to photograph it, as a change both in brightness and form is suspected.

ON THE SPECTRUM OF ζ URSÆ MAJORIS.—An examination of seventy photographs of the spectrum of this star, taken on as many different nights at Harvard College, and beginning on March 27, 1887, has led Prof. Pickering to conclude that the K line is double at intervals of 52 days, and that, for several days before and after it is seen to be double in the photographs, it presents a hazy appearance. From the period assigned, it was predicted that the line should be double on December 8, 1889, and January 30, 1890, and the duplicity

was confirmed on the former of these dates by each of three photographs. Two more stars have been found having a similar periodicity—β Aurigæ and *b* Ophiuchi. The hydrogen lines of ζ Ursæ Majoris appear to be broader when the K line is double than when it is single. Several other lines are also seen double when the K line is double. Measures of the plates gave a mean separation of 0·246 millionths of a millimetre for a line whose wave-length is 448·1, when the separation of the K line, whose wave-length is 393·7, was 0·199.

The explanation of this phenomenon proposed by Prof. Pickering is that the brighter component of this star is itself a double star having components nearly equal in brightness, but too close to have been separated as yet visually, and some interesting results have been worked out which appear to support this hypothesis.—*American Journal of Science*, January 1890.

SPECTROSCOPIC OBSERVATIONS OF ALGOL.—A note on the motion of this star in line of sight has previously appeared (NATURE, vol. xli. p. 164). The detailed investigation of the six photographs taken at Potsdam is given by Prof. Vogel in *Astronomische Nachrichten*, No. 2947, from which the following is taken. Motion towards the earth is represented by a minus sign, and a motion of recession by a plus sign; both are expressed in geographical miles per second:—

Potsdam mean time. h.	Distance from minimum. h.	Motion in line of sight.
1888, Dec. 4, 6·6	11·4 after.	−5·0
1889, Jan. 6, 5·7	22·4 before.	+6·9
„ 9, 5·5	19·4 before.	+7·5
Nov. 13, 9·3	13·3 after.	−5·6
„ 23, 9·0	22·3 before.	+6·2
„ 26, 8·5	19·6 before.	+6·8

From these results it will be seen that, before minimum, Algol has an average motion of recession of 6·8 geographical miles per second, but after minimum it approaches the earth with an average velocity of 5·3 geographical miles per second. A reduction of the measures by the method of least squares shows the velocities per second to be—

Before the minimum, +5·3 geographical miles,
After the minimum, −6·2 „

which give an average motion of recession or approach = 5·7 miles. The entire system is found to be moving towards the earth with a velocity of 0·5 geographical miles per second.

GEOGRAPHICAL NOTES.

AT a meeting of the South Australian branch of the Royal Geographical Society, on November 1, 1889, Mr. Tietkens gave an account of his recent explorations in Central Australia. His expedition was despatched by the Central Australian Exploring and Prospecting Association, and consisted of a party of five persons, including a black tracker and a native boy. At one point of his journey, when the party came within sight of "an imposing range," Mr. Tietkens hoped to find a watercourse flowing from its slopes to Lake Amadeus. He was disappointed. No watercourse worth mentioning was discovered, nor any spring or place where water could collect. Mr. Tietkens discovered several ranges of hills, to which he gave names. One of the pleasantest places found by him he called Gill's Creek, after the hon. treasurer of the South Australian branch of the Royal Geographical Society. Here a stream flows from a range of hills through a gorge or glen of sandstone formation. "This," he says, "was a most beautiful spot, where a few days could be spent profitably, so the camels were unloaded, and Billy and myself went up the creek to explore its wonders. We found that the creek separated into three distinct channels. Following the principal one, we found the creek to be running through a glen with perpendicular cliffs 80 or 100 feet high on each side, and fully three miles in length. We returned to our charmingly situated camp late in the afternoon. . . . The water will not be found to be always running, but in the glen at the head of the creek, and which I have named after my sister Emily, large deep pools will be found, four or five chains long, 10 and 15 feet deep, and so shaded by rocks from the sun that they cannot be looked upon as otherwise than permanent." After the reading of the paper Mr. G. W. Goyder, Surveyor-General, expressing gratitude to Mr. Tietkens, said that although as an effort to increase the extent of Australian mineral and pastoral resources Mr. Tietken's expedition might have been a comparative failure, yet the route which he had travelled might serve as a most useful base for after-comers. His journey showed that no large large river, as had been hoped, flowed into Lake Amadeus, and only gave another proof that the interior of Australia consists of a series of low mountains with shallow basins, which in wet seasons form lakes and in dry seasons evaporate.

MESSRS. GEORGE PHILIP AND SON have issued an excellent map showing all Stanley's explorations in Africa from 1868 to 1889. Each expedition is distinctly marked in colour, and dated on the map; and a condensed account of the explorer's travels and discoveries is provided by Mr. E. G. Ravenstein.

THE SOURCES OF NITROGEN IN SOILS.[1]

THE number of this half-yearly Journal, issued last April, contains nineteen valuable contributions, covering a considerable portion of the large subject of agriculture. Many of them are of purely practical import, such as the report upon the previous year's prize farm competition, on implements exhibited at the Nottingham meeting, and on the Exhibition of thoroughbred stallions of February last. Among the articles of special scientific interest may be named "The History of a Field newly laid down to Permanent Grass," by Sir J. B. Lawes, F.R.S.; "Grass Experiments at Woburn," by W. Carruthers, F.R.S.; "The Composition of Milk on English Dairy Farms," by Dr. Paul Veith, and the Annual Reports of the scientific staff of the Society. The Journal contains 380 closely-printed pages, is well illustrated, and replete with tables and statistics. Among such a mass of information, all of which possesses important economic value, it is by no means easy to make a selection for special notice. The changes within the soil, in the formation of a meadow by Sir John Lawes, are, however, worthy of close attention at a time when grazing and stock-feeding appears to be the most popular remedy for the agricultural depression under which the country has so long suffered. These observations are also important scientifically, as they throw light upon the interesting question as to the sources of nitrogen in all soils. The gradual improvement of grass land, from the period when it is first laid down until it assumes the character of old pasture, is a well-known agricultural fact. The gradual increase in the amount of nitrogen per acre in the meadow selected by Sir John Lawes throws light upon this practical observation, and is recorded as follows:—"There can be no doubt that there has been a considerable accumulation of nitrogen in the surface soil during the formation of the meadow (1856 to 1888), amounting in fact to an average of nearly 52 pounds per acre per annum over the last twenty-three years. The question arises, Whence has this nitrogen been derived?" This is, as is well known, a controverted point. The balance in favour of this accumulation of nitrogen within the soil is still large, even after every source of nitrogen in fertilizers employed, foods fed upon the land by live stock, rainfall, and from every other possible source is taken into account. Therefore, Sir John comes to the conclusion that the gain of nitrogen in the surface soil must have had its source either in the subsoil, the atmosphere, or both. There is much experimental evidence pointing to the conclusion that at any rate some deep-rooted leguminous plants derive a considerable quantity of nitrogen from the subsoil. Reasoning upon the question as to how far the whole of the accumulated nitrogen in the surface soil has been derived by deeply-searching roots from the subsoil, Sir John says, "On this point we think it may safely be concluded, from the results of the experiments of Boussingault and of those made at Rothamsted, many years ago, that our agricultural plants do not themselves directly assimilate the free nitrogen of the air by their leaves. But in recent years the question has assumed quite a new aspect. It now is, Whether the free nitrogen of the atmosphere is brought into combination within the soil under the influence of micro-organisms, or other low forms, and so serving indirectly as a source of nitrogen to plants of a higher order? Thus Hellreigel and Wilfarth have found, in experiments with various leguminous plants, that if a

[1] "The Journal of the Royal Agricultural Society of England," April 1889. (John Murray, Albemarle Street.)

soil free of nitrogen have added to it a small quantity of soil-extract containing the organisms, the plants will fix much more nitrogen than was otherwise available to them in the combined form. It further seemed probable that the growth and crop residue of certain plants favoured the development and action of special organisms. It is admittedly not yet understood, either in what way the lower organisms affect the combination, or in what way the higher plants avail themselves of the nitrogen thus brought into combination. . . . Should it be firmly established that such an action does take place in the case of certain plants, though not in that of others, it is obvious that part, at any rate, of the gain of nitrogen by the soil supporting the mixed herbage of grass land may be due to the free nitrogen of the air brought into combination under the influence of the action supposed." This must be regarded as an important concession to the view that nitrogen may be derived for the purposes of plant nutrition from the inexhaustible ocean of the atmosphere, and it will probably not be long before the vexed question of the sources of nitrogen in soils will be placed upon a more satisfactory basis.

JOHN WRIGHTSON.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, December 5, 1889.—"A New Form of Wedge Photometer." By Edmund J. Spitta.

The author explained that his attention was called to the necessity of devising an arrangement of this nature during a series of experiments upon which he has for some time been engaged to ascertain the cause or causes of the discrepancy previously shown to exist when points of light are photometrically compared with objects of sensible size ("On the Appearances presented by the Satellites of Jupiter during Transit," *Monthly Notices R.A.S.*, vol. 48). This investigation has served to indicate that a portion of the error to which reference has been made arises from the wedge form itself when employed upon a disk of any appreciable area, for it will be remembered that hitherto this instrument has only been employed upon points of light such as is exhibited by the stars. Woodcuts are given to explain how this takes place, but it may be briefly stated, that as the field of view in a single wedge photometer is of necessity variable in intensity of absorption, so the *preceding* limb of a disk is not extinguished at the same part of the wedge, and so not at the same "wedge-reading," as the *following* limb. Hence when comparing two different sized disks it is not difficult to understand that an error in the "wedge-interval," technically so called, must inevitably occur. To meet this difficulty, the error resulting from which will of necessity vary with the size of the area under consideration, the new photometer has been devised.

It essentially consists of two wedges of neutral tinted glass, arranged to pass one another in equal proportions by the turning of a single milled headed screw. A little consideration suffices to show that by this exceedingly simple means, the field of view in the photometer must be absolutely uniform in density throughout its extent, but that its power of absorption can be increased or diminished by the shifting of the wedges in the manner described. Another improvement is submitted by the addition of a wheel of tinted glasses of varying density, which, by revolving in front of the eye-piece, enables the operator to employ the photometer upon objects having a wide range of intensity. The instrument in its complete form, is mounted on the *occulting eye-piece* (*Monthly Notices R.A.S.*, vol. 45) to afford the observer a means of hiding any object or objects not under examination for the time being, which it is needless to point out is a matter of great consideration in all photometric comparisons.

Mathematical Society, January 9.—J. J. Walker, F.R.S., President, in the chair.—The following communications were made:—On the deformation of an elastic shell, by Prof. H. Lamb, F.R.S.—On the relation between the logical theory of classes and the geometrical theory of points, by A. B. Kempe, F.R.S.—On the correlation of two spaces, each of three dimensions, by Dr. Hirst.—On the simultaneous reduction of the ternary quadric and cubic to the forms $Ax^2 + By^2 + Cz^2 + Dw^2$, $ax^3 + by^3 + cz^3 + dw^3$, by the President (Sir J. Cockle, F.R.S., Vice-President, in the chair).

PARIS.

Academy of Sciences, January 13.—M. Hermite in the chair.—On some new fluorescent materials, by M. Lecoq de Boisbaudran. In continuation of his recent communication the author has investigated zircon and $Z\beta$; tin dioxide and samaria; tantalum pentoxide and samaria; tin dioxide and $Z\alpha$; tantalum pentoxide and $Z\alpha$; tin dioxide and $Z\beta$; tantalum pentoxide and $Z\beta$. All these fluorescent substances are fresh examples of the number of spectra obtained from the same active material with different solid solvents. In combination with the agents the solvents must naturally always modify the wave-lengths of the bands as well as their constitution, while still leaving to the various spectra of the agents a family likeness, whereby their common origin may at once be recognized. But if the identity or diversity of two active materials has to be determined *by exact wave-length measurements*, then it becomes essential to operate with absolutely similar solid solvents.—Multiple resonances of M. Hertz's electric undulations, by MM. Edouard Sarasin and Lucien de la Rive. Certain experiments are here described, which tend to throw doubt on Hertz's well-known hypothesis on the undulatory propagation of electric induction. The reading of the paper was followed by some remarks by M. Cornu, who pointed out that it would now be necessary to receive with the greatest reserve the theoretical consequences drawn by M. Hertz from his remarkable researches, more especially as regards the measurement of the velocity with which the induction is propagated in a rectilinear conductor. His experimental method will have to be subjected to much careful study before it can be accepted as a demonstration of the identity of light and electricity.—On the relation between the electric and thermal conductivities of the metals, by M. Alphonse Berget. In a previous paper the author described an easy method for measuring, by means of simple determinations of temperature, the thermal conductivity of the different metals relative to that of mercury, whose absolute value had already been determined. He has now extended these determinations to copper, zinc, iron, tin, lead, and several other metals. The tabulated results show that the order of the conductivities is the same for heat and electricity, but that the relation of the mean coefficients of thermal and electric conductivity is not absolutely constant. Hence the law of their proportionality is only approximately correct, and subject to somewhat the same conditions as Dulong and Petit's law of specific heats.—Heat of formation of platinum tetrachloride, by M. L. Pigeon. A process is described for obtaining this substance in considerable quantities, and the heat of formation of the anhydrous chloride is determined at + 20·5 calories. To complete its thermochemical study M. Pigeon is now endeavouring to determine its heat of solution in water and that of its hydrate.—On the combinations of gaseous phosphoretted hydrogen with boron and silicium fluorides, by M. Besson. The boron compound has the formula $2BF_3.PH_3$, and is decomposed by water with liberation of gaseous phosphoretted hydrogen. The silicon compound was obtained in the form of small and very bright white crystals, their composition corresponding to two volumes of phosphoretted hydrogen gas to three of silicon fluoride or thereabouts. These and some other compounds that remain to be studied render the analogy between phosphoretted hydrogen gas and ammonia still closer.—On the state of equilibrium of a solution of a gas in a liquid, different portions of which are kept at different temperatures, by M. P. Van Berchem. These researches were made with hydrochloric acid and ammonia, their high coefficient of solubility facilitating the detection of slight differences of concentration. The results show that there exists a special state of equilibrium for solutions of gases if the lower part of the solution is cooled, and the upper part heated.—Note on the rotatory power of matezite and matezo-dambose, by M. Aimé Girard. Some numerical errors in the author's former papers on the rotatory power of these bodies (*Comptes rendus*, lxxvii. p. 995) are here rectified, and the author's fresh experiments confirm his previous conclusion that their rotatory power is absolutely identical.—Papers were submitted by M. Emile Picard, on the employment of successive approximations in the study of certain equations with partial derivatives; by MM. Maquenne and Ch. Tanret, on a new inosite ("racemo-inosite"); by M. Edouard Heckel, on the utilization and transformations of some alkaloids present in corn during germination; by M. A. Giard, on the relationship of the annelids and mollusks; by M. Léon Vaillant, on the bichique (*Gobius* and *Sicydium*) fisheries in the island of Réunion; by M. A. Vaissière, on *Prosopistoma variegatum* of Madagascar; and by M. Salomon Reinach, on the volcanic eruptions supposed to have taken place in France during the fifth century A.D.

BERLIN.

Physiological Society, December 27, 1889.—Prof. du Bois-Reymond, President, in the chair.—Dr. Augustus Waller, of London, demonstrated the electrical negative variation of the heart which accompanies the pulse. The demonstration was preceded by a short introductory description of the method by which it is possible to detect the negative variation accompanying each beat of the heart both in man and other normal animals. The peculiar position of the heart determines the special position of the equipotential lines for the cardiac muscle, and these then determine the way in which the electrodes must be applied to the outer surface of the body in order to obtain the most marked results. Thus, for instance, when one pole of the capillary-electrometer is applied to the head or right shoulder of a man, while the other pole is connected with his left hand, this arrangement is effective, and the mercurial meniscus in the electrometer can be seen to move synchronously with the pulse. When the poles are applied to the left shoulder and left foot, or left hand and left foot, or right hand and right foot, these arrangements are non-effective. In the horse, dog, and cat, results are obtained by connecting the fore-limbs with the hind-limbs through the electrometer; this is due to the fact that in these animals the heart is placed with its axis from right to left, thus dividing the body symmetrically into a front and hinder half. The demonstrations were made on a man, a horse, and a dog.—Mr. Auschütz exhibited an apparatus ("Schnellseher") for the stroboscopic examination of instantaneous photographs (twelve per second) of moving objects. The reproduction of the movements afforded by the instrument was very perfect.

STOCKHOLM.

Royal Academy of Sciences, January 8.—On our knowledge of the nature of the Antarctic regions, and on the desirableness of researches there as well planned and comprehensive as those which have been conducted by Swedish investigators in the Arctic regions during many years, by Baron Nordenskiöld. If contributions could be obtained from Australia, Baron O. Dickson and Baron Nordenskiöld would fit out a scientific expedition to the Antarctic regions to start from Sweden in 1891.—On remains of birds from the Saltholms Limestone (Upper Cretaceous) at Limhamn, in Scania, by Prof. W. Dames, of Berlin. (The right humerus, scapula, and coracoideum, of probably a wading-bird, being next the Enaliornis of the chalk of Cambridge, in England, the only European find of a Cretaceous bird. It has been named *Scaniornis Lundgreni*, Dam.)—Researches on oiazotiol combinations, by Herr Hector.—On Jurassic woods from Green Harbour, in Spitzbergen, by Prof. Schrenk, of Leipzig.—On the secretions of the digestion in the median intestines, and some phenomena in combination therewith in insects and Myriopoda, by Dr. G. Alderz.

DIARY OF SOCIETIES.

LONDON.

THURSDAY, JANUARY 23.

ROYAL SOCIETY, at 4.30.—On a Photographic Method for Determining Variability in Stars: Isaac Roberts.—Physical Properties of Nickel Steel: Dr. Hopkinson, F.R.S.

INSTITUTION OF ELECTRICAL ENGINEERS, at 8.—Magnetism: Dr. J. Hopkinson, F.R.S. (Discussion.)

ROYAL INSTITUTION, at 3.—Sculpture in Relation to the Age: Edwin Roscoe Mullins.

FRIDAY, JANUARY 24.

INSTITUTION OF CIVIL ENGINEERS, at 7.30.—The Up-keep of Metalled Roads in Ceylon: Thos. H. Chapman.

ROYAL INSTITUTION, at 9.—The Scientific Work of Joule: Prof. Dewar, F.R.S.

SATURDAY, JANUARY 25.

ROYAL BOTANIC SOCIETY, at 3.45.

ROYAL INSTITUTION, at 3.—The Natural History of the Horse, and of its Extinct and Existing Allies: Prof. Flower, C.B., F.R.S.

SUNDAY, JANUARY 26.

SUNDAY LECTURE SOCIETY, at 4.—John Milton, the Champion of Liberty: Dr. Stanton Coit.

MONDAY, JANUARY 27.

SOCIETY OF ARTS, at 8.—The Electro-magnet; Dr. Silvanus P. Thompson.

TUESDAY, JANUARY 28.

SOCIETY OF ARTS, at 8.—The Relation of the Fine Arts to the Applied Arts: Edward C. Robins.

ANTHROPOLOGICAL INSTITUTE, at 8.30.—Anniversary Meeting.—Presidential Address.

INSTITUTION OF CIVIL ENGINEERS, at 8.—Recent Dock Extensions at Liverpool: George Fosbery Lyster. (Discussion.)—Bars at the Mouths of Tidal Estuaries: W. H. Wheeler.

ROYAL INSTITUTION, at 3.—The Post-Darwinian Period: Prof. G. J. Romanes, F.R.S.

WEDNESDAY, JANUARY 29.

SOCIETY OF ARTS, at 8.—The Utilization of Blast-furnace Slag: Gilbert Redgrave.

THURSDAY, JANUARY 30.

ROYAL INSTITUTION, at 3.—Sculpture in Relation to the Age: Edwin Roscoe Mullins.

FRIDAY, JANUARY 31.

ROYAL INSTITUTION, at 9.—Smokeless Explosives: Sir Frederick Abel, C.B., F.R.S.

SATURDAY, FEBRUARY 1.

ROYAL INSTITUTION, at 3.—The Natural History of the Horse, and of its Extinct and Existing Allies: Prof. Flower, C.B., F.R.S.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Atlas of Commercial Geography: J. G. Bartholomew (C. J. Clay).—Electric Light, 3rd edition: J. W. Urquhart (C. Lockwood).—North American Birds, Parts 1 and 2: H. Nehrling (Wesley).—Handbüch der Palæontologie, ii. Abthg., 8 Liefg. (München).—Handbüch der Palæontologie, i. Abthg., iii. Band, 3 Liefg. (München).—Year-book of Photography for 1890 (Piper and Carter).—Livy, Book xxi.: Allcroft and Masom (Clive).—Queensland Meteorological Report for 1887.—Handleiding tot de Kennis der Flora van Nederlandisch Indië. Eerste Deel: Dr. J. G. Boerlage (Leiden, Brill).—Die Arten der Gattung Ephedra: Dr. O. Stapf (Wien).—Grasses of the Southern Punjab: W. Coldstream (Thacker).—Prof. Arnold Guyot; J. D. Dana (Washington).—Miscellaneous Papers relating to Anthropology (Washington).—Accounts of the Progress in Anthropology, Zoology, Mineralogy, Chemistry, Physics, Geography and Exploration, Vulcanology and Seismology, North American Geology in 1886 (Washington).—Bibliography of North American Palæontology in 1886 (Washington).—The Advance of Science in the Last Half Century: T. H. Huxley (Washington).—Report of the Smithsonian Exchanges for the Year ending June 30, 1887 (Washington).—Preservation of Museum Specimens from Insects and the Effects of Dampness: W. Hough (Washington).—Ethno-Conchology: R. E. C. Stearns (Washington).—The Human Beast of Burden: O. T. Mason (Washington).—Notes on the Artificial Deformation of Children among Savage and Civilized Peoples: Dr. J. H. Porter (Washington).—Cradles of the American Aborigines: O. T. Mason (Washington).—The Ether Theory of 1839, Part 1: J. Johnstone (Edinburgh, Gemmell).—Third Annual Report on the Puffin Island Biological Station: Dr. W. A. Herdman (Liverpool).—Journal of Anatomy and Physiology, January (Williams and Norgate).—Traité Encyclopédique de Photographie, January 15 (Paris, Gauthier-Villars).—Records of the Geological Survey of India, vol. xxii., Part 4.—Journal of the College of Science, Imperial University, Japan, vol. iii., Part 3 (Tokio).

CONTENTS.

THURSDAY, JANUARY 30, 1890.

THE HYDERABAD CHLOROFORM COMMISSION.

THE safety of anæsthetics is a subject of the deepest personal interest to everyone, either on his own account or on that of his family or friends. For this reason, the general public, as well as the medical profession, have been looking with interest for the Report of the Chloroform Commission which has lately been trying to work out the subject under the generous auspices of the Nizam and his Minister Sir Asman Jah. As we pointed out in NATURE of December 19, 1889, p. 154, two views regarding chloroform are commonly held. The one view is that it may kill by paralyzing the heart directly. The other is that it really kills by paralyzing the respiration, and only stops the heart indirectly through the asphyxia which quickly follows stoppage of the respiration. The first view is generally held in London, the second in Edinburgh, where it was strongly insisted on by the late distinguished surgeon Prof. Syme. As we learn from the Report now published, it was in consequence of his reverence for Syme's teaching, that Surgeon-Major Lawrie moved for the appointment of the Commission, which was generously granted by the Nizam's Government. This teaching was founded on clinical experience, but the results of some physiological experiments appeared to show that it was incorrect, and that chloroform paralyzed the heart directly. To ensure anything like general acceptance of Syme's teaching it was necessary that it should be shown that these experiments did not really disprove it. But this necessitated a complete revision of the whole question of the *modus operandi* of chloroform, and of the production of an immense amount of experimental evidence. This has been supplied by the present Commission, and the result of their labours appears to be that there is some truth in both views, but that when chloroform is given in the ordinary way by inhalation, it is the respiration which stops first. When chloroform vapour is blown down the trachea the heart may be stopped by it, but when the vapour is drawn into the lungs in the usual way by the movements of the chest, this is not the case, for, the respiratory movements being arrested first, their stoppage prevents any more chloroform vapour from being taken into the lungs. Embarrassment of respiration constitutes the first sign of danger, and should be at once attended to. The breathing should not be allowed to stop, but if it should do so by any accident, life may still be preserved by the immediate use of artificial respiration. Should the interval of asphyxia between the stoppage of natural breathing and the commencement of artificial respiration be too long, the heart may fail to such an extent that artificial respiration is in vain; and if the administrator waits for a failing pulse to warn him of danger, the warning may come too late. In a former research by the Glasgow Committee of the British Medical Association, some of the experiments, in the opinion of the Committee, seemed to show that chloroform not only lowers the blood-pressure and paralyzes the heart, but does so sometimes in an unexpected and capricious manner. The Commission has repeated their experiments, and found a similar fall of the blood-pressure and slowing of the pulse, but has come to a different conclusion regarding their causation, and attributes them not to chloroform but to asphyxia. If this opinion be correct, it shows how much care is necessary to avoid asphyxia, for the Glasgow Committee appear to have overlooked its presence, notwithstanding the serious effects it was producing on the heart in the animals on which they were experimenting. The work of the Hyderabad Commission points strongly to the conclusion that deaths from chloroform in man are likewise due to asphyxia, and the Commission considers that by careful attention to the respiration all deaths may and should be prevented. The Report points out that instead of the conclusions at which the Commission has arrived being opposed to those of Claude Bernard, they are almost exactly those at which that distinguished physiologist, so well known for his accurate work, had arrived, although his name is often quoted in support of the doctrine that chloroform kills by paralyzing the heart. The number of experiments on which the Commission bases its conclusions is very large, no fewer than 430 having been done without recording apparatus, and 157 with recording apparatus. The former consisted chiefly of experiments, firstly, on the general action of chloroform given in various ways, in various dilutions, and in different conditions of the animal, *e.g.* fasting, after meals, after a preliminary dose of spirits, &c.; and, secondly, on the limits within which artificial respiration could restore life, and the effect of morphine, strychnine, atropine, &c., in modifying the action of the anæsthetic and the reviving power of artificial respiration. The necessary apparatus was taken out by Dr. Lauder Brunton, and on his arrival at Hyderabad the Commission was at once constituted: Surgeon-Major Lawrie, President; Drs. Lauder Brunton, Bomford, and Rustamji, members; Dr. Bomford acting as secretary. They were greatly aided in their work by the members of the first Commission, Drs. Hehir, Kelly, and Chamarette, as well as to Messrs. Tripp, Carroll, and Mayberry, the latter of whom gave the chloroform. To Dr. Chamarette's energy and fertility of resource the success of the experiments was mainly due. The work was continued daily from 7 a.m. to 5 p.m., except on Sundays and holidays, from October 23 to December 18. From a speech made by Dr. Lauder Brunton at a dinner given to the Chloroform Commission by the Nawab Intesar Jung, we learn that the facilities for work afforded to the Commission were such as were not to be found even in the great laboratories of the continent of Europe; and, indeed, the large number of experiments which were made in a comparatively short time, is sufficient of itself to show this. At this dinner the Nawab Intesar Jung reminded his guests that Europe is indebted to Mohammedan writers of the schools of Bagdad and Cordova for the preservation of medical science during the dark ages; and as Dr. Lauder Brunton very truly said in his reply, the Nizam has not only followed the traditions of the Mussulmans in selecting the subject of research, but has rivalled the generosity of Haroun-al-Raschid and Abdurrahman in supplying the Commission with every-

thing it could require. Although the liberal endowment of universities and schools is now fortunately much more common, especially in America, than it used to be, yet there are few instances of such liberality as the Nizam has shown towards definite subjects of scientific research. For the excellent example they have shown in this matter, the Nizam and his enlightened Minister, Sir Asman Jah, deserve the thanks of the scientific world, while they also deserve that of the public in general for their endeavour to save life and lessen suffering by rendering the administration of anæsthetics so safe that they may be employed without fear whenever they are required.

HYGIENE.

Hygiene, or Public Health. By Louis C. Parkes, M.D. (London: H. K. Lewis, 1889.)

DR. LOUIS PARKES has conferred an important service by the opportune publication of his manual of hygiene. The public mind has been slow to perceive the importance of the science of preventive medicine. For nearly half a century Sir Edwin Chadwick and others have preached the doctrine. It fell for a long time on sterile ears. No doubt provisions have been made by Parliament from time to time, when some special danger or disease-cause was brought prominently into notice: not, indeed, as a part of a system of sanitary protection, but as if it were the only matter to be cared for. Thus, vaccination was made compulsory to stop small-pox, but for a long time many other diseases were ignored. These scattered efforts in sanitary legislation were brought to a focus in 1875, and systematic sanitation may be said to have been instituted by the division of the country into sanitary areas, and by the appointment of medical officers of health. These provisions were rather a theoretical recognition of the importance of the subject than a practical creation of efficiency, for the medical officers in a large number of instances have not received such remuneration as would enable them to give their whole time to their duties; nor do they possess security of tenure. They have been, for the most part, men in local practice, who have been content to receive an honorarium in some cases as low as £20 or £10, and occasionally even £5 and £3 a year. Such payments could not be expected to induce men to do more than give a nominal service to their official duties; and it is, indeed, notorious that in many instances the object of members of the sanitary authority which has made the appointment, who are themselves owners of house property, has been to nominate men who would let matters rest, and would not compel owners of cottages to spend money on sanitation.

We are now, however, entering upon a new era in sanitation. The creation of County Councils which took place last year has introduced a new feature. Although the powers vested in these bodies are permissive and somewhat tentative, it has already become quite certain that they will, sooner or later, bring the whole sanitary service of the country under their general supervision and control.

The Local Government Act of 1888 lays down the provision that the medical officer of health to be appointed by a county must be qualified in sanitary knowledge—that is to say, in the knowledge of the prevention of disease, as distinguished from curative knowledge. It will, therefore, be necessary that the men appointed shall have spent time and money in obtaining the required qualifications for their duties: hence they will expect adequate salaries to remunerate them for the trouble and expense which they will have incurred in thus educating themselves. The call for education in preventive medicine will react upon the medical schools and the various degree-conferring bodies—such as the Universities—and will compel them to hold examinations in, and to confer diplomas or certificates upon the possessors of, sanitary knowledge. Moreover, the sanitary authorities, in order to justify to themselves the higher salaries which they will be compelled to pay, will be induced to place enlarged areas under the medical officer, and, in order that he may effectually perform his duties, he will insist on being furnished with a better educated staff of sanitary inspectors or inspectors of nuisances than have been, as a rule, appointed under the old *régime*.

It is thus evident that there will soon be a great call for sanitary education, and Dr. Parkes's volume forms a very useful commentary upon what are the general heads comprised in a course of instruction in the methods necessary for applying various branches of science to the prevention of disease. A glance at the table of contents shows the very large field embraced under the title of preventive medicine. It concerns not only the medical man, but the engineer, the architect, the chemist, the physiologist, the meteorologist, and the statistician. The questions to be studied include climatic conditions; the effect on health of the state and movement of the atmosphere; the health of soils; the purity of water-supply, and the prevention of injury to health from fouled water; the construction of buildings, their warming, lighting, and ventilation; questions of food and clothing; the history of communicable diseases; and bacteriology, as well as hygienic chemistry and statistics.

A brief summary of the present position of our knowledge shows us that preventive medicine is still far removed from being an exact science. We have, no doubt, lately made much progress in removing from the medical man the imputation that his proceedings were empirical. Physiological studies in recent years have established the relationship between certain diseases and the presence of micro-organisms; and although this relationship may not be as universal as some persons would hold, yet we know that there is a positive relationship in the case of certain diseases. When the causes of diseases are known; when the action of the causes can be studied, and their mode of entrance into the body ascertained; when the methods which can be applied to their destruction are discovered; then the science of the prevention of disease ceases to be empirical.

Whilst, however, our progress in this knowledge has of late years been extremely rapid as compared with former experience; yet when, as in this volume, we are brought face to face with the various problems of the prevention of disease, we are amazed to find what a vast field is still unexplored in the knowledge of the causes of disease. Dr. Parkes has given a very interesting summary of our knowledge on this part of the question in his chapters on

contagia and communicable diseases. We may be said at present to be only standing on the threshold of this very intricate subject. Even in the case of those diseases which have been ascribed with the greatest assurance to the presence of organisms in the blood or the tissues, we are told that it is as yet uncertain whether the symptoms of disease are the results of the direct action of microbes themselves upon the tissues, or are caused by their indirect action in producing poisonous alkaloids or ferments. We have not yet elucidated the curious connection between the diseases of animals and mankind; but whilst we are gradually acquiring the conviction that some diseases from which animals suffer are communicable to the human race, it is at any rate satisfactory at the same time to have arrived at the certainty that those laws of cleanliness in air, soil, and water, which are the basis of human sanitation, are the most effective safeguards to be observed in the case of domestic animals, if certain classes of disease are to be avoided. But with all our increased knowledge of the existence and methods of propagation of the various forms of organisms which appear to co-exist with certain forms of disease, we have not yet discovered why certain diseases become epidemic at certain times, whilst they lie comparatively dormant at other times; Nature has not yet revealed all her secrets to the microscope or to the laboratory.

Take as an instance, the influenza which is now present with us. Its epidemics are historical. It has appeared over and over again at somewhat distant intervals. But we do not know why it comes at one time and not at another. It has been specially described on various occasions since 1557. In 1837 it covered the whole of the north of Europe in fifteen days. It travels as rapidly through sparsely inhabited as through populous countries. In 1780 it manifested itself in ships in mid-ocean, which had had no communication with the shore. The facts connected with its incidence are thus well known. Its progress would scarcely seem to be accounted for by contagion or infection in the common acceptance of the word. Is its present advent due, like the beautiful sunsets with which we were favoured a few years ago, as some observer suggests, to a catastrophe in some distant part of the globe? or is it owing, as M. Descroix, of the Meteorological Observatory at Montsouris, tells us, to the remarkably stagnant atmosphere of last autumn? Large populations agglomerated in towns depend, for the removal of the foul emanations continually passing into the atmosphere from their midst, upon the action of winds and storms, and these causes of ventilation were notably absent during the past autumn; and Dr. Descroix points out that the failure to remove this impurity would favour the propagation of organisms injurious to the health of the community, acting in this respect just as a festering drain or manure heap would act.

The advance which each separate science makes opens out new views to the hygienist, and this short reference to the epidemic of influenza serves to point out the extent of the subject, and to impress upon us the fact that it is almost impossible that a moderate-sized treatise by a single individual could form an adequate text-book for the student in these various and intricate questions.

Dr. Parkes's volume, admirable as it is in many respects, leaves something to be desired in its treatment of some of the subjects. We would especially refer to those relating to civil engineering and architecture, which are not the special subjects of a medical man. The treatment of these branches presents some weak points, and there is an occasional tendency to recommend specific inventions rather than to enunciate principles, which may somewhat militate against the general acceptance of the volume as a complete and permanent text-book.

It would have been better if the educational features of the book had been limited to those special subjects with which the profession of the author has made him most familiar. The work is, however, a convenient hand-book, and will serve as a valuable guide to show the student what are the several subjects which have to be studied; and in that sense we can safely recommend it as an adjunct to the library of every sanitarian.

IN THE HIGH ALPS.

Im Hochgebirge. Wanderungen von Dr. Emil Zsigmondy. Mit Abbildungen von E. T. Compton. Herausgegeben von K. Schulz. (Leipzig: Duncker and Humblot, 1889.)

THIS handsome volume possesses a melancholy interest, for it is in reality a memorial to a young and ardent mountaineer who was killed by a fall from a precipice in the year 1885. Emil Zsigmondy was by descent a Hungarian, but was born and educated in Vienna, where his father practised as a physician. The son followed the same profession, of which he was a distinguished student. As a boy he showed a love of mountain-climbing. At the age of fifteen, he and his brother Otto, without guides, made an ascent of the Reiseck, a peak 2958 metres high. The expedition occupied twenty-six hours, of which twenty-two were spent in actual walking, a remarkable feat of endurance on the part of two lads.

After this Emil made annually an Alpine excursion, the expeditions increasing in difficulty and (with the exception of one year) in number. The first of which a description was published was accomplished in his eighteenth year, and after this references to the journals of foreign Alpine Clubs and similar publications are frequent on the list. Altogether, as we are told in the brief biographical notice prefixed to this work, Emil Zsigmondy, though he perished a few days before completing his twenty-fourth year, had climbed nearly 100 summits of more than 3000 metres in height above the sea—in more than nine cases out of ten unaccompanied by guides. Most of the expeditions described in this volume have already appeared in various journals, and describe excursions which in themselves are not new; but many of them have this special interest, that they were made without guides. Sometimes the brothers were alone, but on the more difficult excursions they were generally accompanied by one or two trusty friends, such as Prof. Schulz, editor and part-author of this work.

The book is a record of Alpine expeditions told in plain but graphic language. It scarcely touches upon scientific questions, though we are informed that Emil was a student of Alpine botany, zoology, and geology, and published some observations on these subjects in a work which

appeared before his death. But now and then a chance remark indicates the geologist, and there is an interesting account of a remarkable appearance of the "Brocken spectre." This was witnessed from a rocky ridge near the summit of the Bietschhorn, a lofty peak on the southern side of the Bernese Oberland. The shadow of the observer was seen within a triple rainbow-ring. Of these rings, the inner one exhibited the usual tints; these were weaker in the second, and barely visible in the third. The shadow was larger than life, but was less than the diameter of the inner ring. By this, according to the text, it was encircled; but in the accompanying woodcut the shadow of the legs from below the knees is thrown upon the rings. The sun was getting low, and towards the west, for it was nearly 4 o'clock on an afternoon early in September. The wind came from the same direction, and the clouds were drifting eastwards from the mountain-peak. The "spectre" remained visible for nearly an hour, while the observers completed the ascent to the actual summit.

The illustrations are numerous, and some of them are not without a scientific value as faithful renderings of mountain scenery. It is seldom that the same can be said of similar engravings in English books. These, if no longer the caricatures which were formerly supposed to represent mountains, are still too often devoid of character. Mr. Whymper can and does give the outline of a mountain peak and the distinctive features of its rocks, but the ordinary illustrator is content with some conventional smudging which serves impartially for granite or limestone, for schist or slate, and is equally unlike each one of them, or, indeed, anything that exists on this earth. But as our artists are at length beginning to realize that Nature's workmanship is better than their own, and to follow the path which was trodden by Turner, Elijah Walton, Ruskin, and a few pioneers, we may hope that the illustrations of mountain scenery in English books may rise to the level of Continental publications, which, though not free from mannerisms, do make some attempt at accuracy. Those in the present work consist of eighteen full-page photogravures, copied apparently from water-colour drawings, and of a large number of woodcuts, which are in part from finished drawings, in part from pen-and-ink outline sketches. Many of the former are excellent, so also are some of the latter; but these are less successful in representing scenery than in recording little incidents in the mountaineers' experience. The simple unaffected narrative of adventure, in which there is evidence of skill in dealing with mountain difficulties, and courage, pushed, perhaps, sometimes to the border of rashness, is very pleasant to read, and it is sad to think that such a life has been lost to his many friends. The fatal fall occurred during an attempted ascent of the Meije, in Dauphiné, by a new route up the southern cliffs. Emil had climbed some distance above his two companions, when he fell from a cliff. They bravely attempted to check his descent by means of the rope which was attached to his waist, but it snapped under the strain, and the climber in a few moments lay lifeless on a glacier 2000 feet below. A full account of the accident was published in the *Alpine Journal* for 1885, which indicates that on this occasion more risk was being incurred than could be justified. T. G. B.

THE STORY OF CHEMISTRY.

The Story of Chemistry. By Harold Picton, B.Sc., With a Preface by Sir Henry Roscoe, M.P., D.C.L. LL.D., F.R.S. Pp. 386. (London: Isbister, 1889.)

IT is a matter for surprise that, among the many books on the different branches of chemistry, so few are to be found devoted to the historical treatment of the science. The ordinary student in attempting to get an idea of the development of the subject labours under considerable disadvantages. From time to time, indeed, our professors are to be heard expounding "The Atomic Theory," "Joseph Priestley," "The Birth of Chemistry," and like topics; books on such subjects also exist. Our larger treatises, as a rule, have short historical introductions; text-books, too, occasionally contain information such as "the gas discovered by Rutherford in 1772 was subsequently named nitrogen by Chaptal." From such sources, however, a conception of the fundamental discoveries which have led up to the chemistry of to-day is only possible by dint of much searching, and at an expenditure of time far beyond that at the disposal of most students. A short history of the science in a handy form would be a decided acquisition to chemical literature. The name of the little volume before us is thus a promising one, and on perusal, the book in no way belies its title.

After showing who the alchemists were, and the state of chemical knowledge before they appeared on the scene, the author proceeds to divide his subject into nine periods. The first of these, "Alchemical Mysticism," extending from the time of the mysterious Hermes Trismegistus to that of Roger Bacon and Raymond Lully, includes also an account of Geber and Albertus Magnus. Next comes "Medical Mysticism," in which are sketches of Basil Valentine and his "Triumphant Chariot of Antimony," of Paracelsus and Van Helmont; followed by the "Decline of Mysticism," reaching down to the founding of the Royal Society of London by Charles II. in 1662, and embracing the work of Glauber and Helvetius. The fourth period, "The Beginnings of Science," deals with Boyle, Hooke, Mayow, and Hales. The reader's attention is then directed to Black's introduction of "weighing" as a means of investigation. This chapter, which gives, besides, a pretty picture of Cullen, Black's instructor, constitutes the "Childhood of Truth." Then follows, under the heading of "The Conflict with Error," a succinct account of the rise and progress of Stahl's phlogiston theory, with its bearings on the researches of Priestley, Cavendish, Scheele, and their contemporaries. Lavoisier's keen penetration and masterly deductions, "The Triumph of Truth," are then discussed, and lead up to "The Atomic Theory," Dalton's idea, and its later developments, from the time of Gay-Lussac, Ampère, and Avogadro, to that of Newlands and Mendeléeff. After a separate chapter on Davy and Faraday the book is brought to a close by short descriptions of the present state of inorganic and organic chemistry.

Mr. Picton's style is fresh and pleasing; his descriptions are clear and to the point. Whenever possible, brief surveys of the life and work of the men of science mentioned are given. Extracts from original writings are frequently quoted, and pains taken to enable the reader to form an idea of the general character of the individuals apart from their chemical discoveries alone. Chronological

order has not in every case been adhered to, the main idea and its subsequent development being frequently treated together; but the sequence of epoch-making events is strictly maintained. The work is quite up to date; when advisable, the author has introduced facts which have only been established by recent investigations.

The book is tastefully bound, and the illustrations are numerous. The latter are varied, and embrace cuts from "Die Zwölf Schlüssel," apparatus historic and modern, and portraits of celebrated chemists. To the reader possessed of some chemical knowledge the volume will be most useful, and to the uninitiated its earlier chapters, at least, cannot fail to be inviting.

LUMINOUS ORGANISMS.

Les Animaux et les Végétaux Lumineux. Par Henri Gadeau de Kerville. (Paris: J. B. Baillière et fils, 1890.)

THIS little book is a semi-popular summary of what is known in regard to the photogenous structures of the various kinds of luminous animals and plants, commonly (but improperly, as the author points out) known as phosphorescent. As it is on the whole fairly complete and accurate, being based largely upon the important researches of Panceri, Sars, R. Dubois, Emery, and others, it will probably be useful not only to amateurs, but also to students who wish to get a general knowledge of the range in organic nature of light-producing forms, and of the more important investigations on the subject which have been made since the days of Aristotle and Pliny.

Although the title-page of this book bears the date 1890, the important discovery by Giard, in September last, of luminosity in Amphipods which is due to an infectious disease is, it may be supposed, too recent to have been included—at any rate, it is not referred to.

After a short historical *résumé*, the first half of the work (170 pages) is occupied by a systematic account of those plants and animals which are luminous, commencing with the plants and then working up through the animal series from Protozoa to Vertebrata. More animals than plants are photogenous, and most of these are marine. Few observations have been made upon freshwater forms, and none are known from brackish water. M. Gadeau de Kerville takes care to point out, what is undoubtedly the case, that many supposed instances of luminosity, especially in dead animals or in the neighbourhood of harbours, &c., where there is much decaying organic matter, are due, not to any "phosphorescence" of the animal observed glowing, but to the presence of luminous Bacteria on the surface, in mucus, or in the tissues. Several species of light-producing micro-organisms (*Bacilli* and *Micrococci*) are already known, and the list will probably be largely added to in the future. It is, however, an excess of caution to doubt the claim of *Ceratium* (*Peridinium*) to be placed amongst photogenous genera, as two or three of the species appear to be responsible for a good deal of the "phosphorescence of the sea" around our western coasts in autumn—a phenomenon which is usually attributed even by naturalists to *Noctiluca miliaris*, although at such times it often happens that not a single *Noctiluca* is caught by the townet!

The well-known observations and experiments of Panceri on *Pennatula* and other forms are given, and the figures reproduced, and it will no doubt be useful to many to have the information obtained by various investigators thus collected into one volume. On p. 83 is given an observation by Quatrefages upon certain luminous *Talitri* (Amphipod Crustaceans) on the beach, which he supposed had derived their luminosity from contact with *Noctiluca*. Is it not more probable that, like Giard's diseased Amphipods at Wimereux (which, by the way, have turned up lately at Jersey, and will probably be found to be widely spread), they were infested by a photogenous microbe?

In connection with the remarkable "luminous globules" of some Schizopods (*Euphausia*, *Nyctiphanes*, &c.), M. Gadeau de Kerville suggests that these organs are light-perceiving, as well as light-producing, and that, therefore, the old designation of "accessory eyes" was not improperly applied. This view is supported by several observed cases where the true eyes of higher Crustacea were luminous; but it should be remembered that it is entirely opposed to the matured opinion given by G. O. Sars in his Report on the *Challenger* Schizopods.

Chapter xiii. is devoted to an account of the anatomy and physiology of the photogenous organs, in which, however, little of importance is added to what was given in the preceding part of the book. The author adopts the view of Dubois (founded upon experiments on *Pholas dactylus* made at the Roscoff Laboratory), that in all cases the luminosity is a purely physico-chemical phenomenon, and is dependent on the presence of two substances—the one (*luciférine*) soluble in water and obtainable in the crystalline state, the other (*luciférase*) a soluble ferment (like diastase)—which must be brought in contact in order that light may be produced. This seems going further than our present knowledge really warrants. The light-producing organisms and organs are so varied that it is possible that the causes of the luminosity may be manifold; and, at any rate in the higher forms, the bringing together of the *luciférine* and *luciférase* must be under the direct control of the nervous system, as the production of light is a reflex, perhaps in some cases a voluntary, action.

In a short chapter, entitled "Philosophie naturelle," the author considers, from the evolution stand-point, such questions as the origin of luminosity, the reason why only relatively small numbers of animals and plants are luminous, why the majority of luminous animals are marine, &c.; but for a discussion of these points, and also of the various uses (both to animals and to man) which the luminosity may have, reference must be made to the book itself, which, although some of the illustrations are poor, and there is unnecessary repetition and verbosity in the text, forms a readable and useful introduction to a very interesting and important subject.

W. A. HERDMAN.

OUR BOOK SHELF.

The Chemistry of Photography. By R. Meldola, F.R.S. (London: Macmillan and Co., 1889.)

THIS work is well worthy of study by serious devotees of photography. It enters, as its title indicates, into the chemistry of photography, and that in a very thorough and

easily understandable manner. There are some very few points in the author's explanations of phenomena as regards which we cannot quite agree with him. For instance, when he is considering the action of light on silver chloride he states that an oxychloride is formed (on the authority of Dr. Hodgkinson). That this is not always the case is shown by the fact that silver chloride is darkened when exposed in the presence of bodies which contain no oxygen, as, for instance, when the exposure is given in benzene. The author has adopted the plan of calling his chapters lectures, and in this instance we shall find no fault with what often is an artifice to cover slipshod writing, since the subject-matter is good, the language clear, and descriptive experiments are appended after each note in the narrative. We feel assured that if a student be fairly grounded in elementary chemistry and carries out these experiments, he will have a far better knowledge of the theory of photography than nine out of ten students possessed before this work was written. The author rightly points out that much in the theory of photography still requires elucidation, and with this we quite agree; but by putting into a connected shape those portions of the theory which may not require reconsideration, he has done much towards facilitating the solution of the remaining problems which are still *sub judice*.

The Popular Works of Johann Gottlieb Fichte. Translated from the German by William Smith, LL.D. With a Memoir of the Author. Fourth Edition. In Two Vols. (London: Trübner and Co., 1889.)

THESE volumes form part of the well-known "English and Foreign Philosophical Library." The translations included in them were first published in 1845–49, when German philosophy had only begun to attract attention in England. Fichte holds so clearly marked a place in the development of modern thought that it is still worth the while of students to make themselves familiar with his governing ideas; and there can be no disadvantage in their beginning with his popular rather than with his more systematic works. So far as the form of Fichte's teaching is concerned, it cannot of course be said to meet the needs of the present day. To many minds there is something even irritating in his use of large, abstract expressions, which are incapable of precise definition, and in the dogmatic tone in which he proclaims his convictions, as if he had somehow had special access to the sources of absolute truth. But his effort to solve the questions which lie behind the problems of physical science has at least the interest that belongs to perfect sincerity; and his methods and conclusions, whether they commend themselves to our judgment or not, are often in a high degree suggestive. He was personally of so manly and noble a character that his popular writings, in which he expressed his sympathies and tendencies freely, are perhaps more valuable from the ethical than from the strictly intellectual point of view. Dr. Smith's work as a translator is, we need scarcely say, excellent; and the like may be said of his work as a biographer. His memoir of the philosopher is written in a thoroughly appreciative spirit, and with adequate knowledge.

Travels in France. By Arthur Young. With an Introduction, Biographical Sketch, and Notes, by M. Betham-Edwards. (London: George Bell and Sons, 1889.)

EVERYONE who has given even slight attention to the pre-revolutionary period of French history knows, at least by hearsay, something about Arthur Young's "Travels in France." No other work of that time throws so clear and steady a light on the social and economic conditions which prevailed among the mass of the French people immediately before their great national convulsion. This is well understood by French historical students, who have found in the record of Young's observations a mine of information on the very subjects about which they are most anxious to obtain trustworthy contemporary statements. The present reprint deserves, therefore, to be cordially welcomed. It has been carefully edited by Miss Betham-Edwards, who, in an interesting introduction, prepares the way for the study of the book by presenting "a contrasted picture of France under the *ancien régime* and under the third Republic." She also gives a valuable biographical sketch of Arthur Young, the materials having been supplied by his grandson.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Acquired Characters and Congenital Variation.

MR. DYER accuses me of invading the pages of NATURE by methods of discussion characteristic of the political debater. Those methods, however, may be good as well as bad. In addition to direct affirmative arguments in support of a particular conclusion, they may trace the working and the power of preconceptions which in science, as well as in other things, are an abounding source of error. On the other hand, methods of debate may be confused and declamatory, dealing in vague phrases, and delighting in clap-trap illustrations. If I could not handle a scientific question by some method less adapted to the "shilling gallery" than the method of my censor in this case, I should wish to be silent for evermore. In his letter I see "Teleology" compared to "a wise damsel" who is "temporarily ruffled," but who nevertheless "gathers up her skirts with dignity." I see Addison brought in, head and shoulders, with "the vision of Mirza." I see Fortuity described as "inseparable from life," with the somewhat obscure oratorical addition that "it is at the bottom one of the most pathetic things about it." I see mixed metaphors of all sorts and kinds, "the church," and "orthodoxy," and "automatically self-regulating machines," and "tenacity about outworks"—and many other such words and phrases—all handled according to methods which do not strike me as at all perfect examples of true scientific reasoning.

Nor am I able to follow Mr. Dyer's logic better than I can admire his declamation. The object of my last letter, which he attacks, was to lay down and defend the proposition that "there is no necessary antagonism between congenital variation and the transmission of acquired characters." Mr. Dyer admits this proposition to be "perfectly reasonable," adding, in respect to this supposed antagonism, "theoretically there is none." But then he proceeds to say, "this does not make the transmission of acquired characters less doubtful." In other words, the complete and effectual removal of an adverse presumption is of no value in an argument which rests altogether on difficulties and doubts. This would be unreasonable enough considered merely in the abstract. But it becomes still more unreasonable when we recollect that the whole doctrine of evolution implies, of necessity, the continual rise of new characters and the transmission of them. These new characters are "acquired" in one sense, and they may be congenital in another. They not only may be, but probably they must be, acquired from latent congenital tendencies, and they may be fixed and transmitted only by those tendencies ceasing to be latent. On this view of the matter, the present controversy between the two conceptions becomes a mere logomachy. The different breeds of dog do undoubtedly transmit characters which have been "acquired." But it is always possible to assert, and always impossible to deny, that these characters arose out of congenital tendencies latent in the species. Mr. Dyer's assertion that this method of reconciling the two ideas "does not make the transmission of acquired characters less doubtful," is an assertion, therefore, which is obviously wrong. The reconciliation attacks the difficulty about the "inheritance of acquired characters" at its very heart and centre. It shows it to lie—as a thousand other difficulties have lain before—in an ambiguous word. "Acquired"? Yes; but from what? From "use"? Yes, but whence came the possibility

of "use"—and the tendency—or the disposition—or the instinct—to use? The answer may be, and perhaps always must be, that the possibility of each new use, and the disposition to it, has been acquired from the evolution of elements inherent in the germ.

The next specimen of pure scientific reasoning which I find in Mr. Dyer's letter is involved in his rebuke to me for having made an assertion in support of which I have produced no "definite observed evidence." That assertion he correctly quotes thus:—"All organs do actually pass through rudimentary stages in which actual use is impossible." He challenges me for proof. I return the challenge, and summon Mr. Dyer to produce one single instance of any animal which does NOT pass through such stages. It is the universal law of all organic beings. In some germ—in some bud—in some egg—in some womb, every living thing begins to grow. Moreover, what is true of it as a whole, is true of all its parts. All its organs—be they few and simple, or many and complex—pass through stages of incipience, of impotence—of divorce from even the possibility of actual and present use. It is truly an astonishing circumstance that any scientific man should ask for any proof of this. It is a signal illustration of the power of neglected elements in reasoning—of the familiar becoming the practically unknown, because it is the unconsidered.

Possibly, Mr. Dyer may say that he did not understand me to make the assertion of each individual organism. But this is a distinction without a difference. If the Darwinian theory be true, there has never been any other origin for species than the origin of a few first germs—developed ever since by the processes of ordinary generation, through a succession of individuals. The well-known generalization of Darwinian embryologists is that the fœtal development of the individual organism is the type and repetition of the development of species in the womb of time. In the doctrine of "prophetic germs," which he quotes as mine, nothing is mine except, perhaps, the adoption of the words. It is the embodiment, in what I hold to be accurate and appropriate language, of the most familiar facts in nature, and of the intellectual conceptions which are their necessary counterpart in mind.

There is one consequence necessarily following from this conception, which is seldom thought of and never fully accepted or recognized; and that is, that, if every organism has been developed from older organisms by very slow and gradual and minute changes through unnumbered ages, there must have been a constant succession of new organs coming on, along with an equally constant succession of other organs passing off. I see no escape from this conclusion. Yet if it be true, nothing can be more unreasonable than to wonder at the occurrence of structures which are divorced from actual use, and which are variously called "rudimentary," or "aborted." The common interpretation always is that they are the inherited remains of structures which have been once in full use, and have been lost by the atrophy of disuse. This may or may not be true, according to special facts in each case. But that there has always been in time past a series of incipient structures on the rise for actual use in future generations of development is a necessary consequence of the Darwinian hypothesis, and indeed of all other forms of pure evolutionism. The only escape from it is the supposition that special organs may have arisen suddenly—may have advanced rapidly into functional use—as rapidly as a caterpillar rushes into the structure of a butterfly, after a short interval of inactivity or sleep.

This is possible—this is at least conceivable. Nay more, this may have been the process by which new species have been introduced. But this is not Darwinism. The occasional introduction of new germs, with new potentialities, and the "hurrying up" of these through rapid stages of development, or of hatching, is an idea which, if I remember right, did not escape the speculative glance of Darwin. But it was too incongruous to be easily assimilated with his special formulæ, and so his fine eye glanced off it again, after only a momentary look; and at a later date he was so biassed in favour of the mechanics of fortuitous variation that he came to regard the very idea of development being guided towards any use yet lying wholly in the future as incompatible with his theory, and indeed destructive of it.

Mr. Dyer says that there was nothing in my last letter "which has not been worn threadbare by discussion." If so, it seems a pity that Mr. Dyer should have interposed in a discussion which he thinks exhausted. This impression may account for the poverty of the contribution made by an able man to a subject which is perhaps the most difficult, the most interesting, and the most far-reaching which can engage the human understanding. ARGYLL.

Inveraray, January 19.

Multiple Resonance obtained with Hertz's Vibrators.

WHILE Mr. Trouton and I were carrying out some experiments to try and drive an independent current through the arc formed when a spark passes in a Hertzian resonating receiver, we succeeded to some extent in doing so, but obtained an unexpected result which may be of service to others working upon this matter. We found that if the two sides of the receiver be connected with a delicate galvanometer, it is affected whenever a spark passes. It is not so easy to get sparks to pass when the galvanometer is so connected as when the receiver is insulated; but whenever a spark passes, the galvanometer—a 7000-ohm Cambridge Scientific Instrument Company's pattern—is deflected through several degrees and often off the scale. It is not very easy to see how the action takes place, because one would imagine that an electro-dynamometer would be required. The current is reversed along with the reversal of the primary induction, and seems to be connected with the direction of the electromagnetic impulse that first breaks down the air-space in the receiver: an explanation founded upon this consideration explains the facts so far, but further investigation is required to fully confirm it. We have found this method of observing the Hertzian phenomena, which we have worked successfully with an apparatus giving a wave-length of 0·6 metre, much more satisfactory than the method founded on utilizing the conductivity of the spark as a path to drive an independent current either across or along. Some experiments in a vacuum tube, however, showed that this method is capable of extension. We found it also more satisfactory than a bolometer method, which, however, worked fairly well. For this we interposed, instead of the spark-gap, a very fine wire, which was made into one of the arms of a Wheatstone's bridge. The great desideratum was a *very* fine wire, and we intend trying silvered quartz fibres if we can obtain them, and lead drawn inside glass, &c., our hearts having been broken trying to use that brittle beauty, Wollaston wire.

Any of these methods, in which your observing apparatus, the galvanometer, can be at a distance from the receiver, is more manageable than ones like that described by Mr. Gregory, in which the receiver is itself also the observing apparatus. We exhibited our method of observing the occurrence of spark by connecting the ends of the spark-gap with a delicate galvanometer at the meeting of the Dublin University Experimental Science Association last November. GEO. FRAS. FITZGERALD.

January 25.

AS I see from a notice of the proceedings of the Academy of Sciences, Paris, in last week's NATURE (p. 287), that MM. Edouard Sarasin and Lucien de la Rive have observed the fact that "multiple resonance" can be obtained by using different sized resonators with a Hertzian "vibrator," I adjoin the following short account of experiments of a somewhat different character made during last autumn, which have led to the same results, and which were brought before the notice of the Dublin University Experimental Association last November. Since then I have learnt what these experimenters also seem not to have known—that some of Hertz's earlier experiments were more especially concerned with this very fact.

First, it was found that the wave-length in the Hertzian experiment of loop and nodes formed by reflection from a large metallic sheet had altered since the apparatus had been last used some months previously. This was attributed at first to something in the "vibrator," such as the width of the spark-gap; but ultimately, on remembering how an accident had necessitated a new resonator being made, the cause was recognized—namely, that it was not exactly the same size as the previous one; and when several resonators of different sizes were made, they were found to give the node at different distances from the reflecting sheet. The intensity of the sparking with which these were affected increased with their size up to a certain point, and then diminished. So that it seems as if a "vibrator" did not send out a "line spectrum," so to speak, but sends out a "band spectrum," the centre of which is the brightest. The period, then, of a "vibrator" is that belonging to the centre of this "band."

Again, in like manner, a "resonator" was always found to give the node in different positions according to the size of the "vibrator" employed. This is what would be expected from the principle of resonance, a resonator being able to respond to any member of the "band" it would itself give out when acting as a radiator, the central period of course with the greatest ease. Some such factor as $\epsilon^{-(\lambda-\lambda_0)^{2n}}$ could, perhaps, express this sort of thing, where λ belongs to the period of the radiation, supposed for the moment "monochromatic," falling on the resonator, and λ_0 belongs to the "period" of the resonator, or that of the centre of its "band."

The position of the node was also found to vary on altering the character of the dielectric surrounding the resonator; even laying a piece of sealing-wax on the wire of the resonator was sufficient to be observed. This might be employed to determine "V" in a dielectric of which only a small quantity was obtainable.

It is obviously of importance for the "central period" of the resonator employed to coincide with that of the vibrator, especially when determining the velocity of the disturbance, for this is presumably the period given by theory. This is practically always done when arranging their relative sizes, so as to obtain greatest intensity. So that the caution urged by M. Cornu in connection with Prof. Hertz's measurements of this velocity seems, from these considerations, to be to a great extent unnecessary.

It would obviously be of importance to investigate what form the resonator should take, so as to give out a radiation most approaching one definite period. FRED. T. TROUTON.

Bourdon's Pressure Gauge.

As there does not seem to be in any of the more familiar text-books of Physics or Engineering any satisfactory explanation of the action of the Bourdon gauge, the following may be of use to some of your readers.

What we have to explain is the uncurling of the gauge under internal pressure whether of gas or liquid.

Instead of the usual flattened tube of more or less elliptical section

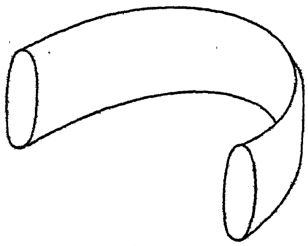


FIG. 1.

bent into the arc of a circle as in Fig. 1, think, for convenience, of one of rectangular section, such as AB of Fig. 2, in which A

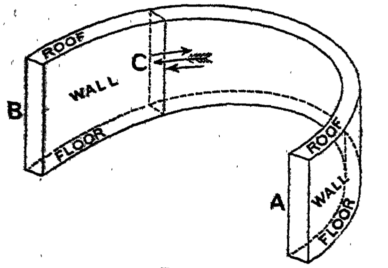


FIG. 2.

is the fixed and B the free end, and in which we shall distinguish, as indicated, the walls, roof, and floor.

If the annulus of tube were complete, as shown in the central cross-section (Fig. 3), then it is evident that under the influence of internal fluid pressure the outer wall would be everywhere in a state of tension in the direction of its length, and the inner wall in a state of compression. In the immediate neighbourhood of the ends A and B this state of compression or extension will be somewhat modified, but at a sufficient distance from either the condition of the walls will be the same as if the annulus really were complete.

If T be the tension of the outer wall in the direction of its length, P the pressure of the inner, and R the resultant fluid pressure on any cross-section such as A or B (Fig. 2), then for the equilibrium of the half of the annulus lying on either side of the diameter AB (Fig. 3) we must have

$$T = P + R.$$

Consider now the equilibrium of any portion BC (Fig. 2) contained between the free end B and a cross-section C at some little distance from B, when the internal pressure is applied, and before uncurling takes place.

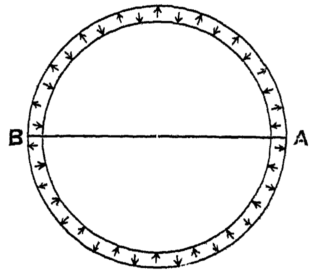


FIG. 3.

Imagine the fluid within BC to be solidified, then the external forces acting on BC (see Fig. 4) reduce to

(1) A tension, T, due to the action of the outer wall beyond C.
(2) A pressure, P, " " inner " "
(3) A resultant fluid pressure, R, acting at the centre of pressure of the cross-section C.

and since $P + R = T$, these reduce to a couple tending to uncurl the tube, and the same holds for all sections sufficiently removed from A and B.

As the tube uncurls, however, new forces come into play, viz. the resistance to bending of the walls, but especially of the floor and roof of the tube, whose width in the direction of a principal

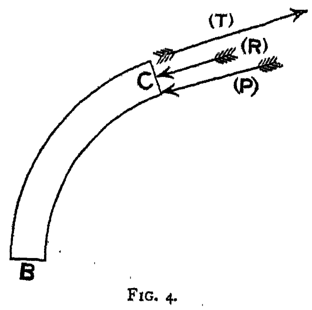


FIG. 4.

radius of the annulus, and consequently whose resistance to bending, is much greater than that of the walls. Uncurling goes on till the moment of the couple resisting flexure is equal to the moment of the bending couple.

It is evident from this explanation that even a tube of circular section would tend to uncurl, but that it would be very insensitive on account of its strength to resist flexure, and that up to a certain point sensitiveness is gained by having the walls of thin material, high, and very near together.

Devonport, December 23, 1889. A. M. WORTHINGTON.

Foreign Substances attached to Crabs.

REFERRING to Mr. F. P. Pascoe's letter (NATURE, December 26, p. 176), I cannot refrain from expressing my astonishment at his inability to "see where protection comes in" in the case of crabs covered with sponges, Polyzoa, &c. I should have thought it obvious to everybody that slow-moving crabs, such as all those he mentions and many others that I have seen, would have a much better chance of escaping their enemies when

covered with material that renders them almost indistinguishable from the stones and gravel in which they are found than if they were naked.

As regards the use of the peculiar hind legs in the Anomoura and *Dorippe*, perhaps the enclosed extract from a paper read by me on December 12 before the Chester Society of Natural Science may be of interest. It will shortly be published in vol. iv. of the Transactions of the Liverpool Biological Society.

ALFRED O. WALKER.

London, W., January 17.

"An interesting fact, illustrating the ingenuity shown by more than one species of Crustacea in concealing themselves, came under my notice last summer. Having dredged a number of Amphipoda, I placed them in a vessel of sea water till I could examine them. Among them I noticed what seemed to be a piece of dead weed swimming rapidly about and occasionally falling to the bottom. Examination with a lens showed that the piece of weed was carried by an Amphipod (*Atylus swammerdamii*), which grasped it by the two first pairs of walking legs (peræopoda). When it came to the bottom the animal concealed itself beneath the weed, which was much larger than itself.

"In connection with this habit of *A. swammerdamii*, it may be mentioned that another species, *Atylus falcatus* (Metzger), resembles the first-named minutely in every respect but one, viz. that the first peræopod has the claw (dactylus) immensely developed, while at the base of the next joint are two or three strong blunt spines or tubercles into which the point of the claw fits. This would appear to give the latter species a great advantage over its congener in grasping an object for purposes of concealment. It is a rare species, but I have met with a few specimens this summer: I am not aware of its having been recorded as British yet.

"In some of the Podophthalmata the same instinct has been observed, and especially among the Anomoura. All these have the last or hindmost pair of legs of a shrunken and apparently almost abortive form. They never appear to be used for walking, and are generally carried turned up on the back; but they are utilized by some species of curiously shaped, flat-bodied crabs (*Dorippe*) to carry the valve of a bivalve mollusk over their backs, under which they can squat and hide. From this it is an easy transition through various stages to the hermit crabs (*Paguridæ*), which ensconce themselves altogether in a univalve shell, and use the curiously abortive hind limbs to cling to the inside whorls. My friend Surgeon-Major Archer has seen crabs of the genus *Dorippe* protecting themselves (probably from the scorching tropical sun), at low tide, on the mud flats at Singapore, by carrying large leaves over their backs (Journal of Linn. Soc., vol. xx. p. 108)."

I CAN corroborate Mr. Ernest Weiss's remarks on the use of the modified legs of Dromia. A small one I had in an aquarium would, when the sponge was removed from the back, hunt about until it found something—a shell, a pebble, or even a dead fish—to replace the sponge. When there was nothing in the aquarium which it could seize, it was evidently in an unhappy condition.

With regard to foreign substances on other crabs, I have caught spider-crabs so completely covered with sponges as quite to hide their shape, and, until they moved, it was impossible to say what they were.

DAVID WILSON-BARKER.

Thought and Breathing.

WITH reference to Prof. Leumann's researches into the influence of blood circulation and breathing on mind life, referred to in NATURE of January 2 (p. 209), it is worthy of note that regulation and suppression of the breath (*Prānāyāma* or *Hatha-Vidyā*), is an all-important religious observance amongst Hindus.

It is one of the eight chief requisites of the Yoga philosophy, for attaining "complete abstraction or isolation of the soul in its own essence," and minute instructions exist for the exercise, which is adopted, apparently, as an immediate aid to deep meditation. Some of these instructions are quoted in Prof. Monier-Williams's recent work on Buddhism (p. 242), and he also quotes, in connection with this subject (p. 241), Swedenborg's opinion that thought commences and corresponds with respiration.

Swedenborg also says:—"It is strange that this correspondence between the states of the brain or mind and the lungs has not been admitted in science."

R. BARRETT POPE.

Brighton.

On the Effect of Oil on Disturbed Water.

HAVING seen the interesting article by Mr. R. Beynon on the above subject (NATURE, January 2, p. 205), shortly before leaving England, I propose to make a few observations on the theoretical aspect of the phenomena described by him.

The simplest case of wave-motion in a viscous liquid arises when two-dimensional waves are propagated in a liquid whose depth is so great in comparison with the lengths of the waves that the former may be treated as infinite. If at any particular epoch, which we may choose as the origin of the time, the form of the free surface is determined by the equation $\eta = A\epsilon^{\iota mx}$, where $2\pi/m$ is the wave-length, its form at any subsequent time may be represented by $\eta = A\epsilon^{kt+\iota mx}$, and the object of a theoretical solution is to find the value of k. The equation for determining k is given in the last chapter of my "Hydrodynamics"; and it is there shown that if the viscosity of the liquid be sufficiently small, k will be of the form $-\alpha \pm \iota\beta$, where α and β are *real positive* constants. Hence the equation of the free surface, in real quantities, may be written—

$$\eta = A\epsilon^{-\alpha t}\cos(mx - \beta t) \quad . \quad . \quad . \quad . \quad . \quad (1)$$

which represents periodic motion whose amplitude diminishes with the time, and which therefore ultimately dies away, the rapidity with which the motion decays depending upon the magnitude of α. If, however, the viscosity be large, the solution changes its character, since in this case k is a *real negative* quantity, and the equation of the free surface becomes

$$\eta = A\epsilon^{-\alpha t}\cos mx \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

which represents non-periodic motion, which rapidly dies away.

The phenomena discussed by Mr. Beynon are somewhat different from the special case of deep-sea waves, inasmuch as a thin film of a highly viscous liquid, viz. oil, whose thickness is very small compared with the wave-length, is spread over the surface of water, which is a liquid whose viscosity is so small, that it might probably be neglected altogether. The action of the wind would also introduce an additional complication; but the circumstance that the thickness of the oil is small compared with the wave-length, would, on the other hand, facilitate the calculations which would be necessary in order to obtain a theoretical solution. There can, however, I think, be little doubt that the free surface would be given by equations of the forms either of (1) or (2); where α is so large, that after a short time has elapsed after the film of oil has spread itself over the water, the amplitude of the existing motion would be small compared with that of the original motion.

A. B. BASSET.

Hôtel Beau Site, Cannes, January 11.

Luminous Clouds.

IN the correspondence that has taken place on luminous clouds, totally different classes of phenomena have been mentioned. There are *self-luminous* clouds entirely distinct from what I have termed "sky-coloured clouds," which latter, though by some deemed self-luminous, have been generally admitted to shine by reflecting the direct light of the sun.

The self-luminous clouds described by Mr. C. E. Stromeyer (p. 225) appear to have been a part of the aurora which was visible at the same time; but other correspondents have mentioned self-luminous clouds which have apparently not been of a truly auroral character, and the nature of these clouds seems not to be understood, and requires investigation; there may be various kinds of these and causes of their luminosity. I have myself not unfrequently seen what I call *irregular auroras*, which may be one form of what others call self-luminous clouds. They consist of bands which, unlike regular auroras, appear indifferently in all parts of the sky, and lie in any direction; they are usually much fainter than the Milky Way, and are always feebler near the zenith than near the horizon. The bands composing them are generally parallel, or nearly so, and 3° to 10° wide. They differ from ordinary cirrus in being, so far as I can judge, perfectly transparent, and also in moving extremely slowly, giving one the impression that they are much higher up in the atmosphere than cirrus. Their spectrum is

continuous, though they are sometimes as bright as true magnetic auroras which show the citron line.

The average number of nights on which I have seen these irregular auroras in the past 28 years, chiefly at Sunderland, is 1·9 per annum; and, if doubtful cases are included, 2·7. They agree with magnetic auroras in so far as they show some tendency to an eleven-year periodicity, being most frequent about 2 years after the sun-spot maximum, and least so about 5 years later.

T. W. BACKHOUSE.

Sunderland, January 15.

MR. STROMEYER'S letter in NATURE of the 9th inst. (p. 225) reminds me of a magnificent display that I once saw of luminous white clouds, transparent to the stars, which shone brightly through them. These clouds were extended like ribbons from north to south across the sky, in a way not uncommon with true clouds. I thought, and still think, that they were an aurora. May not those described by Mr. Stromeyer have been the same?

Belfast, January 15. JOSEPH JOHN MURPHY.

The Meteorite of Mighei.

WITH reference to the interesting meteorite of Mighei, examined by M. Stanislas Meunier, I have not observed, in any of the notices I have seen, any statement as to whether the organic matter exhibited any traces of an organized structure. I would suggest that, if it has not already been done, it should be carefully examined to see if the carbonaceous matter shows any such traces.

J. RUTHERFORD HILL.

January 11.

Achlya.

I SHALL be very grateful to any of your readers who can send me specimens of *Achlya* with the sexual reproduction, which I cannot at present obtain in my cultures. The culture should be dropped bodily into a cold saturated solution of corrosive sublimate, in a wide-mouthed corked bottle, and this filled up with the liquid to the cork before posting.

MARCUS M. HARTOG.

5 Roseneath Villas, Cork, January 6.

The Parallelogram of Forces.

WHAT is the force of the word "rigid," introduced into the statement and proof of the parallelogram of forces and other theorems in Statics, as quoted by Mr. W. E. Johnson from the ordinary text-books?

The word "rigid" requires definition; it describes a state of things which is not met with in Nature; and it is redundant and limiting; because the conditions of equilibrium of a body are the same, whether elastic to an appreciable extent, or to such an inappreciable extent that the word "rigid" may be applied to it.

Better omit the word "rigid" altogether.

A. G. GREENHILL.

Foot-Pounds.

IN the statics and dynamics paper set in the last Woolwich entrance examination, candidates are asked to determine the magnitude of a moment of a force in *foot-pounds*. Surely it is unfortunate to introduce this term in such a sense. One foot-pound is a unit of work, and though its dimensions (ML^2T^{-2}) are the same as that of a unit of a moment of a force, the two conceptions are perfectly distinct, and the introduction of a foot-pound as a unit of a moment of a force is likely to cause confusion, especially in the minds of beginners.

A. S. E.

Chiff-Chaff singing in September.

IN a review of certain recent ornithological works, in one of your latest issues, doubt seems to be thrown on the fact of the chiff-chaff singing late in September.

I believe it is not an unusual occurrence. It always nests in my garden, and this year, as I see by a note made at the time, it sang on the 20th, 21st, and 22nd of that month. We had a slight frost on the 21st.

F. M. BURTON.

Highfield, Gainsborough, January 6.

EAST AFRICA AND ITS BIG GAME.[1]

FOR sporting purposes Cape Colony and the adjoining districts are long ago "used up," and the hunter who would fain see "big game" must follow Mr. Selous into Matabelé-Land and Mashoona-Land, if he does not find it better to cross the Zambesi. Even here, some of the largest animals are already exterminated. The redoubted hunter whose name we have just mentioned has not met with a White Rhinoceros during the past four seasons, and his "bag" of ivory shows a yearly diminution. So much for the south of the Dark Continent. The northern entrance to the great Interior, which afforded Sir Samuel Baker and those who followed him such splendid sport on the Atbara and Settite, has been closed up by the Mahdists, and until we have made up our minds to "clear out Khartoum," no European can hope to penetrate in this direction. There remains, therefore, only the eastern coast as a mode of access to the wild interior of game-tenanted Æthiopia, the west coast being practically closed by swamps and fevers.

On the eastern coast of Africa, however, immediately under the equator, a splendid stretch of high-lying land, full of big game, and easy of access, is still open to the enterprising sportsman. First made known to us by the German missionaries Rebmann and Krapf, the "Kilimanjaro District," as it is now usually called, was subsequently opened to us by Von der Decken, New, and Hildebrandt. To these explorers succeeded Mr. Joseph Thomson on his route to Masai-Land, and Mr. H. H. Johnston on his expedition up the Kilimanjaro Mountain, to which Dr. Hans Meyer and other more recent travellers have also devoted their special attention. Access to this sportsman's paradise is rendered easy by the port of Mombas, now under the benign sway of the British Imperial East African Company, and connected with Aden by a regular line of steamboats. Here, in the autumn of 1886, having made the necessary preparations at Zanzibar, the author of the present volume, with his brother sportsmen Sir Robert Harvey and Mr. H. C. V. Hunter, assembled their caravan. Their plan was to reach as quickly as possible the forest of Taveta, distant about 250 miles from the coast and within ten miles of the base of Kilimanjaro, and having established their head-quarters in this favoured spot, to work thence the surrounding plains and open country. Mr. C. B. Harvey, the brother of Sir Robert, was to join them when his leave commenced, a month later.

How well this programme was carried out the entertaining pages of Sir John Willoughby's narrative fully explain to us, while the map at the commencement clearly shows the route and the nature of the different districts traversed, as they appeared to the eyes of the enthusiastic sportsmen. Much time and trouble was saved to the expedition by the selection of a Maltese named Martin as "chief of the staff." Martin had accompanied Mr. Thomson during his adventurous journey into Masai-Land, and was, moreover, the owner of a "freehold building-site" at Taveta. Hereon was a house and a range of huts, forming three sides of a large square, while the fourth was bounded by a sparkling rivulet well stocked with fish. Such a haven of refuge, protected, as it was, by a thorn-hedge with a strong gateway, and situated in the immediate vicinity of a good game-country at an elevation of 2400 feet above the sea-level, seemed little less than a Paradise to our travellers, who arrived here on December 26, about sixteen days after leaving Mombas. Into their various excursions from this convenient centre we need not closely follow them. Suffice it to say that their routes were

[1] "East Africa and its Big Game, the Narrative of a Sporting Trip from Zanzibar to the Borders of the Masai." By Captain Sir John C. Willoughby, Bart., Royal Horse Guards. With Postscript by Sir Robert G. Harvey, Bart. Illustrated by G. D. Giles and Mrs. Gordon Blake; those of the latter from photographs taken by the Author. (London: Longmans, 1889.)

mostly to the west of Taveta, amongst the numerous streams that drain the southern slopes of Kilimanjaro and unite to form the Ruvu River, which enters the sea at Pangani, and to the east of the great mountain on the head waters of the Tzavo. These various hunting expeditions occupied the time until April 21, when a safe return was effected to Mombas, and thence to Europe.

The list of larger game-animals killed by the party during their four months shows a goodly total of 330 head, although we are assured by Sir John Willoughby that no useless slaughter was perpetrated during the expedition, and that no animal was killed unless required for a specimen, or for food by the hunters and their native companions. In the list of these 330 animals, we find 21 Buffaloes, 66 Rhinoceroses, 2 Elephants, 4 Hippopotamuses, 4 Zebras, and 211 Antelopes of different species. But a much more attractive list to the naturalist will be found in the appendix "on the fauna of the plains round Kilimanjaro," compiled by Mr. Hunter. So little is yet known of the larger mammals of this fine country, except from fragmentary notices, that Mr. Hunter's notes, brief as they are, form a not unimportant contribution to zoological science.

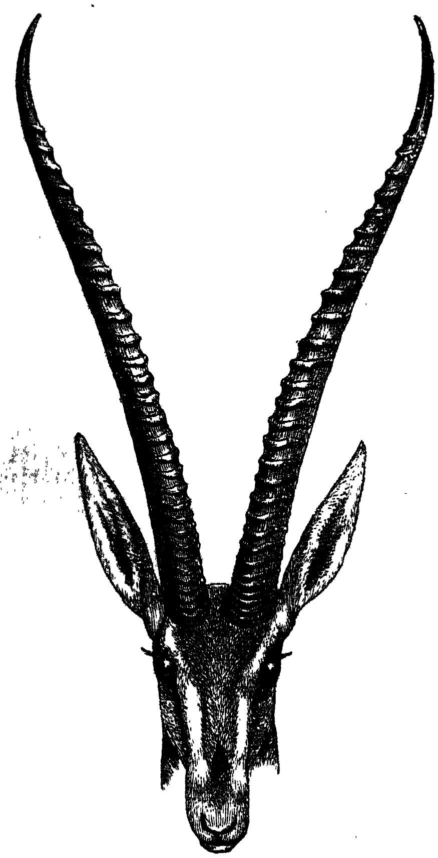


FIG. 1.—Head of Grant's Gazelle.

Lions, Elephants, Hippopotamuses, and Giraffes are prevalent alike in every part of Wild Africa, but the splendid Bovine animals called Antelopes vary very materially in the different districts. In the Kilimanjaro country, sixteen Antelopes are recorded as having been met with, and amongst them are some of the finest and largest of the whole group. The Eland (*Oreas canna*) is "rather local," but there "are a fair number to the south of the mountain." The Eland found here belongs to the variety called Livingstone's Eland, first met with by that great explorer on the Zambesi. "Both males and females are all more or less striped." The Larger Kudu (*Strepsiceros kudu*) was "only seen on two or three occasions on the Useri River"; the Lesser Kudu (*S. imberbis*) is found "in the bush around Taveta," and in several other localities. Two examples of this until lately little-known Antelope from this district are now living in the Zoological Society's Gardens. The Beisa (*Oryx beisa*) is "plentiful on the plains and in thin thorny bush"; the Coke's Hartebeest (*Alcelaphus cokii*) is "quite the most common Antelope on the plains, being found everywhere in immense herds"; while the Brindled Gnu (*Connochœtes gnu*), the Mpallah (*Æpyceros melampus*), and the Waterbuck (*Cobus ellipsiprymnus*) are, according to Mr. Hunter, abundant in appropriate localities. We suspect, however, that Mr. Hunter's so-called "Waterbuck" is the Sing-sing (*Cobus sing-sing*), of which some fine heads were procured by Mr. Holmwood, lately H.B.M. Consul at Zanzibar, during an excursion to the Tavita district. Of the beautiful tribe of Gazelles, three well-marked species, all recently discovered and appropriately named after distinguished African travellers, tenant the plains of Kilimanjaro. These are the Grant's Gazelle (*Gazella granti*), the Thomson's Gazelle (*G. thomsoni*), and the Waller's Gazelle (*G. walleri*). Grant's Gazelle is "common everywhere on the open plains." Its fine lyre-shaped horns attain a larger development than in perhaps any other species of the genus. Their elegant shape and prominent outlines will be seen by the accompanying figure from the Proceedings of the Zoological Society.

FIG. 2.—Head of Hunter's Antelope.

Thomson's Gazelle was found in large numbers in the plains of the Masai country to the south-west of the mountain. Waller's Gazelle was "very rare near Kilimanjaro," but subsequently found to be numerous up the Tana River. One was killed near Lake Jipé. But the great prize among the Antelopes was met with by Sir Robert Harvey and his companions Messrs. Greenfield and Hunter, during a subsequent expedition to Eastern Africa. In the course of this journey they ascended the River Tana, which forms the northern boundary of the dominions of the British Imperial East African Company. Here, on the northern bank, they came across specimens of an entirely new Antelope, "of a bright red colour, in some respects resembling a Hartebeest, especially in regard to the length of its head, and of about the same size, but hardly so high at the withers." This

Antelope has been since named Hunter's Antelope (*Damalis hunteri*) by Mr. Sclater (see Proc. Zool. Soc., 1879, p. 372, Pl. xlii.), and mounted specimens of it may be seen in the Mammal Gallery of the Natural History Museum at South Kensington.

It must not, however, be supposed that the rich mammal-fauna of the Kilimanjaro district has been yet entirely exhausted. We read, in Sir John Willoughby's narrative, of a Duiker Antelope (*Cephalophus*), of a dark red colour, found on the mountain, of which a specimen was obtained by an American traveller, Dr. Abbott, but not by the British sportsmen. On the same mountain, at an elevation of about 9000 feet, Dr. Abbott also secured an example of an "extraordinary animal" like a Serow (*i.e. Capricornis bubalina* of the Himalayas), but "darker in colour and shorter on the legs." There is therefore ample room for future discoveries, both in this and in other branches of natural history. The plateau surrounding Mount Kenia, which has yet to be explored scientifically, would doubtless supply many other novelties. In short, at the present time we know of no other field for zoological discovery so promising and so easily accessible as the slice of Eastern Africa recently assigned to Sir William Mackinnon and his associates of the B.I.E.A. Company, to which the author of the present volume has given us such a useful and agreeable introduction.

THE CORAL REEFS OF THE JAVA SEA AND ITS VICINITY.[1]

SINCE comparatively few of the naturalists who have sojourned in the Indian Archipelago have paid special attention to the coral reefs of that region, it becomes a cause of satisfaction that Dr. C. Ph. Sluiter, of Batavia, who has long been engaged in studying the marine fauna of his neighbourhood, has taken up the subject in earnest. In a paper on the origin of the coral reefs of the Java Sea, and of Brandewijns Bay on the west coast of Sumatra, and on the new coral formations of Krakatão, Dr. Sluiter gives the results of his recent preliminary investigations.[2] This paper is excellent in method, and the results of the highest importance.

In the western half of Batavia Bay, where the depth varies from 5–12 fathoms, there are numerous coral reefs which occur in all stages of growth from the incipient reef to the coral island begirt with a barrier-reef. Being curious to learn how the corals first began to grow on the muddy bottom of this bay, the author of this paper soon found an explanation in the fact that the stones and fragments of sunken Krakatão pumice, which lay in places on the mud, were covered with living corals. Hence he concluded that in those favourable circumstances where several of the stones and pumice fragments lay close together we might have the beginning of a reef. A singular feature in the growth of these reefs then attracted his attention. Some fourteen years ago, an artesian boring was made in the small coral island of Onrust in Batavia Bay, when an accumulation, 20 metres thick, of coral *débris*, shells, and clay, was found to pass downward into a firmer clay. The depth of the sea around is only 11 metres, and after allowing about 2 metres for the height of the island, Dr. Sluiter infers that the coral fragments have sunk down 7 metres into the mud or clay of the sea-bottom.

To support this view, the author gives a section of the shore-reef of Brandewijns Bay, on the west coast of Sumatra, the section being constructed from data supplied by fifteen borings, none deeper than 24 metres, the reef there being rather under 300 metres wide. As is there shown, the volcanic formations of the steep coast-border descend at a precipitous angle under the sea, so that they do not form a foundation for the shore-reef. This reef, the thickness of which varies greatly, being in some places as much as 11 metres and in others only half that amount, lies on "a substratum of clay or mud mixed with coral *débris*, and forming a bed ranging from 2 to 7 metres in thickness." This substratum of clay and coral passes down into a clay or mud, formed from the decomposed andesitic rocks of the district, which may be firm in some places and soft in others. The next point brought out in the section is that the substratum of clay and coral *débris* is thickest and deepest where the underlying clay is soft, and thinnest and nearest to the surface when the clay is firm or is mixed with sand. From these and allied considerations, Dr. Sluiter passes on to the conclusion that the same process has taken place here which occurs in the construction of dams and piers on a yielding bottom, a large amount of coral materials having been sunk in the mud, whilst the reef, by its own weight, has prepared its own foundation.

Having been familiar with the appearance of Krakatão before the great eruption of 1883, Dr. Sluiter observed some interesting changes in connection with the shore-reefs of this island when he revisited it in 1888 and 1889. The pumice and ashes at the time of the outbreak, according to the account of Dr. Verbeek, the historian of the eruption, destroyed all life in the sea around, making the sea-bottom a lifeless waste; and under an accumulation, 20 metres thick, of these materials lies the old shore-reef. In 1888 and 1889 the old condition of things was beginning to re-assert itself. In one place a young shore-reef, composed mostly of madrepores, had attained a breadth of a metre, and living corals were brought up in abundance by the dredge, attached to sunken pumice. Amongst the measurements of coral growth given by the author are those relating to specimens of *Madrepora nobilis*, Dana, which had attained a length of from 2 to 2½ decimetres in a period that could not have exceeded five or six years, and was probably much less. Specimens of *Porites mucronata*, Dana, had also in the same period grown to a length of 1 decimetre.

After referring briefly to the interesting Thousand Islands, a linear group of small coral islands near Batavia, many of which, in the southern part, affect the atoll form, Dr. Sluiter sums up the results of his observations. A coral reef in the Java Sea *commences its growth on a muddy bottom* in the form of a colony of corals growing on the *stones* and *sunken pumice* that there lie. As it increases in extent and height, it secures its own foundation by its weight, a large amount of coral materials sinking into the mud to a depth of seven or less metres. In its upward growth it presents a level top, and displays no hollow or basin, a uniformity which it preserves until a foot from the surface, when it dies in the centre, and the agencies dwelt upon by Murray and Agassiz then co-operate in forming an atoll or a barrier-reef. Because the small coral reefs (500 metres wide) of the Java Sea grow up uniformly until near the surface, Dr. Sluiter places himself in partial antagonism to a portion of Murray's theory. In this, however, he has missed the point of the new view, according to which such small reefs would either have no lagoon or else possess a very shallow one. With this correction, his partial confirmation of Murray's theory becomes more complete.

We hope that, with the great facilities at his disposal, Dr. Sluiter will make an exhaustive examination of the Thousand Islands, the varied and unusual conditions of their growth rendering them particularly important as a field for thoroughly investigating the problem.

H. B. GUPPY.

[1] "Einiges über die Entstehung der Korallenriffe in der Javasee und Branntweinsbai, und über neue Korallenbildung bei Krakatau." Von Dr. C. Ph. Sluiter. (Batavia en Noordwijk: Ernst and Co., 1889.)

[2] *Natuurkundig Tijdschrift voor Nederlandsch Indië*, Band xlix.

THE ELECTRIC LIGHT AT THE BRITISH MUSEUM.

THE authorities of the British Museum are to be congratulated on the thorough and admirable manner in which the scheme for the electric lighting of the galleries has been carried out. Everyone present at the private view on Tuesday evening was pleased with the results which had been achieved. Both arc and glow lamps are employed; the former in the galleries on the ground floor containing Greek and Roman sculpture, the Elgin marbles, and Assyrian and other antiquities, and in some galleries on the upper floor. The suite of bronze and vase rooms on the west, and the ethnographical gallery on the east, of the upper floor are lighted by glow lamps. The light from glow lamps is more agreeable to the eyes than the more powerful light of arc illuminants; but these have been regulated with the utmost care, and on Tuesday evening there was a very general feeling that the beauties of the sculpture were brought out by them more effectually than by such daylight as is at times rendered possible by our northern climate. With regard to the arc lights on the upper floor, it was noticed that they were admirably adapted for the exhibition of the Japanese drawings, even the minutest details of these delicately finished works being rendered plainly visible without any diminution of the beauty of the colours.

We quote from the *Times* of January 29 the following description:—

"In the galleries on the ground floor there are 69 arc lamps of various powers, while on the upper floor there are 57 arc and 627 glow lamps. In addition to these there are five large arc lamps in the reading-room, six in the court-yard, and upwards of 200 glow lamps in the offices and passages. The total current required to work the whole of the lamps is nearly 1200 amperes, with an E.M.F. of 115 volts at the lamp terminals; and this output is produced by the expenditure of nearly 200 brake-horse-power. The current is generated by four Siemens dynamo machines, each capable of giving an output of 450 amperes and 130 volts, which are connected to a general switchboard in the engine-room by means of which they can be put to work in parallel to any or all of the circuits. The switchboard is fitted with instruments indicating the current given off by each dynamo and four circuits are led from it round the Museum—two for the upper and two for the lower floor. The main wires are laid outside the building. In order to insure safety, and to guard, as far as possible, against failure of light, the motive power is in duplicate. The four dynamos are driven in pairs, each pair by a separate engine with a separate countershaft. Each engine has a separate steampipe in direct communication with the boilers, and there is an ample reserve of boiler power. The power of the engines and dynamos is so adjusted that each of the two sets is capable of working the whole of the lamps in those galleries proposed to be lighted on any one evening; the other set standing by ready to work. Further, in order to work if required, at half-power, or in order to provide half-light for the whole of the galleries—which light should suffice for an emergency such as sudden fog or accident—the lamps are connected in pairs alternately, so that half of the number being cut off, the light of the other half still remains evenly distributed. The engines have been supplied and erected by Messrs. Marshall, Sons, and Co. (Limited), of Gainsborough, and the electrical work has been executed by Messrs. Siemens Brothers and Co. (Limited)."

The eastern and western portions of the Museum will be open to the public on alternate week-day evenings, and all the world agrees with the *Times* that "the educational value of the unique collections of art and scientific treasures the Museum contains will be greatly enhanced by the change."

NOTES.

THE Medals and Funds to be given at the anniversary meeting of the Geological Society on February 21 have been awarded by the Council as follows: the Wollaston Medal to Prof. William Crawford Williamson, F.R.S.; the Murchison Medal to Prof. Edward Hull, F.R.S.; and the Lyell Medal to Prof. Thomas Rupert Jones, F.R.S.; the balance of the Wollaston Fund to Mr. W. E. A. Ussher, of the Geological Survey of England; that of the Murchison Fund to Mr. Edward Wethered; that of the Lyell Fund to Mr. C. Davies Sherborn; and a portion of the Barlow-Jameson Fund to Mr. William Jerome Harrison.

THE Council of the Royal Meteorological Society have arranged to hold at 25 Great George Street, Westminster, on March 18 to 21 next, an Exhibition of Instruments and Photographs illustrating the application of photography to meteorology. The Exhibition Committee invite co-operation, as they are anxious to obtain as large a collection as possible. They will also be glad to show any new meteorological instruments or apparatus invented, or first constructed, since last March; as well as photographs and drawings possessing meteorological interest. Anyone willing to co-operate in the proposed Exhibition should furnish the assistant secretary (not later than February 12) with a list of the articles he will be able to contribute and an estimate of the space they will require.

THE second course of the Gifford Lectures at Glasgow will begin on February 5. As announced in the first course, these lectures will treat of what Prof. Max Müller calls "Physical Religion," or the belief in natural, sub-natural, and super-natural powers, discovered in some of the great phenomena of Nature.

SOME most interesting notes on the last days of Father Perry are contributed to the *Tablet* of January 25 by Father Strickland, S.J. The facts stated by the writer bring out in a very striking light the earnest and resolute spirit in which, to the end of his life, Father Perry devoted himself to science. During the voyage he suffered badly from sea-sickness, and on his arrival at the Isles de Salût he was "much done up." Nevertheless, he allowed himself no rest, but landed at once to view the site and introduce himself to the authorities. Captain Atkinson urged him to live on board the *Comus* and land each morning for his work; and Father Strickland is of opinion that if he had done this "his life would not have been sacrificed to his over-anxious desire to do everything for the best for the success of the work confided to him." He preferred, however, to take up his quarters in the hospital, and said nothing about the fact that he was in bad health, making "light of all his personal wants for fear of giving trouble to others." The observatory erected for the occasion was half a mile from the hospital, and "the intervening ground was very rough, being a steep descent and ascent, and the distance was gone over on foot four times each day in fair weather or foul." "On the Friday before the eclipse Father Perry complained of being 'very bad inside,' but he worked on until nearly 3 a.m., and when the men retired to the *Comus* he tried to snatch a little rest where he was, and lay down in a hammock in the tent. He was up again before 6 o'clock to take the position of the sun at rising. At 6.45 the men arrived from the ship, and at 7.30 there was a complete, most careful, and most successful rehearsal of all the operations and duties which were to be performed next morning in the solemn moments of the eclipse, for which they had been preparing so long and had travelled so far. Everyone was surprised at Father Perry's exactitude in contributing to carry out his own orders and his courage in facing fatigue. His readiness to sacrifice himself and his own convenience in order to save trouble to others endeared him to all who worked with him, and challenged their utmost efforts to secure success for their work in

spite of the oppressive climate and surroundings. Just before noon on Saturday, Lieutenant Thierns went to see him at the hospital and found him much exhausted; but he was again at his post in the observatory at 3 p.m., at which time an important photograph was secured with the mirror. In the evening he went on board the *Comus* for dinner, but was only able to lie on a sofa all the time; and he sent to the doctor for some chlorodyne. Much against the wishes and earnest advice of Captain Atkinson (who spoke to me of Father Perry with the sincerest regard and esteem), Father Perry made his way on shore in a violent pouring rain to sleep in his own quarters, and would allow no one to hinder him. Next morning, Sunday the 22nd, was the important moment of the eclipse. Lieutenant Thierns landed with his observatory party at six o'clock, and on arrival was informed by Mr. Rooney that Father Perry had passed a very bad night and was very ill, so a man was sent to help him over the bad half mile from his quarters, as he declined to let himself be carried on a stretcher. He reached the observatory in good time, though in a very exhausted state. As the important moment approached, he seemed to rally, and, during the minutes of the eclipse, seemed to be himself again, and showed no signs of illness or exhaustion. There were two photographic instruments in use—one an old one, which had often been in use before, the other was the special new corona graphic instrument prepared for the occasion, of which Father Perry himself took charge. He was so alert and self-possessed during the eclipse, that his friends about him hoped he was not so ill, but he gave way immediately after, and with much difficulty reached his quarters in the hospital. It was known after, that during the previous night he had been very seriously ill." On Sunday night it became evident that he was suffering from the very worst form of dysentery. On Wednesday, Christmas Day, he was better, and the vessel started for Demerara. All hope was gone on Friday at 1.30 p.m. At 3 p.m. his mind began to wander, and at 4.20 he died. It is pathetic to read that before he quite lost consciousness he thought himself again engaged in "the supreme moment of the scientific mission which had so long filled his thoughts," and "began to give his orders as during the short minutes of the eclipse."

At its annual sitting, the Russian Academy of Sciences elected the following as Corresponding Members:—In Mathematics, Prof. Sophie Kovalevskaya, Stockholm; in Astronomy, Prof. Moris Lœwy, Paris; in Chemistry, Prof. Stanislas Cannizaro, Rome; in Biology, Th. Keppen, Russia, and Prof. Henri Baillon, Paris.

The Sanitary Institute has made arrangements for the ninth course of lectures and demonstrations for sanitary officers. They will be given in the Parkes Museum, and will be specially adapted for candidates preparing for the Institute's examination for inspectors of nuisances. The introductory lecture will be delivered on February 18 by Mr. E. C. Robins. Among the lecturers will be Sir Douglas Galton and Prof. W. H. Corfield. The former will lecture on ventilation, measurement of cubic space, &c; the latter on sanitary appliances.

Messrs. Macmillan and Co. are issuing a monograph of the British Cicadæ, by George Bowdler Buckton, F.R.S. It will consist of eight quarterly parts, each containing on an average ten litho-chromo plates and letterpress, illustrating the forms, metamorphoses, general anatomy, and the chief details connected with the life-history of this family of insects. The work will contain also short diagnoses of all the British species, about 230 in number, most of which have come under the author's notice, each species being illustrated by one or more coloured drawings. Some account will be given of the curious myths and tales told by ancient Greek and Latin poets, and descriptions will be appended relating to the curious sound-organs possessed by some species, and other subjects connected with the economy of this interesting but difficult group of Rhynchotous insects.

Messrs. Macmillan and Co. have in the press a "Manual of Public Health," by Mr. Wynter Blyth, M.R.C.S., Medical Officer of Health for St. Marylebone.

Malta has suffered a great loss in the almost sudden death of Dr. Gulia, Professor of Botany, Hygiene, and Forensic Medicine in the Royal University of Valletta. He was born, in 1835, at Cospicua, a suburb of Valletta, where his father was a physician. He graduated in medicine and surgery, in 1855, at Valletta, and afterwards went to complete his studies at Paris, where he made the acquaintance of a large number of eminent men, including Milne-Edwards, Blanchard, and Vidal. On his return to reside in his native town, he was elected to the above-mentioned Chair in the University in Valletta. Besides attending to his professorial duties and the requirements of a large medical practice, Prof. Gulia found time to edit an important medical journal, in which he exhibited great literary and scientific talents. He also issued, among other writings, a "Flora of Malta." His son is about to publish his last work, containing the completest account of the flora of Malta up to the present time, bringing the total number of species up to 833.

At a meeting of the Society of Arts, last week, Mr. Brudenell Carter read a valuable paper on "Vision-testing for Practical Purposes." Referring to colour blindness, Mr. Carter said that Dr. Joy Jeffries, in the last edition of his work on the subject, tabulates the results of the examination of 175,127 persons, and shows that the percentage of this number who were colour blind amounted to 3·95. Any method of examination which gives a percentage differing from this in any marked degree must, Mr. Carter thinks, be vitiated by some error. Of the methods of examination pursued on the English and Scottish lines of railway, and by the Board of Trade, he said they had one feature in common—they were all wrong, "the direct offspring, in almost every instance, of a degree of ignorance and presumption, the very existence of which would be incredible if the proofs of it were not brought daily under our observation." "Even where the use of Holmgren's method is professed," said Mr. Carter, "the rules laid down by Holmgren for conducting it are, as a rule, utterly ignored, and the results obtained are as utterly misleading. The test should be used in exact conformity with his very detailed and precise instructions, or it should not be used at all."

The first of a series of Friday evening lectures on Astronomy was delivered on Friday, the 24th instant, by Mr. E. J. C. Morton, at the Battersea Public Baths. An audience numbering over 400 assembled, and manifested much interest in the subject with which Mr. Morton dealt. The lectures are being given in connection with the University Extension Scheme.

The following science lectures will be given at the Royal Victoria Hall during February: 4th, "Algeria and Morocco," by Mr. Henry Blackburn; 11th, "Arsenic," by Mr. Ward Coldridge; 18th, "Eyesight and Some of its Defects," by Dr. Collins; 25th, "Sinai and Palestine," by Sir Charles Wilson.

The third series of lectures given by the Sunday Lecture Society will begin on Sunday afternoon, February 2, in St. George's Hall, Langham Place, at 4 p.m., when Dr. B. W. Richardson, F.R.S., will lecture on "The Health of the Mind; and Mental Contagions." Lectures will subsequently be given by Sir Henry E. Roscoe, M.P., F.R.S., Mr. Justin H. McCarthy, M.P., Mr. G. Wotherspoon, Mr. H. L. Brækstad, Mr. Louis Fagan, and Dr. James Edmunds.

Great efforts are being made to secure that the eleventh meeting of the National Electric Light Association, to be held

at Kansas City from February 11 to 14, shall be, as *Science* puts it, "one of the most interesting conventions ever held." Those who propose to go to Kansas from New York may look forward to a pleasant journey. A vestibule train, to be called the Electric Limited, is to run through without change to Kansas City *viâ* Chicago and the Chicago, Burlington, and Quincy Railroad. The committee making the necessary arrangements feels confident that this train will be "the finest ever run out of New York." It will be composed of the latest Pullman vestibule sleeping-cars, lighted by electricity, a dining-car, composite car containing barber shop, bath room, card room, library, writing desk, smoking room, &c., and an observation car with a large open room luxuriously furnished, as well as an observation platform. The train will be supplied throughout with fixed and portable electric lamps.

HERR TRAUTWEILER thinks that a railway should go to the top of the Jungfrau, and in the *Schweizerische Bauzeitung* gives a brief account of his scheme. The railway would go from the valley below to the summit, and would be almost entirely underground. There would be several intermediate stations, from which convenient, well-arranged tunnels would lead to points on the mountain whence the best views are to be had. If stormy weather came on, the passengers could withdraw into the shelter of those tunnels. The railway would be lighted by electricity.

THE following is translated from a notice published by the authorities of the Madrid Observatory:—"D. Ernesto Caballero, Professor of Physics, and director of the electric lighting manufactory in Pontevedra, writes to this Observatory, giving details of a remarkable meteorological phenomenon which appeared at 9.15 p.m. on the 2nd inst. In a sky serene and clear, there appeared suddenly a globe or ball of fire of the apparent size of an orange, which after falling (it is not possible to well indicate how or from whence) upon the conducting wires stretched across the city, entered the manufactory (referred to) by a skylight or window, struck the apparatus for distributing the light, from which (after raising the armature of a magnetic current closer) it struck the dynamo at work. In the presence of the alarmed engineer and workmen present it rebounded twice from the dynamo to the conductor, and from the conductor to the dynamo, then fell and burst with a sharp and clear detonation into a multitude of fragments, without producing any harm or leaving any trace of its mysterious existence. In various parts of the city the lights swiftly oscillated and were extinguished for some seconds, and that the darkness was not general and long continued was owing to the admirable self-possession of the *employés*, who almost instantly established the order of things so suddenly and strangely interrupted by this mysterious meteor, of whose action and presence there only remained traces on the melted (or soldered) edges of the thick copper plates belonging to the armature of the circuit closer. Outside the building, and at the moment of falling upon the conducting wires, it was seen by (among others) the Professor of Natural History, Señor Garcéran, and from various effects observed on the wires during the following day there were undoubted manifestations (in no other way explicable) of its electrical origin."

THE second part of a voluminous bibliography of meteorology prepared by Brigadier-General Greely, Chief Signal Officer of the United States Army, and edited by Oliver L. Fassig, has been issued, and consists of a classed catalogue of printed literature relating to moisture, from the origin of printing to the close of 1881. The whole literature included is divided into 22 subdivisions, a comprehensive classification which will be highly appreciated. A section is devoted to rainfall in general, others to distribution and variation of rainfall, others to heavy rainfall and drought, and so on throughout the whole work. A division on "Showers of Miscellaneous Matter," though not properly a part of the subject, has been deemed of sufficient interest in connection with the general subject of precipitation to be included within this volume. Although supplements to Part I. Temperature, and Part II. Moisture, may appear later, it is to be regretted that it will be impracticable for any other part of this bibliography to be issued from the Signal Office.

IN *Petermann's Mitteilungen* for December last, Dr. R. Spitaler has an instructive paper on the temperature "anomalies" of the surface of the earth in January and July, with charts showing those districts which are too warm (in positive anomaly) or too cold (in negative anomaly), compared with the normal values of their geographical positions. Such charts were first drawn by Dove; but as the materials at the disposal of Dr. Spitaler are much better than those which Prof. Dove possessed, the charts differ in several important particulars. The influence of the warm and cold ocean-currents upon the temperature anomaly is very clearly shown. Europe, for instance, being under the influence of the Gulf Stream and south-west winds, is always in positive anomaly, whereas Central Asia is a district which has in winter 24° C. of negative anomaly, while in summer it has 6° of positive anomaly, or of greater heat than the same latitude in Europe. The July chart shows in the northern hemisphere two decided maxima of positive anomaly, and two minima, while in the southern hemisphere, owing to the less amount of land, the anomaly is much smaller. In July the continents of the northern hemisphere are almost entirely in positive anomaly, while the whole of the Atlantic and Pacific Oceans and Central America are in negative anomaly.

IN the current number of the Journal of the Anthropological Institute there is a valuable paper, by Dr. Arthur Thomson, on the Veddahs of Ceylon. Discussing the affinities of the Veddahs, he says there appears to be little doubt that if they be not of the same stock as the so-called aborigines of Southern India they at least present very strong points of resemblance as regards stature, proportions of limbs, cranial capacity, and form of skull. The similarities of hair and colour between these races have often been remarked, so that, on the whole, if physical features alone be taken into account, Dr. Thomson thinks the affinities of the Veddahs with the hill tribes of the Nilgherries and the natives of the Coromandel coast, and the country near Cape Comorin, are fairly well proved.

MR. H. B. BASHORE sends to *Science* sketches of an interesting Indian pipe. It is made of dark green steatite, carved into an admirable image of a turtle, and represents, no doubt, one of the Delaware totems. The back of the animal is well polished and distinctly marked with lines, and the hole for the stem is well drilled, and of a smooth bore. This relic was found thirty years ago on the site of what is now the village of Fairview, on the Susquehanna, close to an old Indian burying-ground.

THE Punjab Government is obtaining a number of young olive trees from Italy, and proposes to find out by experiment whether the low hills below Murree in the Rawul Pindi district are suitable for olive cultivation.

THE Laccadive Islands have been suffering severely from a plague of rats. According to the Calcutta Correspondent of the *Times*, these invaders have destroyed the cocoanut plantations and reduced the islanders to a state of destitution.

MR. R. M. JOHNSTON lately called the attention of the Royal Society of Tasmania to the extreme variability of the genus *Unio*, inhabiting the northern rivers of Tasmania. The shell seems to be capable of a remarkable number of modifications with regard both to form and colour. This, Mr. Johnston says, is more particularly the case if specimens marking different stages of growth are compared with each other. In specimens

marking seven stages of growth, the variation in form—from youth to the adult stage—embraces characteristics covering "most of the distinctions upon which many of the Australian forms mainly depend for the recognition of distinct specific rank." Such being the variability of local form in the individuals of the various stages of growth, Mr. Johnston thinks there is good reason for the belief that the several forms erected into specific ranks in various parts of Australia may prove to be local varieties, or particular stages of growth of one widely distributed species.

THE destruction of the native opossum is attracting some attention in Tasmania. It is said that about 75 per cent. of the animals killed have had young in the pouch at the time. The opossum has great commercial value, and there seems to be a general opinion that it ought to be efficiently protected.

IN the third report of the Liverpool Marine Biological Station on Puffin Island, Prof. W. A. Herdman gives a concise and interesting account of much good work done during the past year. In the autumn the station was closed, but it will be re-opened at the beginning of either April or May, and Prof. Herdman has no doubt that next summer all the different lines of investigation hitherto started will be followed up with a renewed enthusiasm which will more than make up for the loss of the winter observations.

THE *Annuaire de l'Académie Royale de Belgique* for the current year contains the usual information about the Academy and the awards of the various prizes. There is little to interest non-members except the series of biographies and portraits of former distinguished members, including Houzeau.

DR. C. HART MERRIAM, chief of division of ornithology and mammalogy, in the U.S. Department of Agriculture, has issued a series of directions for the measurement of small mammals and the preparation of museum skins. The directions are accompanied by an illustration, showing the appearance of a well made skin.

MR. DE ZILVA WICKREMASINGHE, assistant librarian of the Colombo Museum, has compiled a valuable list of the "Pansiyapanas Játaka," the 550 birth stories of Gautama Buddha. In order to make the record complete the compiler consulted many old manuscripts belonging to temple libraries in various parts of Ceylón. The list has been published in the Journal of the Ceylon branch of the Royal Asiatic Society, and is also printed separately.

SOLUTIONS to the questions in Pure Mathematics, Stages I. and II., set at the May examinations of the Science and Art Department from 1881 to 1886, have been published by Messrs. Chapman and Hall in book form. Each of the questions has been fully worked out, and together they make a useful series of examples in elementary mathematics.

MESSRS. DULAU AND Co. have issued a catalogue of works relating to cryptogamic botany.

WE have to acknowledge receipt of £2, sent by Mrs. Morton Sumner towards the payment of the debt on the laboratories of Bedford College, to which we called attention last week.

AN interesting paper is contributed by Prof. Carnelley to the *Philosophical Magazine* for January, in which he attempts to express the periodic law of the chemical elements by means of an algebraic formula. For reasons which are given in detail in the memoir, an expression of the form $A = c(m + \sqrt{v})$ is adopted, where A represents the atomic weight of the element, c a constant, m a member of a series in arithmetical progression, depending upon the horizontal series in the periodic table to which the element belongs, and v the maximum valency or the number of the vertical group of which the element is a member. From a number of approximations, Prof. Carnelley finds that m is best represented by the value 0 in the lithium-beryllium-boron &c., horizontal row, by $2\frac{1}{2}$ in the sodium series, 5 in the potassium series, and $8\frac{1}{2}$, 12, $15\frac{1}{2}$, 19, $22\frac{1}{2}$, &c, in the subsequent rows. Thus m is a member of an arithmetical series of which the common difference is $2\frac{1}{2}$ for the first three members, and $3\frac{1}{2}$ for all the rest. On calculating the values of the constant c from the equation $c = \frac{A}{m + \sqrt{v}}$ for 55 of the elements, the numbers are all found to lie between 6·0 and 7·2 with a mean value of 6·6. In by far the majority of cases the value is much closer to the mean 6·6 than is represented by the two extreme limits, thus in 35 cases the values lie between 6·45 and 6·75. If the number 6·6, therefore, is adopted as the value of c, and the atomic weights of the elements are then calculated from the formula $A = 6{\cdot}6(m + \sqrt{v})$, the calculated atomic weights thus obtained approximate much more closely to the experimental atomic weights than do the numbers derived from an application of the atomic heat approximation of Dulong and Petit. The number 6·6 at once strikes one as being remarkably near to the celebrated 6·4 of Dulong and Petit, and Prof. Carnelley draws the conclusion that there must be a connection between the two. This assumption appears to be supported by the following interesting facts. If we assume c to represent the atomic heat, then atomic weight = atomic heat × $(m + \sqrt{v})$ = atomic weight × specific heat × $(m + \sqrt{v})$; or specific heat = $\frac{1}{m + \sqrt{v}}$. On calculating the specific heats of the elements from this equation, they are found to agree remarkably well with the experimental values, except in those cases in which the observed specific heat is known to be abnormal. Again, Bettone has shown that the hardness of the elements is inversely proportional to their specific volumes. If this be so, hardness = $\frac{\text{specific gravity}}{6{\cdot}6(m + \sqrt{v})}$; and, on calculating the hardness from this formula, the numbers are again found to agree very closely with the hardness experimentally determined by Bettone. That the periodic law may therefore be approximately expressed by a formula of the type $A = c(m + \sqrt{v})$ appears very probable, and that the number 6·6 is a very close approximation to the value of c appears also to be established. Moreover, the fact that m in the even series represents a whole number, while in the odd series it represents a whole number and a half, corresponds to the well-known difference in chemical properties between the members of these series; and the assumption that the common difference between the first three values of m is only $2\frac{1}{2}$, while between all the rest it is $3\frac{1}{2}$, is borne out by Mendeleeff's statement that the elements of the lithium and sodium rows are more or less exceptional in their nature, and not strictly comparable with the subsequent series.

THE additions to the Zoological Society's Gardens during the past week include two Brown Capuchins (*Cebus fatuellus* ♂ ♂) from Paraguay, presented by Mr. E. Malateste; a Bonnet Monkey (*Macacus sinicus* ♀) from India, presented by Miss Alice Booth; a Macaque Monkey (*Macacus cynomolgus* ♂) from India, presented by Mr. C. Harris; a Green Monkey (*Cercopithecus callitrichus* ♂) from West Africa, presented by Quarter-Master Serjeant Mathison, W.I.R.; a Silver Pheasant (*Euplocamus nycthemerus* ♂) from China, presented by Mr. W. R Rootes; a Malbrouck Monkey (*Cercopithecus cynosurus* ♂) from Rorke's Drift, South Africa, a Bonnet Monkey (*Macacus sinicus* ♀) from India, deposited.

OUR ASTRONOMICAL COLUMN.

OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at Greenwich at 10 p.m. on January 30 = 6h. 40m. 20s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G.C. 1425	—	—	6 26 31	+10 14
(2) DM. + 17° 1479 ...	6	Yellowish-red.	6 56 1	+17 53
(3) ε Canis Minoris ...	5	Yellowish-white.	7 19 6	+ 9 30
(4) γ Geminorum ...	2	Bluish-white.	6 31 24	+16 30
(5) 78 Schj.	6	Reddish-yellow.	6 28 59	+38 31
(6) R Leonis	Var.	Very red.	9 41 39	+11 56

Remarks.

(1) This nebula is described by Sir John Herschel as "pretty large, cometic, much brighter nucleus south following." The remarks relating to the nebula G.C. 1185 (see p. 257) apply equally in this case, the spectrum not having been recorded. Next in importance to observations of the general character of the spectrum will be observations of differences between the spectrum of the nucleus and that of the "tail." It seems hardly likely that the same spectrum will be given by the dense and sparse portions of the nebula.

(2) This star has a fine spectrum of the Group II. type. Dunér states that bands 2–8 inclusive are visible, and possibly also band 9, all the bands being very wide and dark. The point chiefly requiring attention in a spectrum of this character is the presence or absence of the compound fluting of carbon which extends from about wave-length 468 to 474, it having been suggested that band 9 is simply a contrast band due to the presence of this fluting. The mean wave-length given by Dunér for the edge of band 9 is 476·0, and if the suggestion referred to be of any value, this ought to be coincident with the less refrangible edge of the carbon group. This can only be satisfactorily settled by direct comparisons of the spectrum of the star with that of carbon, obtained in the usual way from a Bunsen or spirit-lamp flame.

(3) Vogel classes this with stars of the solar type. The usual differential observations are required.

(4) A star of Group IV. (Gothard). The usual observations are required (see p. 285).

(5) This is a star of Group VI., Dunér stating that the bands 2–9 are visible. Band 6 is a little weaker than the other carbon flutings. It seems probable that some of the brighter stars of the group will give metallic line absorptions, seeing that they are most probably formed by the cooling of stars like the sun, in which there are only traces of carbon absorption, whilst the line absorption is strongly marked. If the *b* group be present, it will most likely produce an apparent displacement of the carbon fluting to a slightly less refrangible position, its absorption being added to that of carbon. This can readily be determined by comparison with the spirit-lamp flame. Other lines may also appear, but *b* is mentioned as being amongst the more prominent solar lines.

(6) Mr. Espin found bright lines in the spectrum of this variable, when near its maximum in 1889. The star will again be at a maximum on January 30, and observers will therefore have an opportunity of making a more detailed examination of the spectrum. The general spectrum is of the Group II. type. Particular attention should be given to the bright carbon flutings, both at maximum and for some time after, as it seems probable that an increase of carbon radiation will accompany the appearance of the bright lines of hydrogen. The star ranges from about magnitude 6 at maximum to 9·5 at minimum, and the period is 313 days.

A. FOWLER.

THE TOTAL ECLIPSE OF JANUARY 1, 1889.—With a summary of the observations of this eclipse, Prof. Holden has come to the conclusion that coronal forms vary periodically, those of 1867, 1878, and 1889 being of the same form; that the outer corona, terminated in branching forms, suggests the presence of streams of meteorites near the sun, whilst the extension of the corona along and very near the plane of the ecliptic would show that such streams must have long been integral parts of the solar system. The photographs taken just before second and after third contact prove the corona to be, without doubt, a solar appendage. Spectroscopic observations indicate that the true atmosphere of the sun may be comparatively shallow, it being conclusively shown that the length of a coronal line is not always an indication of the depth of the gaseous coronal atmosphere of the sun at that point.—*Observatory*, January 1890.

THE ORBITS OF THE COMPANIONS OF BROOKS' COMET (1889 V., July 6.—The four companions which accompanied this comet were notified as B, C, D, and E, respectively, by Prof. Barnard, of the Lick Observatory (*Astr. Nach.*, 2919), the principal portion being called A. Prof. Bredichin has computed, as far as possible, the orbits of the companions (*Astr. Nach.*, 2949). Taking the elements given by Mr. Chandler for the principal mass A, the following elements have been found for C and E; all are reduced to mean equinox 1889·0:—

Elements of A's Orbit.	Elements of C's Orbit.
T = 1889 October 2·1112	T = 1889 October 1·3369
$\omega = 344°\ 29'\ 20''.6$	$\omega = 344°\ 11'\ 47''.1$
$☊ = 17\ 52\ 19.0$	$☊ = 17\ 15\ 24.5$
$\iota = 6\ 5\ 6.9$	$\iota = 6\ 5\ 6.2$
$\phi = 28\ 2\ 11.6$	$\phi = 28\ 2\ 13.2$
$\log a = 0.565011$	$\log a = 0.505059$
Period = 7·0390 years.	Period = 7·0402 years.

Elements of E's Orbit.

T = 1889 October 8·7356

$\omega = 347°\ 30'\ 18''.9$
$☊ = 17\ 52\ 24.5$
$\iota = 6\ 5\ 6.2$
$\phi = 28\ 10\ 10.5$
$\log a = 0.564834$
Period = 7·0348 years.

The orbit of the mass B is situated between the orbits of A and C, and the orbit of D between those of C and E. From the inclination and position of the node it is evident that the division of the comet was effected in the plane of A's orbit, and the elements of C and E indicate almost the same point for the separation of the comet into these masses. It may be, therefore, that the separation was due to the action of Jupiter.

GREENWICH OBSERVATORY.—The Astronomer-Royal has issued the Greenwich Observations for 1887. An additional feature is the ten-year catalogue of 4059 stars, deduced from observations extending from 1877 to 1886, and reduced to the epoch 1880. The work, therefore, appears more bulky than ever.

THE PHYSICAL AND CHEMICAL CHARACTERISTICS OF METEORITES AS THROWING LIGHT UPON THEIR PAST HISTORY.

IN several articles which appeared in NATURE last year I used the term meteorite as a generic one, to include all meteoritic masses, whether they consist of the tiniest specks which give rise to the instantaneous appearance of a shooting-star in the highest reaches of our air, or of the largest masses which have so far been found after their descent to the earth's surface.

I must now confine it to those masses which have reached the earth's surface, whether large or small, and I have first to refer to the various suggestions which have been made as to their origin.

The members of the Academy of Sciences of Paris were the last to acknowledge their extra-terrestrial origin, and that long after the writings and reasonings of Chladni, to which reference has been made.

Laplace ascribed them to lunar volcanoes,[1] by others it was imagined that they came from our own volcanoes; there were those, also, who held that they came from the sun; while, again, others thought they were fragments of a broken planet.

The theory of the volcanic origin of meteorites, whether lunar or terrestrial, does not satisfactorily explain the orbital motions around the sun, for, if this were their real origin, the meteorites would travel round the earth. Neither does it explain the relations which exist between comets and meteorites, for no one supposes that comets are effects of volcanic action. Further, fragments thus ejected from the earth's surface would be consumed in the journey by the same process which is afterwards to render them visible to us as shooting-stars.

With regard to the theory of the solar origin of meteorites, it is difficult to understand how solid bodies can come from the sun after passing through an immense thickness of the intensely heated solar atmosphere. Then, again, particles shot out from the sun would not travel in an orbit, as the meteorites do, but would simply move outwards in a straight line, and then fall back.

That the meteorites are fragments of a broken planet is supported by a considerable number of facts, but the main difficulty

[1] "Les Météorites," Meunier, p. 112.

is to establish the connection between comets and meteorites, as even the supporters of the theory do not claim that the comets are parts of a broken planet. Then, again, it is only an assumption that such a planet ever existed, and it is difficult to understand how a broken planet should so far disobey the law of gravity as to divide itself into small scattered fragments.

The real parentage of those meteorites which fall on our earth is, therefore, probably cometic, for the association of comets, meteorites, and shooting-stars can no longer be denied, and it is an observed fact that comets do break up.

The discovery of Schiaparelli (1866), and his view that the head of a comet was the largest meteorite in a swarm, of course, put these origins of some meteorites, at all events, out of the question.

Reichenbach (1858) did not consider that the head of a comet was a large meteorite, but a swarm of small ones, and the large meteorites he considered to be built up in some way out of the smaller ones brought into our system by comets. If this view be subsequently confirmed, since we now know that, as suggested by Schiaparelli, comets are nebulous shreds, brought into our system by solar or planetary attraction, it follows that in the nebulæ also we may be only dealing with excessively small masses.

If meteorites, in the restricted sense of the term above referred to, do not exist sporadically in external space, they must be manufactured in our system, and two tests should be open to us: (1) no meteorites should reach us from outer space; and (2) they should bear traces of the process by which they have been built up from cometary materials.

If we can establish this, then we imagine a gradual progression in the size of meteoritic masses from regions where they are so small that luminous collisions are all but impossible, to those regions nearest to a cooling sun, like our own, where there has been the richest supply of cometary material, furnished at successive perihelion passages for the longest time.

With regard to (1), we have the facts that it is only very rarely meteorites fall in displays of shooting-stars, and that when the earth has passed near a comet no increase in the average number of meteorites has been noticed.

The most important piece of evidence on this point, however, has been recently furnished by Prof. Newton, who, from a complete discussion of the data extant from all known falls, has come to the conclusion that all the meteorites now in our collections have come from a single ring of bodies circulating round the sun.

We next come to (2). The most important point to consider here, in the first place, is the very special structure of meteorites.

Thumb Markings.

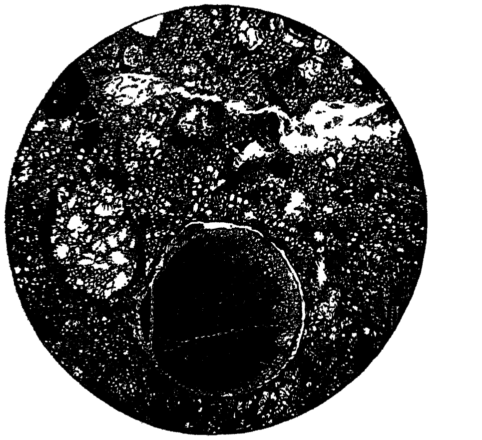
Regarding the origin of the remarkable pittings of the surfaces of aërolites and aërosiderites, an opinion was lately expressed and advocated by Daubrée,[1] that in their flight through the air they undergo erosion and excavation by joint effects of combustion and fusion, assisted mainly by air vortices attacking most violently certain portions of their surface. An important paper on this subject by Prof. Maskelyne was published immediately afterwards in the *Philosophical Magazine* (of August 1876). It is true that pittings identical in appearance with those of meteorites are found on the surfaces of certain large grains of powder blown unconsumed from the mouths of the large modern rifled ordnance (excellent specimens of this kind received from Prof. Abel and Major Noble having been shown by Prof. Maskelyne to M. Daubrée in the summer of 1875); but two important grounds for exception, in regard to this explanation, are pointed out by Prof. Maskelyne, which must not be overlooked. The closest examination of the molten glaze with which, like other parts of these surfaces, the pittings or depressions of meteorites are coated over, shows no indications of vorticose action of the air, although stream-lines of the glaze from front to rear are of frequent and conspicuous occurrence. The process of atmospheric combination, or combustion, is also rare, if not entirely absent, during the period of most intense operation of the heat, as is shown by particles of metallic iron which are occasionally found embedded in the glaze, and even by cases where the highly oxidizable mineral oldhamite (calcium sulphide), occurring in spherules in the Bustee meteorite, is glazed over equally with the augite without offering any signs of combustion or of the production of cavities where they are exposed.

[1] *Comptes rendus*, April 24, 1876. See "Report on Observations of Luminous Meteors for the year 1875–76," p. 167.

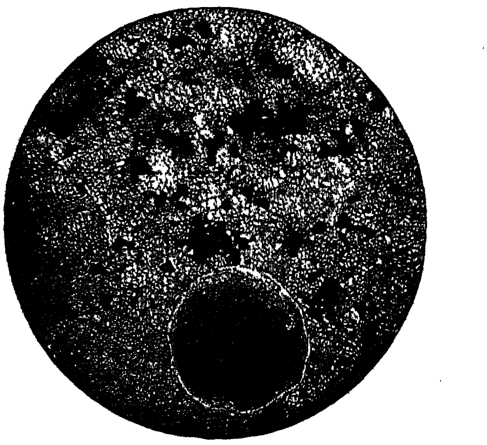
Chondritic Structure.

We have spherules of iron, like small shot of different sizes, in the stones.

These spherules, or chondroi, as they are sometimes called, vary very considerably in size; some reach the size of a cherry, while others are so small that they can only be seen by the aid of a microscope.



Chondroi in Soko-Banya meteorite (magnified 10 diameters)



Chondroi in Mocs meteorite (magnified 10 diameters)

By examining sections of chondritic stony meteorites we find that they consist sometimes almost entirely of spherules. The Parnellee aërolite affords us a very good instance of this, the most varied groups of spherules being seen collected together in one section. These spherules are sometimes encased in small shells of nickeliferous iron, or sometimes in addition with a kind of pyrites, a sulphide of iron termed troilite (FeS), peculiar to meteorites.

Some chondroi have round depressions which point to plasticity during contact, as if the spherules which form the splintered fragments had acquired their form during the act of rubbing. Others, again, have projections of a rounded form, or an almost pointed end.[1]

Our terrestrial rocks contain no structure identical with that chondritic structure so peculiar to meteorites, and the characters of the spherules are found to be quite different from those in either perlite or obsidian.

Tschermak[2] directs attention to the peculiarities observed in several chondritic meteorites. The first is the occurrence of a crust over the surface of the bronzite spherules, possessing fibrous structure. This crust is thin, and is distinguished from the inclosed material by its paler colour; it has the same fibrous

[1] Flight, "History of Meteorites," p. 207.
[2] Quoted from "Report of Observations of Luminous Meteors during the Year 1877–78," p. 107.

structure, doubly refractive power, and, in fact, is optically orientated like the inclosed silicate. It appears to be produced by some agent acting from without, perhaps heat in conjunction with a reducing gas. The agent has not caused friction, but a slight modification of the texture of the surface.

Indications afforded by Crystalline Structure.

The mixed minerals of meteorites have been subjected to microscopic examination by Sorby[1] and Rose,[2] and both have found that the crystals differ in some essential particulars from those of volcanic rocks.

Sorby long since showed that when crystals are formed by deposition from water or from a mass of melted rock, they often catch up portions of this water or melted stone which can be seen as cavities containing fluid or glass. Crystalline minerals formed by purely aqueous or by purely igneous processes can thus be distinguished. One of the most common of the minerals in meteorites is olivine, and when met with in volcanic lavas this mineral usually contains only a few and small glass-cavities in comparison with those seen in such minerals as augite. The crystals in meteorites are generally only small, and thus the difficulty of the question is considerably increased. However, by careful examination with high magnifying power, Sorby found well-marked glass-cavities, with perfectly fixed bubbles, the inclosed glass being sometimes of brown colour and having deposited crystals. On the contrary he was never able to detect any trace of fluid-cavities, with moving bubbles, and therefore he holds it very probable, if not absolutely certain, that the crystalline minerals in meteorites were chiefly formed by an igneous process, like that which has produced lava, and analogous volcanic rocks.

Passing from the structure of the individual crystals to that of the aggregate, Sorby points out that in some cases we have a structure in every respect analogous to that of erupted lavas, though even then there are very curious differences in detail.

The results of the observations of the kinds of crystallization noted in meteorites by many eminent authorities go to show that it took place hastily. Thus Brezina, after making a complete study of the Vienna collection, comes to the conclusion that the structural features of meteorites are the result of a hasty crystallization.

Again, it is the opinion of several high authorities that the crystallization did not necessarily take place under conditions of high temperature.

M. Daubrée's opinion is thus expressed :—[3]

"It is extremely remarkable that, in spite of their great tendency to a perfectly distinct crystallization, the silicate combinations which make up the meteorites are there only in the condition of very small crystals, all jumbled together as if they had not passed through fusion. If we may look about us for something analogous, we should say that, instead of calling to mind the long needles of ice which liquid water forms as it freezes, the fine-grained texture of meteorites resembles rather that of hoar frost, and that of snow, which is due, as is known, to the immediate passage of the atmospheric vapour of water into the solid state."

This possibility of the absence of high temperature is thus further insisted upon by Prof. Newton :—[4]

"The meteorites resemble the lavas and slags of the earth. These are formed in the absence of water, and with a limited supply of oxygen, and heat is present in the process. But is heat necessary? Some crystallizations do take place in the cold; some are direct changes from gaseous to solid forms. We cannot in the laboratory reproduce all the conditions of crystallization in the cold of space. We cannot easily determine whether the mere absence of oxygen will not account fully for the slag-like character of the meteoric minerals. Wherever crystallization can take place at all, if there is present silicon and magnesium and iron and nickel, with a limited supply of oxygen, their silicates ought to be expected in abundance, and the iron and nickel in their metallic forms. Except for the heat, the process should be analogous to that of the reduction of iron in the Bessemer cupola, when the limited supply of oxygen combines with the carbon, and leaves the iron free."

Should this view be subsequently confirmed, all early ideas touching the formation of meteorites will require to be modified. Thus, in 1855, Prof. Lawrence Smith stated : "They have all been subject to a more or less prolonged igneous action corresponding to that of terrestrial volcanoes." Haidinger, in 1861, not only declared for high temperature, but for high pressure.

[1] Proc. R.S., January 1864. [2] Berlin Acad. Trans.
[3] Quoted by Newton, NATURE, vol. xxxiv. p. 535. [4] NATURE, *loc. cit.*

Obviously, these views, which were based more upon the analogues of some of the meteorites with volcanic basic rocks than upon the actual character of the crystallization, suggested the formation of large masses; and the ideas that comets were solid bodies and that meteorites were fragments of comets or planets were both based upon these views,[1] and the higher the temperature required and the slower the crystallization, the larger in imagination did these possible birthplaces of the meteorites become.

If neither much time nor heat be required to produce the crystallization observed, then, with Prof. Newton, we can suppose "a mass containing silicon, magnesium, iron, nickel, a limited supply of oxygen, and small quantities of other elements, all in their primordial or nebulous state (whatever that may be), segregated somewhere in the cold of space. As the materials consolidate and crystallize, the oxygen is appropriated by the silicon and magnesium, and the iron and nickel are deposited in metallic form. Possibly the heat developed may, before it is radiated into space, modify and transform the substance. The final result is a rocky mass (or possibly several adjacent masses) which sooner or later is, no doubt, cooled down throughout to the temperature of space."

We shall see subsequently that there are many known causes in operation which will provide us with just such a mixed mass of vapours as Prof. Newton requires, and it is at once obvious that, not only is the generic separation into iron and stones thus accounted for, but the special form of crystallization observed in stones and the special chondritic structure observed both in irons and stones would all arise from the same cause.

Evidences of Heating and Action of Violent Forces at Different Times.

The peculiarities in the mineralogical structure of the meteorites are probably in part due to the successive heatings and coolings to which they were subjected with each approach of the comet to the sun, and partly, perhaps, to the heat of combination of oxygen and silicon. They were most probably formed in a limited supply of oxygen, so that the elements possessing greatest affinity for that element were the first to form compounds, leaving iron and nickel in the metallic or uncombined state.

Some meteoric stones from examination seem to have been heated to a high temperature right through their mass. Such cases as Orvinio, Chantonnay, Juvenas, and Weston show signs that fragments are cemented together with a material of the same substance as themselves. Again we have indications of chemical changes, the chondroi in some stones being found to be surrounded by spherical and concentric aggregations of minute particles of nickel, due, as is supposed, to the reducing action of hydrogen at a high temperature.

Some meteorites are merely breccias, consisting of fragments, the *débris* of pre-existing meteorites, or of the original mass tremendou-ly shattered, and subsequently cemented together.

In this connection Sorby writes :—

"It would therefore appear that, after the material of the meteorites was melted, a considerable portion was broken up into small fragments, subsequently collected together, and more or less consolidated by mechanical and chemical actions, amongst which must be classed a segregation of iron, either in the metallic state or in combination with other substances. Apparently this breaking up occurred in some cases when the melted matter had become crystalline, but in others the forms of the particles lead me to conclude that it was broken up into detached globules whilst still melted (Mezö-Madaras, Parnellee). This seems to have been the origin of some of the round grains met with in meteorites; for they occasionally still contain a considerable amount of glass, and the crystals which have been formed in it are arranged in groups, radiating from one or more points on the external surface, in such a manner as to indicate that they were developed after the fragments had acquired their present spheroidal shape (Aussun, &c.). In this they differ most characteristically from the general type of concretionary globules found in terrestrial rocks, in which they radiate from the centre; the only case that I know at all analogous being that of certain Oolitic grains in the Kelloways rock at Scarborough, which have undergone a secondary crystallization."[2]

Mr. Sorby remarks : "A most careful study of their microscopical structure leads me to conclude that their con-

[1] See Newton, NATURE, vol. xxxiv. p. 534.
[2] "Microscopical Structure of Meteorites," Proc. R.S., June 16, 1864.

stituents were originally at such a high temperature that they were in a state of vapour, like that in which many now occur in the atmosphere of the sun, as proved by the black lines in the solar spectrum." We may, in fact, look upon them as being to planets what the minute drops of water in the clouds are to an ocean. He has shown that possibly, after the condensation of the vapour, they collected into larger masses, which have been subsequently changed by metamorphic action, broken up by mutual impact, and again collected and solidified, the meteoric irons possibly being those portions of the metallic constituents which were separated from the rest by fusion when the metamorphosis was carried to the extreme point.

In this manner the subsequent heating, or any number of subsequent heatings, are explained.

Iron Meteorites not fused in falling.[1]

A question of no slight interest in regard to the changes which meteoric irons undergo during their passage through the atmosphere is whether their surface becomes fused. From his study of the Charlotte meteorite, Dr. Smith is inclined to answer it in the negative. The fact of the delicate reticulated surface having been preserved is a proof that the heat, instead of having been raised to a high temperature, has quickly been conducted away into the mass of metal. Had fusion of the superficial layer taken place, the meteorite would have been coated with molten oxide.

Veins.

Now and again we come across meteorites which have veins, like terrestrial rock-veins, running right through them. Prof. Maskelyne's description of them is as follows:—[2]

"Just as in a mine one may meet with a fissure that, once dividing the 'country,' but subsequently filled by rocky matter, cuts across the course of a mineral vein which itself was originally formed in a similar way; and just as such a cross fissure, thus intersecting with the original metalliferous vein, often gives us evidence of a heave, *i.e.* that one side of the new fissure has slid upwards or downwards along the other, so an exactly similar thing is met with in meteorites, and is admirably seen in the microscopic sections of them."

Faults and throws are both represented in meteorites. In that of Aumières there is a throw of several centimetres indicated, and faults intersect. These faults are accompanied by heat due to the friction of the surfaces, and in the case of gray stony meteorites the faults are black like the crust.[3] (The black veins are physically connected with the crust, and are supposed to have the same origin, the melted material having filled up the fissures.)

On examining such meteorites as Château-Renard, Pultusk, and Alessandria, it is found that some of the spherules even are broken in half and the halves separated from each other by a vein of meteoric iron or troilite, and in some cases by a black fused substance, like the crust of a meteorite.

The Presence of Sulphides.

The presence of sulphides, which must have been formed when both water and free oxygen were absent, shows a distinctly non-terrestrial condition, as, indeed, does also the presence of small particles of iron. On this point Dr. Flight remarks:[4] "If the conditions necessary for the formation of pure calcium sulphide be borne in mind, the evidence imported into this inquiry by the Bustee aërolite seems further to point to the presence of a reducing agent during the formation of its constituent materials."

Sorby's General Conclusions.

We have before referred to Sorby's microscopical examination of meteorites. In 1865 he stated the general conclusions he had arrived at as follows. It will be seen how remarkable the agreement is between him and Reichenbach.

"As shown in my paper in the Proceedings of the Royal Society (xiii. 333), there is good proof of the material of meteorites having been to some extent fused, and in the state of minute detached particles. I had also met with facts which seemed to show that some portions had condensed from a state of vapour; and expected that it would be requisite to adopt a modified nebular hypothesis, but hesitated until I had obtained more satisfactory evidence. The character of the constituent particles of meteorites and their general microscopical structure differ so much from what is seen in terrestrial volcanic rocks, that it appears to me extremely improbable that they were ever portions of the moon, or of a planet, which differed from a large meteorite in having been the seat of a more or less modified volcanic action. A most careful study of their microscopical structure leads me to conclude that their constituents were originally at such a high temperature that they were in a state of vapour, like that in which many now occur in the atmosphere of the sun, as proved by the black lines in the solar spectrum. On cooling, this vapour condensed into a sort of cometary cloud, formed of small crystals and minute drops of melted stony matter, which afterwards became more or less devitrified and crystalline. This cloud was in a state of great commotion, and the particles moving with great velocity were often broken by collision. After collecting together to form larger masses, heat, generated by mutual impact, or that existing in other parts of space through which they moved, gave rise to a variable amount of metamorphism. In some few cases, when the whole mass was fused, all evidence of a previous history has been obliterated; and on solidification a structure has been produced quite similar to that of terrestrial volcanic rocks. Such metamorphosed or fused masses were sometimes more or less completely broken up by violent collision, and the fragments again collected together and solidified. Whilst these changes were taking place, various metallic compounds of iron were so introduced as to indicate that they still existed in free space in the state of vapour, and condensed amongst the previously formed particles of the meteorites. At all events the relative amount of the metallic constituents appears to have increased with the lapse of time, and they often crystallized under conditions differing entirely from those which occurred when mixed metallic and stony materials were metamorphosed, or solidified from a state of igneous fusion in such small masses that the force of gravitation was too weak to separate the constituents, although they differ so much in specific gravity. (Report of British Association, 1864.) Possibly, however, some meteoric irons have been produced in this manner by the occurrence of such a separation. The hydro carbons with which some few meteorites are impregnated may have condensed from a state of vapour at a relatively late period.

"I therefore conclude provisionally that meteorites are records of the existence in planetary space of physical conditions more or less similar to those now confined to the immediate neighbourhood of the sun, at a period indefinitely more remote than that of the occurrence of any of the facts revealed to us by the study of geology—at a period which might in fact be called *pre-terrestrial.*"

Are Meteorites merely Modern Phenomena?

It has often been a subject of remark that in spite of the very considerable number of undoubted meteorites now in various collections, we scarcely have traces of any which suggest like falls in any of the geological periods preceding the present one.

The iron found by Prof. Nordenskiöld at Ovifac, Western Greenland, was at first thought to be meteoric iron of Miocene age, but after an analysis of the basalt or lava rocks of Assuk, Disco Island, a part of the same basaltic range in Greenland, only 100 miles from the spot where Prof. Nordenskiöld's discovery was made, it was held by most authorities to be no other than the metallic nickel-iron which is, though extremely rarely, a native product in some terrestrial rocks. Other explorers besides Prof. Nordenskiöld have brought back specimens of this iron, and Dr. Lawrence Smith has stated, not only that the nickel-iron of Ovifac is without doubt of terrestrial origin, but that the specimens brought back by the other explorers resembles the Ovifac and each other remarkably, while they differ from meteoric iron by the large proportion of combined carbon in their composition.

Again, in NATURE, vol. xxxv. p. 36, we have a description of another meteorite supposed to be a fossil one, found in a block of Tertiary coal. It was said to belong to the group of meteoric irons, and was taken from a block of coal about to be used in a manufactory of Lower Austria. On its examination by various specialists, different origins were assigned to it. Some believed it to be meteoric, others an artificial production, and others again thought it was a meteorite modified by the hand of man. After a careful examination Dr. Gurlt came to the conclusion that there was no ground for believing in the intervention of human agency. The mass was almost a cube, two opposite faces being rounded, and the four others being made smaller by these roundings. A deep incision ran all through the cube. The

[1] Quoted from the "Report on Observations of Luminous Meteors during the year 1874–75," p. 247.
[2] NATURE, vol. xii. p. 505.
[3] Flight, *loc. cit.*, p. 111.
[4] *Loc. cit.*, p. 119.

faces and the incision bore such characteristic traces of meteoric iron as to show that the mass was not the work of man. A layer of oxide formed a thin covering of the iron; it was 67 mm. high, 67 mm. broad, and 47 mm. at its thickest part; it was found to be about as hard as steel, and besides carbon it contained a small percentage of nickel. It resembled the meteoric masses of St. Catherine in Brazil, and Braunau in Bohemia, found in 1847.

The evidence, however, is so strong that what we really obtain now at the earth's surface forms but a very small portion of the meteorites which enter the upper air, that it would not be probable that in former ages of the earth's history, when the atmosphere was denser than it is now, anything whatever would be left by the time the surface was reached.

J. NORMAN LOCKYER.

SCIENTIFIC SERIALS.

American Journal of Science, January.—Measurement of the Peruvian arc, by E. D. Preston. In this paper, which was read before the American Association for the Advancement of Science at Toronto, August 1889, the author reviews the whole question of the relative lengths of the earth's axes, dealing in detail with Bouguer's expedition to Peru in 1735, and arguing that the amplitude of his Peruvian arc may be in error by many seconds. Hence he contends that the geodetic science of to-day demands the remeasurement of this arc.—Neutralization of induction, by John Trowbridge and Samuel Sheldon. A system of neutralization for inductive disturbances is here described, which might be adopted where it is impossible to employ entire metallic circuits in which the earth plays no part.—Divergent evolution and the Darwinian theory, by Rev. John T. Gulick. The author discusses Darwin's apparently contradictory views on the causes of natural selection on the one hand, and on the other on the causes of diversity of natural selection. He concludes that, though Darwin has not recognized segregation as a necessary condition of divergence of species, he has indicated one process (geographical or local separation under different environments) by which segregation is produced in nature, adding, however, that this is not the only cause of segregation and consequent divergence.—The Devonian system of North and South Devonshire, by H. S. Williams. During a recent visit to England the author studied this system both on the spot and in the geological collections in London and elsewhere. He dwells especially, (1) on the close resemblance of the English Devonian species to those of the New York Devonian, though mostly passing under different names, and (2) on the character of the North and South Devonian rocks, which in appearance, composition, and order are as different as two distinct systems well can be.—The zinciferous clays of South-West Missouri, and a theory as to the growth of the calamine of that section, by W. H. Seamon. Full analyses are given of the so-called "tallow" and "joint" clays occurring associated and sometimes intermixed in every calamine digging in South-West Missouri. These analyses show a large percentage, often from 50 to 56, of zinc oxide, and it is inferred that at one time all the massive calamine probably existed in "tallow clays" precipitated from solutions.—On the spectrum of ζ Ursæ Majoris, by Edward C. Pickering.—Origin of normal faults, by T. Mellard Reade. Some critical remarks are offered on Prof. Le Conte's recent explanation of the origin of normal faults, which is not new, and presents many insuperable difficulties.—Papers were submitted by J. Dawson Hawkins, on a specimen of minium from Leadville; by William P. Blake, on some minerals from Arizona; by F. A. Genth, on a new occurrence of corundum in Patrick County, Virginia; by Alfred C. Lane, on the estimation of the optical angle of observations in parallel light; by L. G. Eakins, on a new stone meteorite from Texas; by Edward S. Dana, on the barium sulphate from Perkin's Mill, Templeton, Province of Quebec; and by O. C. Marsh, on some new Dinosaurian reptiles recently discovered in Wyoming, Colorado, and Dakota.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, January 9.—"A Milk Dentition in *Orycteropus*." By Oldfield Thomas, Natural History Museum. Communicated by Dr. A. Günther, F.R.S.

Of the few Mammalia in which no trace of a milk dentition has been found, *Orycteropus*, the Aard-Vark, has always occupied a prominent place, owing partly to the peculiar structure of its prominent teeth, and partly to its very doubtful systematic position.

An opportunity has now fallen in my way of proving that it has after all two sets of teeth, those of the first, or milk set, being rudimentary, and probably quite functionless, but nevertheless so far developed as to be all completely calcified, and to be for the most part readily distinguishable by form and position from those of the second or permanent set.

Among the collections in the Natural History Museum there are two very young females of *Orycteropus afer* in spirit, presented by Sir Richard Owen, and it is in these that the milk teeth now to be described occur. The larger of the two measures 18 inches in total length, and the smaller 14 inches.

Each of these specimens has a complete, although rudimentary, set of milk teeth, extending the whole length of the maxillary bones above, and along a rather shorter portion of the mandible below. None, however, are observable in the premaxillæ, or in the corresponding anterior part of the mandibles. The teeth are all quite minute, and it is doubtful whether they would ever have cut the gum.

In the upper jaw there appear to be normally no less than seven milk teeth. Of these the most posterior is by far the largest, has a rudimentary crown, and two distinct roots, anterior and posterior. The others are simple and styliform.

In the lower jaw there are four milk teeth only, of which, again, the most posterior is more or less molariform.

As to the structure of the milk teeth, a horizontal section of the last upper one, ground down in the dry state, presents numerous large openings which are obviously the sockets into which pulp-papillæ have extended, so that the milk teeth show a commencement of the remarkable histological structure characteristic of the permanent teeth.

But important as a knowledge of the presence of a milk dentition in *Orycteropus* is, it does not at present render any easier the difficult questions as to the phylogeny and systematic position of that animal. Although called an Edendate, it has always been recognized as possessing many characters exceedingly different from those of the typical American members of the order.[1] It has in fact been placed with them rather on account of the inconvenience of forming a special order for its reception, than because of its real relationship to them. Now, as they are either altogether toothless or else homodont and monophyodont (apart from the remarkable exception of *Tatusia*), it seems more than ever incorrect to unite with them the solitary member of the *Tubulidentata*, toothed, heterodont, and diphyodont, and differing from them in addition by its placentation, the anatomy of its reproductive organs, the minute structure of its teeth, and the general characters of its skeleton.

But if *Orycteropus* is not genetically a near relation of the Edendates, we are wholly in the dark as to what other Mammals it is allied to, and I think it would be premature to hazard a guess on the subject. Whether even it has any special connection with *Manis* is a point about which there is the greatest doubt, and, unfortunately, we are as yet absolutely without any palæontological knowledge of the extinct allies of either. *Macrotherium* even, usually supposed from the structure of its phalangeal bones to be related to *Manis*, has lately proved (see Osborn, *American Naturalist*, vol. xxii. p. 728, 1882) to have the teeth and vertebræ of a Perissodactyle Ungulate, and one could not dare to suggest that the ancestors of *Manis* or *Orycteropus* were to be sought in that direction. Lastly, as the numerous fossil American Edentates do not show the slightest tendency to an approximation towards the Old World forms, we are furnished with an additional reason for insisting on the radical distinctness of the latter, whose phylogeny must therefore remain for the present one of the many unsolved zoological problems.

Physical Society, January 17.—Prof. W. G. Adams, Vice-President, in the chair.—Owing to the unavoidable absence of Mr. F. B. Hawes, his paper on a carbon deposit in a Blake telephone transmitter was postponed.—Dr. S. P. Thompson made a communication on electric splashes, and illustrated his subject by beautiful experiments on the production of Lichtenberg's figures. The author has recently investigated these phenomena as modified by varying the conditions under which

[1] On this subject see especially Flower, "On the Mutual Affinities of the Animals composing the Order Edentata," Zool. Soc. Proc., 1882, p. 358 *et seqq.*

the figures are obtained, and has arrived at the following conclusions: (1) the nature of the dielectric plate does not change the character of the figures produced, and (2) the nature of the powders used seems to have no material effect on their shape. In the course of his experiments he has found a mixture of sublimed sulphur and lycopodium to give better figures than the red lead and sulphur usually employed, and also that a large and highly polished knob is advantageous, particularly when the Leyden jar is charged negatively. Sometimes when obtaining negative figures, nebulous patches occur, and these were attributed to the so-called electric winds sent off from roughnesses on the knob when not sufficiently well polished. If instead of bringing the knob in contact with the plate, it is only brought near to it, then a peculiar figure closely resembling a "splash" results. A positive splash consists of short lines radiating from the point of approach, whilst a negative splash is made up of more or less rounded spots which become elongated in a radial direction as their distance from the centre of the splash increases. Negative splashes are, however, much more difficult to produce than positive ones. When viewed in the dark, the discharge producing the splash is seen to consist of a bundle of small sparks which branch outwards on approaching the plate. In conclusion the author remarked that roughnesses on a conductor produced more electric winds when the conductor is charged negatively than when positively charged, and invited the opinions of members as to the causes of the differences observed between positive and negative electricity. Prof. Rücker said he had recently obtained figures produced by discharges on photographic plates. Generally he observed that negative discharges produce roundish patches, whilst positive ones give more filamentary figures. On passing a spark across a glass plate covered with lampblack, its trace was found to have a black core at one end, whilst the other was quite clear. He also made remarks on the distinctive character of the positive and negative discharges in partial *vacuo*, and considered investigations as to the causes of such differences to be of great importance. Prof. Adams thought any attempt to discover the causes of such differences as those noted in the paper was to be commended, for the well-known fact that it is more difficult to insulate a negative charge than a positive one has long needed an explanation.—A paper on galvanometers, by Prof. W. E. Ayrton, F.R.S., T. Mather, and W. E. Sumpner, was read by Prof. Ayrton. In fitting up the Physical Laboratories of the Central Institution of the City and Guilds of London Institute, the authors have had occasion to obtain galvanometers of various types and patterns, some of which have been made to special designs, and specimens of instruments embodying recent improvements were exhibited at the meeting. The question as to whether fairly sensitive galvanometers should be astatic or non-astatic was answered in favour of the former system, from the fact of its being less affected by external magnetic disturbances, and the greater ease with which great sensibility may be obtained. The usual method of placing the mirror inside the coil was shown to be undesirable, and in the newer forms of instruments Mudford's improvement of placing the mirror outside the coils has been adopted; the space near the axis of the coil being nearly filled with wire. It was also shown that if wire be wound in a certain approximately spheroidal space near the magnets, then these convolutions will tend to oppose the more distant portions of the coil; however, by winding the two parts in opposite directions they conspire to deflect the magnet. Details as to methods of supporting the coils were then discussed, and the importance of fitting them in boxes mounted on hinges or otherwise, so as to be readily removable, was pointed out. A galvanometer devised for teaching purposes, and provided with variable damping arrangements was described, in which the damping is effected by enclosing the mirror in a glass cell whose sides can be caused to approach or recede by turning a milled head outside the instrument. This arrangement enables the damping to be varied between wide limits, and its effect on the swing produced by a given discharge can be determined. The instrument is also serviceable both as an ordinary damped galvanometer, or as a fairly ballistic one. In measuring quantities of electricity by the first swing of a galvanometer needle, a correction has usually to be introduced for damping; this correcting factor is simple enough when the damping is small, but becomes more complicated as the damping increases, and to facilitate the calculations a table of values of the factor for various values of λ (the logarithmic decrement) has been calculated. From this it appears that, for values of λ less than 0·5, the value of the factor is very nearly $(1 + \frac{1}{2}\lambda)$, the correction usually employed. Improvements in methods of insulating the coils and terminals of galvanometers required for insulation tests were next described, the principle of which may be gathered from Figs. 107 and 108 in Prof. Ayrton's "Practical Electricity." A special form of instrument for high insulation work was exhibited, in which the copper resistance of the coils is nearly 400,000 ohms, and the shortest path along which surface leakage can take place from the coils to the base of the instrument is between 30 and 40 inches of ebonite artificially dried by sulphuric acid. This is attained by supporting the coils from two corrugated ebonite rods which depend from a brass ring carried on the top of three corrugated pillars fixed to the base plate. The instrument was constructed to drawing and specification by Messrs. Nalder Brothers, but the method of supporting the coils was suggested by Messrs. Eidsforth and Mudford. With reference to the proportionality of deflection to current in reflecting galvanometers, it was pointed out that ordinary instruments may differ as much as 2 per cent. within the limits of the scale, hence showing the necessity for calibration when any approach to accuracy is desired. Galvanometers of the D'Arsonval type sometimes differ from proportionality quite as much as the one above referred to, but by fitting such instruments with curved pole pieces, and allowing the coil to hang freely from the top suspension, a proportionality true to less than 0·15 per cent. has been attained over a scale about 30 inches long. Coming to the question of sensitiveness, the importance of keeping the wire as close as possible to the magnets was brought prominently forward, as well as the necessity of reducing the "figures of merit" of various instruments to the same standard, in comparing their sensibilities. The standard adopted as most convenient and closely approximating to practical usage is arrived at by supposing the distance of the mirror from the scale to be equal to 2000 scale divisions, and the sensibilities for current and quantity are given as *scale divisions per micro-ampere*, and *scale divisions per micro-coulomb* respectively. The period of oscillation is also taken into account. A table showing the resistances, sensibilities, coefficients of self-induction and volumes of the coils of various instruments, together with the relations existing between them, accompanies the paper, and from this it appears that in the best astatic double coil instruments, of from 10,000 to 30,000 ohms resistance, the number of scale divisions per micro-ampere may reach 400 times the resistance to the $\frac{2}{5}$th power ($400\ R^{\frac{2}{5}}$) when the period is 10 seconds. In obtaining data of various instruments the authors have consulted, amongst others, Prof. Threlfall's paper on the measurement of high specific resistances, in the *Phil. Mag.* for December 1889, and noticed two serious errors. The first of these makes an instrument constructed according to Messrs. Gray's pattern nine times less sensitive than it actually was, whilst the sensibility of a form recommended in the paper is given seventeen times too great. On account of the lateness of the hour, the discussion was adjourned till February 6, before which time it is hoped that a fairly full abstract will appear in the technical papers.

Geological Society, January 8.—W. T. Blanford, F.R.S., President, in the chair.—The following communications were read:—On some British Jurassic fish-remains referable to the genera *Eurycormus* and *Hypsocormus*, by A. Smith Woodward. Hitherto our knowledge of the Upper Jurassic fish fauna has been mainly derived from specimens found in fine lithographic stones, where the various elements are in a state of extreme compression. Within the last few years remains of similar fish have been discovered in the Oxford and Kimeridge Clays of England, and these are of value for precise determination of certain skeletal features in the genera to which they belong. The author described *Eurycormus grandis* from the Kimeridge Clay of Ely, a large species which makes known for the first time the form and proportions of several of the head-bones in this genus. A technical description of all the bones the characters of which are distinguishable was given, and the author concluded that there is considerable similarity between the head of *Eurycormus* and the recent Ganoid *Amia*, even to minute points of detail. He further described *Hypsocormus tenuirostris* and *H. Leedsii* from the Oxford Clay of the neighbourhood of Peterborough, the osteology of this genus not having as yet been elucidated. Portions of the jaws have been discovered, affording valuable information as to the form and dentition of the principal elements. These jaws are not precisely paralleled by any other Jurassic genus, though they possess a resemblance to *Pachycormus*, as also to the Upper Cretaceous genus, *Protosphyræna*. The President remarked that *Amia* is a

freshwater genus, and inquired whether the fossil fish was freshwater or marine. Mr. E. T. Newton remarked upon the great interest and importance of the paper. The author, in reply to the President's question, said that the old Ganoids were marine, and it was only in more recent times that they had become restricted to fresh water.—On the Pebidian volcanic series of St. David's, by Prof. C. Lloyd Morgan. After a brief sketch of the principal theories that have been propounded, the author concluded that our knowledge of this series has not yet reached "a satisfactory position of stable equilibrium." His own communication was divided into three sections. *The Relation of Pebidian to Cambrian:* There are four localities where the junction is described—Caerbwdy Valley, St. Non's Bay, Ogof Golchfa, and Ramsey Sound. The stratigraphy of the second of these was given with much detail, and illustrated. The author concluded that here, together with clear signs of local or contemporaneous erosion, the general parallelism of the strike of Pebidian and Cambrian is most marked. There is no evidence of any bending round of the conglomerate against the strike of the Pebidians. The stratigraphical evidence in each of the localities having been considered, together with the evidence offered by the materials of the Cambrian conglomerate and local interstratification with the volcanic beds (the interdigitation at Carnarwig being well marked), he concluded that there was no great break between the conglomerate and the underlying Pebidians. The uppermost Pebidian already foreshadowed the sedimentary conditions of the Harlech strata, and the change emphasized by the conglomerate was one that followed volcanic conditions after no great lapse of time. Hence the relation of the Pebidian to the Cambrian is that of a volcanic series, for the most part submarine, to succeeding sedimentary strata—these strata being introduced by a conglomerate formed in the main of foreign pebbles borne onward by a current which swept the surface of, and eroded channels in the volcanic tuffs and other deposits. He was disposed to retain the name Pebidian as a volcanic series in the base of the Cambrian system. *The Pebidian Succession:* With the exception of some cinder-beds, which appear to be subaërial, the whole series was accumulated under water. There is no justification for making separate subdivisions; the series consists of alternating beds of tuff of varying colour and basicity, the prevailing tints being dark green, red-grey, and light sea-green. In the upper beds there is an increasing amount of sedimentary material, and more rounded pebbles are found. Basic lava-flows occur, for the most part, in the upper beds. Detailed work, laid down on the 6-inch Ordnance map, appears to establish a series of three folds—a northern anticline, a central syncline, and a southern anticline—folded over to form an isocline, with reversed dips to the south-east. The axis of folding is roughly parallel to the axis of St. David's promontory. The total thickness is from 1200 to 1500 feet. The author devoted a considerable number of pages to further details concerning this series of deposits. He failed to find the alleged Cambrian overlap. "The probabilities are that it is by step-faults brtween Rhoson and Porth Sele, and not by overlap, that the displacement of the conglomerate has there been effected." Also at Ogof Gôch it does not rest upon the quartz-felsite breccia and sheets (group C, of Dr. Hicks), but is faulted against them. A section was devoted to the felsitic dykes, and it was suggested that they may be volcanic dykes of Cambrian age. *The Relation of the Pebidian to the Dimetian:* The author has not been able to satisfy himself of the existence of the Arvonian as a separate and distinct system. He notes the junction of Pebidian and Dimetian in Porthlisky Bay and the Allen Valley at Porth Clais, at neither of which places are there satisfactory evidences of intrusion. At Ogof Llesugn the intrusive character of the Dimetian was strongly impressed upon him. He criticized the mapping of Dr. Hicks, and pointed out the difficulties which present themselves in the way of mapping the Dimetian ridge as pre-Cambrian. He pointed out that not a single pebble of Dimetian rock, such as those now lying on the beach in Porthlisky Bay, is to be found in the conglomerate. He concluded that the Dimetian is intrusive in the southern limb of the isocline, and that there are no Archæan rocks *in situ*. After the reading of this paper there was a discussion, in which the President, Dr. Hicks, Prof. Blake, Prof. Hughes, and Mr. Williams took part.

SYDNEY.

Royal Society of New South Wales, November 6, 1889.—Monthly meeting.—Prof. Liversidge, F.R.S., President, in the chair.—The Chairman announced the death of the Rev. J. E. Tenison-Woods, who had been an honorary member of the Society since 1875.—The following papers were read:—Aids to the sanitation of unsewered districts, poudrette factories, by Dr. J. Ashburton Thompson.—Notes on Goulburn lime, by E. C. Manfred.—Notes on some minerals, &c., by John C. H. Mingaye.

December 4.—Monthly Meeting.—Prof. Liversidge, F.R.S., President, in the chair.—The following papers were read:—Well and river waters of New South Wales, by W. A. Dixon.—The Australian aborigines, by Rev. John Mathew.

PARIS.

Academy of Sciences, January 20.—M. Hermite in the chair.—On the various states of the carbon graphites, and on the chemical derivatives corresponding to them, by MM. Berthelot and P. Petit. The graphites, when oxidized by the wet process at a low temperature, form ternary compounds, one of whose terms has been discovered by Brodie. But M. Berthelot has since shown that there exist several chemically distinct graphites, each forming a particular graphitic oxide, which yields a corresponding hydrographitic and pyrographitic oxide, and which may be recovered with all their primitive properties. These various graphites and the series of corresponding compounds have been studied, first by their composition and behaviour, and in a second memoir by the measurement of the heats of combustion and formation.—Remarks on the formation of the nitrates in plants, by M. Berthelot. The author points out that the facts established by Haeckel and Lundström, taken in connection with his own observations, tend to show an affinity between the microbes present in the soil and those developed in the plant. This applies to the microbes which fix the nitrogen of vegetable humus and the leguminous plants, as well as to those which similarly form the nitrates in amaranthus, sterculia, the coffee shrub and vegetable humus.—Note on a fundamental point of the theory of polyhedrons, by M. de Jonquières. The paper deals with Euler's famous formula $S + H = A + 2$, and shows that it is applicable, and intended by Euler to be applicable, to all polyhedrons without exception, and not restricted to any particular class, as supposed by Legendre, Cauchy, and others.—Ephemerides for the search of the periodical comet of d'Arrest on its return in 1890, by M. Gustave Leveau. Having previously obtained the elements for the years 1870, 1877, and 1883, by allowing for the disturbing influence of Jupiter, Saturn, and Mars, M. Leveau here supplies those for 1890 (February 25, mean Paris time) by studying the disturbing effects produced by Jupiter in the interval between 1883 and 1890.—Observations of Swift's comet made at the Observatory of Nice with the 0·38 m. equatorial, by M. D. Eginitis.—On the solar statistics for the year 1889, by M. Rud. Wolf. From the solar observations made at Zurich and the magnetic observations recorded at Milan, the author has constructed a table of monthly means showing that both the relative numbers and the magnetic variations have continued to diminish during 1890. But he thinks that the retrograde movement will soon cease, and that we probably entered the minimum period towards the end of last year.—On the theory of the figure of the planets, by M. M. Hamy. An attempt is here made to realize theoretically the conditions of a system answering to M. Poincaré's remarkable theorem published in the *Comptes rendus* for June 1888.—On the integration of an equation with partial derivatives, by M. Zaremba. The paper deals with an equation of the form

$$\frac{d^2x}{dxdy} + \phi_1(x+y)\left(\frac{dz}{dx} + \frac{dz}{dy}\right) + \phi_2(x+y)z = 0,$$

where ϕ_1 and ϕ_2 are two functions whatsoever of $x + y$, and shows that the determination of the general integration may be reduced to the integration of an ordinary linear differential equation of the second order, and to quadratures.—On the variation of the resistance of bismuth in the magnetic field, by M. A. Leduc. The author here continues his studies of the electric resistance of bismuth as affected by varying temperature.—Calculation of the compressibility of nitrogen up to 3000 atmospheres, by M. Ch. Antoine. The results of fresh calculations are here summed up in a table resuming all the data relative to the pressure of nitrogen up to a pressure of 3000 atmospheres.—On the combinations of the metals of the alkalies with ammonia, by M. H. W. Bakhuis Roozeboom. An explanation is offered of the curious phenomena mentioned by M. Joannis in his recent

communication (*Comptes rendus*, cix. p. 900) on the combinations of potassium and sodium with ammonia.—On the absorption of the ultra-violet rays by some organic substances belonging to the fatty series, by MM. J. L. Soret and Alb. A. Rilliet. These studies, which to a large extent confirm the conclusions of Messrs. Hartley and Huntington (Philosophical Transactions of the Royal Society, 1879), show in a general way that the measurement of the absorption of the ultra-violet rays constitutes a delicate means of estimating the purity of organic substances.—On the refracting powers of double salts in solution, by M. E. Doumer. These researches have been carried on by the same method which enabled the author to determine the refracting powers of simple salts. The results, which are here tabulated, show that the molecular refracting power of a double salt is equal to the sum of the molecular refracting powers of the constituent simple salts; and in general, the molecular refracting power of any salt, simple or double, is proportional to the number of valences of the metallic part of the salt.—Papers were read by M. Ph. A. Guye, on the molecular constitution of bodies at the critical point; by M. Raoul Varet, on the reactions between the salts of copper and the metallic cyanides; by MM. C. Chabrié and L. Lapicque, on the physiological action of selenious acid; and by M. L. de Launay, on the geology of the island of Lesbos. M. de Launay considers the volcanic eruptions of this island as comparatively recent, possibly not older than the Pliocene epoch, and doubtless contemporary with the disturbances resulting in the creation of the Ægean Sea in a region previously forming a vast marshy plain with shallow lakes.

DIARY OF SOCIETIES.

LONDON.

THURSDAY, JANUARY 30.

ROYAL SOCIETY, at 4.30.—Investigations into the Effects of Training Walls in an Estuary like the Mersey: L. F. Vernon Harcourt.—On Outlying Nerve-Cells in the Mammalian Spinal Cord: C. S. Sherrington.—On the Germination of the Seed of the Castor-oil Plant (Ricinus communis): Prof. J. R. Green.

ROYAL INSTITUTION, at 3.—Sculpture in Relation to the Age: Edwin Roscoe Mullins.

FRIDAY, JANUARY 31.

ROYAL INSTITUTION, at 9.—Smokeless Explosives: Sir Frederick Abel, C.B., F.R.S.

SATURDAY, FEBRUARY 1.

ESSEX FIELD CLUB, at 7.—Annual General Meeting.—Migration of Birds: E. A. Fitch, President.

ROYAL INSTITUTION, at 3.—The Natural History of the Horse, and of its Extinct and Existing Allies: Prof. Flower, C.B., F.R.S.

SUNDAY, FEBRUARY 2.

SUNDAY LECTURE SOCIETY, at 4.—The Health of the Mind; and Mental Contagions: Dr. B. W. Richardson, F.R.S.

MONDAY, FEBRUARY 3.

SOCIETY OF ARTS, at 8.—The Electromagnet: Dr. Silvanus P. Thompson.

SOCIETY OF CHEMICAL INDUSTRY, at 8.—On the Properties and Applications of Metallic Compounds of the Phenols: A. H. Allen and W. W. Staveley.

ARISTOTELIAN SOCIETY, at 8.—The Conception of Sovereignty: D. G. Ritchie.

ROYAL INSTITUTION, at 5.—General Monthly Meeting.

TUESDAY, FEBRUARY 4.

ZOOLOGICAL SOCIETY, at 4.—On the Morphology of a Reptilian Bird (Opisthocomus cristatus): W. K. Parker, F.R.S.—Observations on Wolves, Jackals, Dogs, and Foxes: A. D. Bartlett.—A Synopsis of the Genera of the Family Soricidæ: G. E. Dobson, F.R.S.

INSTITUTION OF CIVIL ENGINEERS, at 8.—Bars at the Mouths of Tidal Estuaries: W. H. Wheeler.

ROYAL INSTITUTION, at 3.—The Post-Darwinian Period: Prof. G. J. Romanes, F.R.S.

WEDNESDAY, FEBRUARY 5.

GEOLOGICAL SOCIETY, at 8.—The Variolitic Rocks of Mount Genèvre: G. A. J. Cole and J. W. Gregory.—The Propylites of the Western Isles of Scotland and their Relation to the Andesites and Diorites of the same District: Prof. J. W. Judd, F.R.S.

ENTOMOLOGICAL SOCIETY, at 7.—On the Peculiarities of the Terminal Segment in some Male Hemiptera: Dr. Sharp.—The Lepidoptera of Burmah: Colonel Chas. Swinhoe.—On the Phylogenetic Significance of the Wing-Markings in certain Genera of Nymphalidæ: Dr. F. A. Dixey.

SOCIETY OF ARTS, at 8.—High-Speed Knitting and Weaving without Weft: Arthur Paget.

UNIVERSITY COLLEGE CHEMICAL AND PHYSICAL SOCIETY, at 4.30.—The Life and Work of Faraday: S. B. Schryver.

THURSDAY, FEBRUARY 6.

ROYAL SOCIETY, at 4.30.

LINNEAN SOCIETY, at 8.—On the Stamens and Setæ of Scirpeæ: C. B. Clarke, F.R.S.—On the Flora of Patagonia: John Ball, F.R.S.

CHEMICAL SOCIETY, at 8.—Ballot for the Election of Fellows.—The Oxides of Nitrogen: Prof. Ramsay, F.R.S.—Studies on the Constitution of Tri-Derivatives of Naphthalene: Dr. Armstrong and W. P. Wynne.—On the Action of Chromium Oxychloride on Nitrobenzole: G. G. Henderson and J. Morrow Campbell.

ROYAL INSTITUTION, at 3.—Sculpture in Relation to the Age: Edwin Roscoe Mullins.

FRIDAY, FEBRUARY 7.

PHYSICAL SOCIETY, at 5.—Annual General Meeting.—On Galvanometers: Prof. W. E. Ayrton, F.R.S., T. Mather, and W. E. Sumpner.—On a Carbon Deposit in a Blake Telephone Transmitter: F. B. Hawes.

GEOLOGISTS' ASSOCIATION, at 7.30.—Annual General Meeting.—Notes on the Nature of the Geological Record: The President.

SOCIETY OF ARTS, at 5.—The Utility of Forests and the Study of Forestry: Dr. Schlich.

INSTITUTION OF CIVIL ENGINEERS, at 7.30.—Reclamation of Land on the River Tees: Colin P. Fowler.

ROYAL INSTITUTION, at 9.—The London Stage in Elizabeth's Reign: Henry B. Wheatley.

SATURDAY, FEBRUARY 8.

ROYAL BOTANIC SOCIETY, at 3.45.

ROYAL INSTITUTION, at 3.—The Natural History of the Horse, and of its Extinct and Existing Allies: Prof. Flower, C.B., F.R.S.

CONTENTS.

THURSDAY, FEBRUARY 6, 1890.

TAVERNIER'S TRAVELS IN INDIA.

Travels in India of Jean Baptiste Tavernier, Baron of Aubonne. Translated from the original French Edition of 1676, &c., by V. Ball, LL.D., F.R.S., F.G.S., &c. In Two Volumes. (London: Macmillan and Co., 1889.)

JEAN BAPTISTE TAVERNIER was a Sindbad of the seventeenth century. To an insatiable love of travel, which prompted him even in his boyhood to rove through the greater part of Europe, and in his mature life to accomplish no less than six voyages to Persia, India, and the still more remote East, he united the faculties of a shrewd and successful trader. By his traffic in jewels and other costly commodities of small bulk, he turned his wanderings to profitable account, and amassed a fortune which enabled him to purchase the Barony of Aubonne, and to enjoy the dignified retirement of a wealthy old age. But, like a true traveller, he remained active-minded and active-bodied to the last. At the age of 79, attracted by the offer of the Elector of Brandenburg to conduct an embassy to India, he set forth on a circuitous journey through Europe, and, disposing of his estate and *château* of Aubonne, he embarked on renewed mercantile ventures. The few remaining years of his life were passed, for the most part, in journeying to and fro in Europe, and he died while so occupied. The place of his death has long been doubtful, and it has only recently been discovered, on the authority of a letter from the Swedish Resident at Moscow, that the indefatigable traveller drew his last breath at Smolensk, in February 1689, when on his journey to the ancient Russian capital.

Despite some inaccuracies and inconsistencies, due mainly to the incompetent editing of the original work, Tavernier's account of his travels has long been appealed to by Indian historians as a recognized authority—the testimony of an eye-witness to the condition of India under the later great Mogul emperors. At the time of his visits, the Mogul Empire was in the zenith of its power and splendour. On the occasion of his first journey to India, he found Shah Jehan, "the most magnificent prince that ever appeared in India," peaceably seated on the Imperial *masnad;* and throughout his dominions, though these were less extensive than in the time of his successor Aurungzebe, a degree of good administration and general prosperity surpassing that attained under any previous or subsequent emperor. He quitted India for the last time only about two months after the death of Shah Jehan, then deposed and imprisoned, when Aurungzebe was setting out on that career of conquest and oppression that in the following century brought about the wreck of the Mogul Empire, and exposed its rich cities and provinces to be wasted and despoiled by Maráthá hordes and Afghan invaders.

At a Court gathered around the famous peacock throne, where emperor and nobles vied with each other in the acquisition of costly jewels, an expert such as Tavernier was received as a welcome visitor; and in pursuit of his calling he travelled without hindrance through the length and breadth of India, visiting the European settlements of Surat, Goa, Madras, and Kásimbazár, the independent Court of Golconda (Hyderabad), and certain of the diamond-mines that were then actively worked both in Southern and Northern India. His work is a medley of historical memoranda, incidents of travel, itineraries, and details of his commercial dealings, put together without much system, but nevertheless highly instructive, and apparently far more trustworthy than was conceded to him by most of his contemporaries; altogether furnishing a fund of information respecting the state of India in the middle of the seventeenth century.

The latest English translation of Tavernier's travels appeared more than two centuries ago, and as Mr. Ball remarks, owing to the translator's misconception of the author's meaning, through want of local knowledge, and to serious abridgment, it gives a very inadequate idea of the true merits of the original work. Mr. Ball's own long experience of India, and his familiarity with its geography and the varied phases of native life, would alone have enabled him to correct most of the errors of his predecessors; and the deficiencies as a philological and historical critic which he modestly urges as having determined him, for a time, to abstain from attempting a new translation, have been made good by the invaluable assistance afforded by the late Sir Henry Yule, under whose advice he eventually undertook the work. The result is the two handsome volumes now before us, in which for the first time the old traveller's experiences are presented to English readers, elucidated by the results of modern research, and in a form which very greatly enhances their value for all purposes of future reference. Some few inconsistencies remain, and are duly pointed out in the footnotes, but they are such as relate to matters of detail, occasional confusion of dates or persons, and the like; and they do not appreciably detract from the general trustworthiness of the narration.

With the political and historical data of Tavernier's work it is hardly our province to deal in this place. Most of his facts relating to the Court of Delhi were probably furnished to him by his cotemporary and sometime fellow-traveller Bernier, and all that is important in them has been long rendered familiar to English readers in the lucid pages of Elphinstone. Neither need we dwell on his descriptions of native customs or the manner of life of those European exiles of various nationalities who were then, as pioneers, exploiting the riches of the East, with no small display of mutual jealousy and animosity, and indulgence in practices sometimes hardly less barbarous than those of the indigenous population amid which they dwelt. The social condition of the Indian people in Tavernier's day was essentially the same as when, more than a century and a half later, the British Empire having been raised and consolidated on the ruins left by Maráthás and Patháns, a new era of peace and civilization was inaugurated by Lord Bentinck, and the suppression of thuggi, dacoity, sati, and other barbarous rites of the Hindu religion, preceded the establishment of schools and Universities, and the opening up of the wilds of India by systems of roads and railways. The social regeneration of India, such as it is, has been almost exclusively the work of the last seventy years, and even now it has hardly penetrated far below the surface.

P

It was the information given by the traveller on the diamond-mines worked in his day, that first drew Mr. Ball's attention to the subject of Tavernier's travels. The mines visited and described by him have long been abandoned, and even their very sites forgotten. With free labour, and at its present enhanced rates, diamond-working is no longer so remunerative as under the despotic governments of the seventeenth century, and it is within the recollection of the present writer that the working of one of the most productive mines of the former Golconda State was let on behalf of the British Government at the modest rental of 100 rupees. Tavernier gives it to be understood, indeed, that only four mines were worked, all of which he visited; but Mr. Ball tells us there is ample reason for believing that they were far more numerous than he had any conception of; and in an appendix he gives a full list of all the Indian localities at which diamonds have been obtained as far as is known, together with the geographical co-ordinates of all such as he has succeeded in identifying. Owing to the vagaries of phonetic spelling, and the ignorance of Indian geography on the part of many who have dealt with this subject, this identification has been far from easy. As amusing examples of the way in which localities have been confused by some previous writers, Mr. Ball tells us that "one author gives Pegu as a diamond-mine in Southern India; in the Mount Catti of another we have a reference to the Ghâts of Southern India"; and he adds: "For some time I was unable to identify a certain Mr. Cullinger, who was quoted by one writer, in connection with diamonds. Will it be believed that this *gentleman* ultimately proved on investigation to be the *fort* of Kálinjar?"—a well-known historical fortress in Bundelkhand.

Indian diamonds are found exclusively in rocks of the Vindhyan formation or in the gravels of rivers that drain these rocks. The formation consists of sandstones, limestones, and other sedimentary rocks, certainly not more recent than the Lower Palæozoic age, but being unfossiliferous, their precise age cannot be determined. In Southern India the diamonds occur only in the Bánaganpili sandstone, at the base of the lower subdivision of the Vindhyan series, or in gravels derived from that bed. This is described by the authors of the "Manual of the Geology of India" as usually from 10 to 20 feet thick, consisting of gravelly, coarse sandstone, often earthy, and containing numerous beds of small pebbles. The diamonds are found in some of the more clayey and pebbly layers, and in the opinion of Dr. W. King, the present Director of the Indian Geological Survey, they are innate in the rock. This view does not, however, appear to commend itself to the authors of the manual. In Northern India, at Panna, in Bundelkhand, the diamond bed is in the upper division of the Vindhyan series; but it is considered not improbable that here also the original *nidus* of the diamonds was, as in Southern India, a bed of the lower subdivision, pebbles of which occur in the diamond bed, and are extracted and broken up in the search for the gem.

As is well known, Tavernier examined, and in his book described and figured, the famous Great Mogul diamond, then in the possession of the Emperor Aurungzebe; and he has been often cited as a principal witness by those who have discussed the question of the history of the Koh-i-noor. To this subject Mr. Ball devotes a long note in the appendix, arriving at conclusions which differ from those of Prof. N. S. Maskelyne, and indeed of most previous writers, with the exception of James Forbes, Major-General Sleeman, and Mr. Tennant. The argument is somewhat complex, and hardly admits of abstraction, and we must therefore refer those who are interested in the subject to the text of Mr. Ball's note. It will suffice here to indicate the main issues. They are concerned with the identification *inter se* of the three diamonds known respectively as the Mogul diamond, Baber's diamond, and the Koh-i-noor. The first of these, described and figured by Tavernier, is the largest diamond on record, and is stated to have weighed originally, before cutting, 900 *ratis* (an Indian weight still in use, but the value of which has varied greatly at different times and under different circumstances). When Tavernier saw it, it had been reduced by unskilful cutting to about two-fifths of its former size, and weighed only 379½ *ratis*, which Mr. Ball computes to be equivalent to 268 English carats. Baber's diamond, of which Tavernier makes no mention, but which is equally historic, Mr. Ball thinks was probably retained by the imprisoned Shah Jehan, and acquired by Aurungzebe only after Shah Jehan's death. The weight of this stone is computed by Mr. Ball, from the statements of Baber and Ferishta, to have been 186 English carats. The weight of the Koh-i-noor when first brought to England was exactly the same as that computed for Baber's diamond, or, accurately, 186·06 carats. Now Prof. Maskelyne, General Cunningham, and several other writers regard these three stones as identical, and the former suggests that Tavernier's estimate of the weight of the Great Mogul diamond in carats (probably Florentine) was erroneous, and due to his having adopted a mistaken value for the *rati*. This view Mr. Ball is unable to accept. Nevertheless he considers it probable that the Koh-i-noor is the remnant of the Mogul diamond, from which portions have been removed while it was in the possession of the unfortunate grandson of Nadir Shah, or some other of those through whose hands it passed before it was acquired by Runjeet Singh; and that Baber's diamond was a distinct stone, now in the possession of the Shah of Persia, and known as the Dariya-i-noor (sea of lustre), the weight of which is also 186 carats.

Mr. Ball's careful criticism of the available evidence, and his clear setting forth of the several steps of his argument, give weight to the conclusion at which he finally arrives, that will doubtless be acknowledged even by those who differ from him. But as regards the identity of the Koh-i-noor and the Mogul diamond, there remains one objection which, as it appears to us, Mr. Ball has hardly adequately disposed of. If Tavernier's figure, as reproduced by Mr. Ball, represents at all faithfully the general form and especially the height of the Mogul diamond, it is difficult to see how a comparatively flat stone like the Koh-i-noor could have been obtained from it without a much greater reduction of its weight than the 82 carats which are all that his data admit of. The lateral dimensions of the two stones accord fairly enough, so that any reduction of Tavernier's figured stone, to bring it down to the required size, could be

effected only by diminishing its height; in which case it would hardly correspond to his description of its form as that of an egg cut in two. The question can only be fairly tested by the weighment of a model constructed as nearly as possible in accordance with Tavernier's figure, and of such lateral dimensions as to be capable of including the Koh-i-noor. It may be that such a model, of the specific gravity of the diamond, would be found much to exceed Tavernier's reported weight of the stone, in which case the importance of his figure as an item of evidence, would be greatly invalidated.

Whatever may be the final outcome of this controversy, Mr. Ball has done a good service to literature and science in re-translating Tavernier's work, in its careful editing, and in throwing light on much that has hitherto remained obscure. The result will certainly be that which he has anticipated, the vindication of Tavernier's claim "to be regarded as a veracious and original author."

H. F. B.

OUR BOOK SHELF.

Star Land. By Sir Robert S. Ball, LL.D., F.R.S. (London: Cassell and Co., 1889.)

THE author of this work is now so well known as a popular expounder of astronomical subjects that it is quite sufficient praise of his new book to say that it fully sustains his reputation. The book is described as "talks with young people about the wonders of the heavens," being founded chiefly on notes taken at his courses of juvenile lectures at the Royal Institution. Astronomy gives plenty of scope for the exercise of the imagination, and Dr. Ball takes full advantage of this. The book abounds with anecdotes and homely illustrations, calculated to impress the facts on the memory as well as to excite wonder at them. The startling figures dealt with in astronomy are, as usual, converted into railway train notation, and otherwise illustrated. One new illustration of the distances of the stars is that it would take all the Lancashire cotton factories 400 years to spin a thread long enough to reach the nearest star at the present rate of production of about 155,000,000 miles per day. The irregularities in the motion of Encke's comet are explained in an interesting dialogue between the "offending comet" and the astronomer, in which the comet explains that his delay was due to the fact that Mercury was "meddlesome."

The only disappointing parts of the book are those which deal with astronomical physics. One point not sufficiently insisted upon is the now generally acknowledged meteoritic constitution of comets; a connection is certainly suggested, but that comets are now supposed to be simply dense swarms of meteorites is not stated at all. Nebulæ, again, are described as "masses of glowing gas," notwithstanding the recent researches on the subject. The theory that meteorites are the products of ancient terrestrial volcanoes is also still adopted by Dr. Ball, without any consideration of the objections to such a view.

The book is well illustrated, and will undoubtedly awaken an interest in the subject in all intelligent readers.

The Magic Lantern: its Construction and Use. By a Fellow of the Chemical Society. (London: Perken, Son, and Rayment.)

THE third edition of this little book has been issued, and will be exceedingly useful to those who work with the lantern. Descriptions are given of the various lights used in lanterns, from the oil lamp to the electric arc; the methods of making simple slides are entered into, and a few experiments, illustrative of elementary scientific principles, are well included. The work is thoroughly practical; none of the little details so necessary to beginners have been omitted, whilst many of the hints it contains may be of service to all who use this optical instrument, whether it be for lecture purposes or for recreation only.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Acquired Characters and Congenital Variation.

I DO not see that the Duke of Argyll's last letter in any way strengthens his position. The questions at issue with regard to evolution are now, I believe, thoroughly understood by biologists. Nothing, in my opinion, can solve them in the direction the Duke desires but the evidence of fact. And that, I can only repeat, is precisely what is not forthcoming. I am equally of opinion that the discussion has been worn threadbare. I should not myself have interfered in it, had it not seemed desirable to show that the motives attributed by the Duke to those who accept Darwinian principles were destitute of foundation.

This part of his position the Duke does not attempt to defend. As to the rest he merely restates what he has said before. His remarks fall under two heads, and I shall content myself with the briefest possible comment upon these.

(1) *Acquired Characters.*—The Duke gives what I presume he intends as a logical proof of the theorem that acquired characters are inherited. It may, I think, be formally expressed as follows:—

"It is always possible to assert" that acquired characters are developed latent congenital characters.

It is admitted that congenital characters are inherited.

∴ Acquired characters are inherited.

It will be observed in the first place that this is a mere *a priori* argument. And next that, while it is not denied by Darwinians that the organism is a complex of congenital tendencies, limitations, and possibilities, this is entirely beside the question. From Lamarck to Darwin, Weismann, and Lankester, the meaning of "acquired characters" has been clearly defined. They are those changes of hypertrophy, extension, thickening, and the like, which are obviously due to the direct physical action of the environment on the body of the individual organism. It was these changes which Lamarck asserted were transmitted to the offspring; and it is this transmission which it is now maintained needs demonstration as a fact.

Let me give another illustration. I read the other day in the newspapers that the police of Paris have carried out an extremely interesting investigation. They have carefully ascertained the recognizable changes in the normal human organism produced by the prolonged pursuit of any particular occupation. The object was to obtain data for the identification of unknown dead bodies. The changes proved more numerous and characteristic than could have been supposed. They supplied, in fact, diagnostic marks by which the occupation of the individual could be accurately inferred. It seems to me impossible to have a more admirable case of the direct action of external conditions. I ask, Is there any reason to suppose that these acquired characters would be transmitted?

This appears to me an extremely plain issue, as it is certainly an extremely important one. There is not the least reluctance on the part of Darwinians to face it squarely. But the Duke appears to me to deliberately evade it.

(2) *Prophetic Germs.*—It seems to me that we are somewhat at cross-purposes. The Duke admits that I have correctly quoted him as saying: "All organs do actually pass through rudimentary stages in which actual use is impossible." When Prof. Lankester challenged the Duke to produce a single instance, he guarded himself by the remark: "The stages here alluded to are—if I understand correctly—ancestral stages, not stages in the embryological development of the individual." The Duke has never repudiated, as far as I am aware, that limitation of his meaning, if it be a limitation. And as he has

never responded to the challenge, I maintain that he has no right in a scientific discussion to reiterate a statement in support of which he has produced no definite observed evidence. He now returns the challenge to me. But it is no affair of mine. I simply take note of the fact that Prof. Lankester pointed out that the Duke's case collapsed unless the challenge was met, and that the Duke acquiesced by silence.

Just, however, as with the question of acquired characters, the Duke in defect of direct evidence now tries an *a priori* argument. He reminds us of the well known principle of embryology, sometimes called the recapitulation theory. Darwin states it in this form: the embryo is "a picture, more or less obscured, of the progenitor, either in its adult or larval state, of all the members of the same great class."

Now, of course, in the development of the individual organism, we have "a series of incipient structures on the rise for actual use," if by "on the rise" we mean in process of nutritive growth. This is, however, not necessarily true of the recapitulative structures which may or may not be temporarily utilized. When they are not so utilized they are mere survivals, and we know that survivals constantly so completely fall out of use, that by mere inspection it is often difficult to conceive what could have been their original function. I may give a single illustration. In flowering plants the homologue of the spore of the vascular cryptogams is still preserved. *Within* it, previous to fertilization, certain rudimentary structures are developed. It has been shown that these are the last recapitulative remnant of an independent series of structures developed *outside* the spore in the fern. In that type they form the prothallus, which possesses all the attributes of an independent organism, assimilates, respires, often reproduces itself asexually, and finally bears the sexual reproductive organs. All this in the flowering plant is not merely reduced to scarcely intelligible rudiments, but, in accordance with a well-known principle in embryology, it is thrown backwards in the order of development, and never emerges from the spore at all, instead of as in the fern being wholly external to and independent of it.

In this case we know the recapitulation and the thing recapitulated. We infer from their comparison that a fern-like plant was amongst the ancestry of the flowering plant. But I defy anyone, from a mere inspection of what happens in the latter, to form any idea of what happens in the former. From cases such as these it is obvious that the analogy between the development of the individual and the evolution of the race only holds for the broad facts of the sequence of stages, and does not give us any information as to the inutility of the structures of the ancestral organisms, or even, indeed, as to the precise period in their life when such structures made their appearance. The Duke's argument may now, I take it, be stated as follows:—

In the development of the individual organism, incipient organs are useless.

The development of the individual organism is a recapitulation of the evolution of the race.

∴ Incipient organs in the evolution of the race are useless.

I observe that the Duke's estimation of my logical powers is the reverse of flattering. I abstain, therefore, from criticizing this piece of reasoning. For my part I must confess I do not possess an *a priori* mind. No argument, however ingenious, is as convincing to me as accurately observed facts. If the Duke's convictions are laws of Nature, the objective verification ought to be forthcoming. W. T. THISELTON DYER.

Royal Gardens, Kew.

THE Duke of Argyll supports his assertion that "all organs do actually pass through rudimentary stages in which actual use is impossible" by reference to the stages of embryonic growth. Surely the assertion remains merely an empty repetition of the Darwinian position that the development of the embryo summarizes the morphological history of the race.

The modern dress coat has developed from a mere blanket, but even the useless parts of the modern coat can be easily shown to have had their use in some anterior forms of completed coat. The embryo, like the coat, preserves traces of evolutional stages at which what now appear useless characters were in reality actual useful characters.

What the Duke has to show is some instance of a completed organ in a completed organism, useless to that organism, not phases in the growth of an organ affording a blurred copy of some form of the organ existent at an anterior stage of the organism, and then useful to it. So far he has merely confounded ontogenal steps of growth with phylogenal phases of plan. F. V. DICKINS.

Burlington Gardens, February 3.

Eight Rainbows seen at the Same Time.

THE following letter which I have just received from Dr. Percival Frost of Cambridge, may interest your readers.

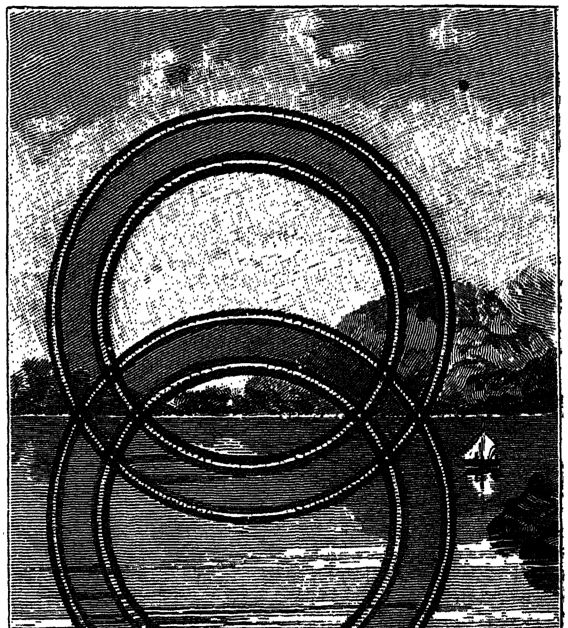
A statement that rainbows are produced not only by the sun itself directly, but by the image of the sun reflected from still water, is given in Prof. Tait's book on "Light." The phenomenon seems to have been observed by Halley in 1698 (see NATURE, vol. x. pp. 437, 460, and 483 for interesting correspondence on the subject).

The diffuse rainbow produced by the image of the sun reflected from a white cloud after sunset, described by Mr. Scouller, is, I believe, a novelty.

WILLIAM THOMSON.

The University, Glasgow, January 31.

IN NATURE (January 23, p. 271) you give a letter from Mr. Scouller describing an interesting case of a rainbow, due to the image of the sun in water, which, with the ordinary, primary, and secondary bows, make up (there being no secondary to that formed by the reflected sun) the *three* which he saw. Here is a short account of what I saw long ago, almost in prehistoric times, in Scotland, where such sights ought, according to your correspondent, to be very commonly seen. I may mention that I saw at the same time, lasting some five minutes, *eight* well-defined rainbows of one sort or another.



In 1841, during the time of a long vacation party, spent at Oban, I walked out with my brother to Dunstaffnage, and we were on the top of the Castle, somewhere between 3 and 4 p.m., on a day in the middle of August. Not a breath of wind, bright sun over, I think, Lismore Lighthouse, dusky clouds all over Ben Cruachan and Conoll Ferry; the sea in the bay (bounded by Dunstaffnage in the west) as smooth as a pond. Gradually there appeared before us the astonishing sight of the aforesaid *eight* distinct rainbows, viz. primary and secondary ordinary bows; primary and secondary bows by reflected sun; primary and

secondary bows formed by light from the real sun reflected from the water after leaving certain drops; primary and secondary formed by light from the sun reflected at the water, and, after leaving certain other drops, again reflected at the water. I have called the latter four distinct bows, because, although they looked like reflections of a solid set of four arcs, they were really formed by means of drops distinct from those which helped to make the first four bows. I append a sketch of what I saw.

PERCIVAL FROST.

15 Fitzwilliam Street, January 29.

[We have received other letters on the subject of Mr. Scouller's letter.]

Thought and Breathing.

I SEND you some extracts from the Sanskrit Yoga-sûtras which treat very fully of the prâ*n*âyâma, or the expulsion and retention of breath, as a means of steadying the mind.

A Yogi has first of all to assume certain postures which help him to fix his mind on certain objects. He cannot concentrate his mind while walking or running. He ought to assume a firm and pleasant position, one requiring little effort. To judge, however, from the description given of some of these postures, they would seem to us anything but pleasant.

When a Yogi has accustomed himself to his posture, he begins to regulate his breath—that is, he draws in the breath through one nostril, retains it for some time in the chest, and then emits it through the other nostril. The details of this process are given in the first chapter of the Yoga-sûtras, sûtra 37. Here the commentator states that the expulsion means the throwing out of the air from the lungs in a fixed quantity through a special effort. Retention is the restraint or stoppage of the motion of breath for a certain limited time. That stoppage is effected by two acts—by filling the lungs with external air, and by retaining therein the inhaled air. Thus the threefold prâ*n*âyâma, including the three acts of expiration, inspiration, and retention of breath, fixes the thinking principle to one point of concentration. All the functions of the organs being preceded by that of the breath—there being always a correlation between breath and mind in their respective functions—the breath, when overcome by stopping all the functions of the organs, effects the concentration of the thinking principle to one object.

Râjendralal Mitra, to whom we owe a very valuable edition of the text and translation of the Yoga-sûtras, adds the following remarks:—"All other Yogic and Tantric works regard the three acts of expiration, inspiration, and retention performed in specific order to constitute prâ*n*âyâma. The order, however, is not always the same. . . . The mode of reckoning the time to be devoted to each act is regulated in one of two ways: (1) by so many repetitions of the syllable om, or the mystic mantra (formula) of the performer, or the specific mystic syllables (vîja) of that mantra; (2) by turning the thumb and the index-finger of the left hand round the left knee a given number of times. The time devoted to inspiration is the shortest, and to retention the longest. A Vaish*n*ava in his ordinary daily prayer repeats the Vîja-mantra once while expiring, 7 times while inspiring, and 20 times while retaining. A Sâkta repeats the mantra 16 times while inspiring, 64 times while retaining, and 32 times while expiring. These periods are frequently modified."

The usual mode of performing the prâ*n*âyâma is, after assuming the posture prescribed, to place the ring-finger of the right hand on the left nostril, pressing it so as to close it, and to expire with the right, then to press the right nostril with the thumb, and to inspire through the left nostril, and then to close the two nostrils with the ring finger and the thumb, and to stop all breathing. The order is reversed in the next operation, and in the third act the first form is required. The Ha*th*adîpikâ says:—"By the motion of the breath, the thinking principle moves; when that motion is stopped, it becomes motionless, and the Yogi becomes firm as the trunk of a tree; therefore the wind should be stopped. As long as the breath remains in the body, so long it is called living. Death is the exit of that breath, therefore it should be stopped."

Some of the minor works on Yoga expatiate on the sanitary and therapeutic advantages of practising prâ*n*âyâma regularly at stated times. In America some spiritualistic doctors prescribe the same practice for curing diseases.

In India prâ*n*âyâma is only a means towards a higher object—namely, the abstraction of the organs from their natural functions. It is a preliminary to Yoga, which consists in *dhâra*n*â*, steadfastness, *dhyâna*, contemplation, and *samâdhi*, meditation, or almost a cataleptic trance. These three are supposed to impart powers or *siddhis* which seem to us incredible, but which nevertheless are attested by the ancient Yogis in a very *bonâ-fide* spirit, and deserve examination, if only as instances of human credulity. I say nothing of modern impostures.

Oxford, January 22. F. MAX MÜLLER.

IN connection with Prof. Leumann's recent researches into the relation between changes in respiration and changes in certain cerebral functions, it seems curious that the employment of deep and rapid respiration as an anæsthetic has received so little attention. Some dentists order their patients to respire as quickly and fully as they can for a period which varies, I believe, from four to six minutes, although as to the exact duration I am insufficiently informed. At the termination of this period the patient becomes giddy, and to a great extent loses consciousness, when a short operation can be painlessly performed. The patient, while unable to move his arms, opens his mouth at the order of the operator. I have heard of no casualties or evil effects from this mode of treatment. W. CLEMENT LEY.

Chiff-Chaff singing in September.

DURING more than forty years' observation of the singing of birds, I have invariably heard the chiff-chaff singing in September, although the song is much less frequently repeated than in the spring. In connection with this observation I may mention that both the male and female birds appear to be always mute for two or three days after their spring arrival in Northern Europe. W. CLEMENT LEY.

Lutterworth, January 31.

Foreign Substances attached to Crabs.

I HAVE read in recent numbers of NATURE some letters on sponges attached to crabs.

There are two crabs on the east coast of Australia—one of them allied to *Dromia vulgaris*—which cover themselves with sponges or with a composite Ascidian. I have in one case counted no less than seven species of sponges on one individual crab.

The Ascidian referred to is usually from ten to thirty times as large as the crab to the back of which it is attached.

Among the specimens brought by me from Australia, and now deposited in the National Collection of the British Museum, there are some of these crabs with sponges and Ascidians attached.

These might, perhaps, be interesting to your correspondents on the subject. R. V. LENDENFELD.

University, Innsbruck, January 25.

Foot-Pounds.

"A. S. E." will find moments, of resistance, of bending, or of turning, expressed in foot-pounds (often inch-pounds or foot-tons) in any treatise on civil, mechanical, or marine engineering, on architecture, land or naval, and, in fact, in every treatise on *real* mechanics he may consult. Why, then, should a different terminology be adopted in a Civil Service examination paper? In metric units, moments are given in kilogramme-metres or -centimetres; but in the C.G.S. system I do not suppose it is suggested to measure moments of dyne-centimetres in ergs.

February 3. A. G. GREENHILL.

IF "A. S. E." will push his researches further, he will find that in Government dockyards the stability moment on ships is calculated in foot-tons. V.

February 3.

PROF. WEISMANN'S THEORY OF HEREDITY.

IN NATURE of October 24, 1889 (p. 621), appeared a criticism by Prof. Vines of my essays on heredity and allied subjects. I should be glad to reply briefly to his objections, and the more so as I hope thus to be able to place the scientific problems at issue in a somewhat

clearer light. With regard to the immortality which I attribute both to the unicellular organisms and to the germinal cells of the multicellular, if I understand Prof. Vines aright, he does not attack the proposition itself, but has simply overlooked the explanation in my book of the way in which mortal organisms arose out of immortal in process of phyletic development, a process which must have taken place if the Protozoa have developed in the course of the world's history into the higher Metazoa,—"the first difficulty is to understand how the mortal heteroplastides can have been evolved from the immortal monoplastides." My explanation was simply that which appears to be the true one for the origin of every higher differentiation—namely, the division of the cell-mass of the Protozoan, on the principle of the division of labour, into two dissimilar halves, differing in substance, and consequently also in function; from the one cell which performed all functions comes a group of several cells which distribute themselves over the work. In my opinion, the first such differentiation produced two sets of cells, the one the mortal cells of the body proper, the other the immortal germ-cells. Prof. Vines certainly believes in the principle of the division of labour, and in the part that it has played in the development of the organic world, as well as I; but it seems to him that this division of a unicellular being into somatic and germinal cells is impossible, and that my explanation of the process by dissimilar division is inadequate, because it strikes him as "absurd to say that an immortal substance can be converted into a mortal substance."

There certainly does seem to be a great difficulty in this idea, but in reality it arises simply from a confusion of two conceptions—immortality and eternity. That the Protozoa and the germ-cells of Metazoa are in a certain sense immortal seems to me an incontrovertible proposition. As soon as one has clearly realized that the division of a monoplastid is in no way connected with the death of one part, there can be no further question that we have to do with individuals of indefinite duration; but this in no way implies that they possess an eternal duration; on the contrary, we imagine that they have all had a beginning. The conception of eternity, however, extends into the past as well as the future; it is without beginning or end, and does not affect the present question; it is an entirely artificial conception, and has no real and comprehensible existence; to express it more accurately, eternity is merely the negation of the conception of transitoriness. Of the objects with which natural science deals, none are eternal except the smallest particles of matter and their forces, certainly not the thousandfold semblances and combinations under which matter and force meet us. As I have said years ago, the immortality of unicellular organisms, and of the germ-cells of the multicellular, is not absolute but potential; it is not that they *must* live for ever as did the gods of the ancient Greeks—Ares received a "mortal" wound, and roared for pain like to ten thousand bulls, but could not die; they can die—the greater number do in fact die—but a proportion lives on which is of one and the same substance with the others. Does not life, here as elsewhere, depend on metabolism—that is to say, a constant change of material? And what is it, then, which is immortal? Clearly not the substance, but only a definite form of activity. The protoplasm of the unicellular animals is of such chemical and molecular structure that the cycle of material which constitutes life returns even to the same point and can always begin anew, so long as the necessary external conditions are forthcoming. It is like the circulation of water, which evaporates, gathers into clouds, and falls as rain upon the earth, always to evaporate afresh. And as in the physical and chemical properties of water there is no inherent cause for the cessation of this cycle, so there is no clear reason in the physical condition of unicellular organisms why the cycle of life, *i.e.* of division, growth by assimilation, and repeated division, should ever end; and this characteristic it is which I have termed immortality. It is the only true immortality to be found in Nature—a pure biological conception, and one to be carefully distinguished from the eternity of dead, that is to say unorganized, matter.

If then this true immortality is but cyclical, and is conditioned by the physical constitution of the protoplasm, why is it inconceivable that this constitution should be, under certain circumstances and to a certain extent, so modified that the metabolic activity no longer exactly follows its own orbit, but after more or fewer revolutions comes to a standstill and results in death? All living matter is variable; why should not variations in the protoplasm have also occurred which, while they fulfilled certain functions of the individual economy better, caused a metabolism which did not exactly repeat itself, *i.e.* sooner or later came to a condition of rest? I admit that I feel such a descent from immortality into mortality far less remarkable than the permanent retention of immortality by the monoplastids and germ-cells. Small, indeed, must be the variations in the complicated qualities of living matter to bring in their train such a fall; and very sharply must the essentials of its constitution be retained, for metabolism to take place so smoothly without creating in itself an obstacle to its own continuance! Even if we cannot penetrate into the mysteries of this constitution, still we may say that a rigorous and unceasing natural selection is unremittingly active in maintaining it at such an exact standard as to preserve its immortality; and every lapse from this standard is punished by death.

I believe that I have proved that organs no longer in use become rudimentary, and must finally disappear solely by "panmixie"; not through the direct action of disuse, but because natural selection no longer maintains their standard structure. What is true for an organ is true also for its function, since the latter is but the expression of the qualities of material parts, whether we can directly perceive their relations or not. If, then, as we saw, the immortality of monoplastids depends on the fact that the incessant metabolism of their bodies is ever returning exactly to its starting-point, and produces no such modifications as would gradually obstruct the repetition of the cycle, why should that quality of the living matter which causes immortality—nay, how *could* it be retained—when no longer necessary? It is obvious that it was no longer necessary in the somatic cells of the heteroplastids. From the instant that natural selection relaxed its watch on this quality of immortality began the process of panmixia which led to its abolition. Prof. Vines will ask, How can one conceive of this process? I answer, Quite easily. When once individuals arose among monoplastids, in the protoplasm of which occurred such variation in chemical and molecular constitution as to result in a gradual check on the metabolic cycle, it would happen that these individuals died; a permanent variety could not grow out of such variations. But if there arose among heteroplastids individuals with a similar differentiation of the somatic cells, the death of these cells would not be detrimental to the species, since its continuance is ensured by the immortal germ-cells. Upon the differentiation into germinal and somatic cells, natural selection was, speaking metaphorically, trained to bear on immortality of the germ-cells, but on quite other qualities in the somatic cells—on motility, irritability, capacity for assimilation, &c. We do not know whether the attainment of these qualities was accompanied by a constitutional alteration which caused the loss of immortality, but it is at least possible; and, if true, the somatic cells will have lost their immortality even more rapidly than through the unaided action of panmixia.

In the fourth essay of my book, I have cited the two Volvocinean genera Pandorina and Volvox as examples

of the differentiation of homoplastids into the lowest heteroplastids; in Pandorina the cells are still all alike and all perform the same functions, in Volvox occur somatic and germinal cells, and in the latter case we should expect to find the commencement of natural death. Recent researches of Dr. Klein ("Morphologische und biologische Studien über die Gattung Volvox," *Jahrb. wiss. Botan.*, xx., 1889) show that this is actually the case; as soon as the germ-cells are ripe and emerge from the sphere, the ciliated somatic cells begin to shrivel up, and die in one or two days. This is the more interesting, as the somatic are also the nutritive cells; for, though the germ-cells also possess chlorophyll, the rapid growth of the latter (which attain an enormous size in Volvox) is only possible by the supply of nourishment from the somatic cells. The latter are so constituted that they assimilate, but cannot grow larger when once the sphere has reached its definite size; they transfer the nourishment which they derive from the decomposition of carbon dioxide, &c., to the germinal cells by means of fine pseudopodia; and themselves wither when once the germs are ripe. In this case adaptation to the nutrition of the germinal cells might well have accelerated the introduction of a natural death of the somatic cells, the capacity for considerable assimilation combined with a drain on their nutrition may have led after a certain time to stoppage of the process of assimilation and to death. To me, the idea that modification of the living matter may have been connected with loss of immortality does not appear more unlikely or more difficult than the generally received view of the gradual differentiation of the somatic cells in the course of phylogeny into their various species of digestive, secretive, motile, and nervous cells. An immortal unalterable living substance does not exist, but only immortal *forms of activity* of organized matter.

I maintain, therefore, in its entirety, my original statement, that monoplastids and the germ-cells of higher forms have no natural death. I do not know how this can to-day be better expressed than by saying that these living units possess a real and actual immortality as against the imaginary ideal immortality of the Greek gods. If death from internal causes does not exist for them, one may yet say with certainty that the fatal hour will one day strike for them all, not from internal causes, but because the external conditions for the constant renewal of vital activity will some day cease. The physicists prophesy that the circulation of water on the globe will end, not from any alteration in the qualities of water, but because external conditions will render this form of motion of aqueous particles impossible.

Prof. Vines then attacks my view of embryogeny. He finds it "not a little remarkable that Prof. Weismann should not have offered any suggestion as to the conception which he has formed of the mode in which the conversion of germ-plasm into somatoplasm can take place, considering that this assumption is the key to his whole position." He sees here the same difficulty as in the phyletic development, and says: "There is really no other criticism to be made on an unsupported assumption such as this, than to say that it involves a contradiction in terms." He means by this that the eternal cannot pass into the finite, as must be the case if the immortal germ-cell grow into the mortal soma. At the bottom of this objection lies the same confusion between immortality and eternity which has already been made clear. I do not wish to reproach Prof. Vines with this obscurity, as I felt the same objection myself for many years, and could not at once discover the reply to it; on the contrary, I am indebted to him for the opportunity to express myself on the point. Up to this time we have had no scientific conception of immortality; if this be accepted, the significance of immortality is not life without beginning or end, but life which, after its first commencement, can continue indefinitely with or without modification (specific changes in the germ-plasm or the monoplastids); it is a cyclical activity of organic material devoid of any intrinsic momentum which would lead to its cessation, just as the motion of the planets contains no intrinsic momentum which would lead to its cessation, although it has had a commencement and will some day, through the operation of extrinsic forces, have an end.

Prof. Vines says later: "I understand Prof. Weismann to imply that his theory of heredity is not—like, for instance, Darwin's theory of pangenesis—a provisional or purely formal solution of the question, but one which is applicable to every detail of embryogeny, as well as to the more general phenomena of heredity and variation." I have, as a matter of fact, designated Darwin's pangenesis as a "purely formal" solution of the question, but should like here to give a slight explanation of the expression, as I fear that not only Prof. Vines, but also many other readers of my essays, have misunderstood me. On the one hand, I am afraid that they see in my words a definite reproach against Darwin for his theory of pangenesis, of which I had not the remotest intention; and on the other, that they incline to charge me with too great an affection for my own theory.

I believe there are two kinds of theory; one may term them the "real" and the "ideal"; practically they are rarely sharply to be discriminated; both often occur in one and the same theory, but should be conceived of separately. The "ideal" theories attempt to render conceivable the phenomena to be explained by an arbitrarily accepted principle, apart from the question whether the principle itself possesses any grain of truth or not; they seek only to show that there are hypotheses on which the phenomena in question become comprehensible. "Real" theories do not make hypotheses at pleasure, but strive to construct such as have some degree of probability; they desire to give not a formal, but, if possible, the right explanation. Sir William Thomson in endeavouring to make clear the dispersion of rays of light, never believed in the remotest degree that such molecules as he pictured really existed, but desired merely to show that there were hypotheses on which the phenomena of dispersion were comprehensible. Darwin's pangenesis was originally intended in this sense, and was by him termed a "provisional" hypothesis, although in later years he may have attributed to it the weight of a real theory. To me his "gemmules" are a pure invention, an invention in no way corresponding to the actual facts, but showing what hypotheses must be made in order to explain the phenomena of heredity. Are, however, such ideal theories worthless? Certainly not. They are often the first and essential step towards the understanding of complicated phenomena, and lay the foundation for the gradual erection of a real theory. It would perhaps never have occurred to me to deny the inheritance of acquired characters, had not Darwin's pangenesis shown me that the matter was only explicable on an hypothesis so difficult to conceive, as that of the giving off, circulation, and reassemblage of gemmules. I do not even now maintain that Darwin's pangenesis cannot possibly contain a kernel of truth; De Vries ("Intracellulare Pangenesis," Jena, 1889) has shown in a recent and most interesting memoir that the ideal impossible pangenesis may be transformed into a real and possible one by means of certain profound modifications; he accepts my view that acquired (somatogenic) modifications cannot be transmitted, and thereby puts on one side just that part of Darwin's theory which has always appeared to me to lie beyond the pale of reality—namely, the circulation, &c., of the gemmules. The future will show whether his view of modified gemmules or my hypothesis is the best explanation of the facts of heredity.

In any case, I am far from assuming that I have settled the whole question of heredity; I have undertaken researches on some of the more important parts of the

problem, and have thus been compelled to formulate some fundamental principles for the explanation of the phenomena; but no one can be more convinced than I how far we are from a definite and complete explanation, not only of "every detail," but also of "the more general phenomena." My endeavour was to put forth a real, in place of the previous ideal, theory; and on this ground I took pains to make only such suppositions as might possibly correspond to actual facts. There certainly is a material carrier of heredity in the ovum; it certainly can be transported from nucleus to nucleus; it certainly can be modified in the process, or can remain the same; and even the supposition that it is able to stamp its own character on the cell contains nothing which seems to us impossible and non-existent; on the contrary, we are able now to state that it is so, even if we do not understand in what wise it happens. My hypothesis relative to the quiescent state of germ-plasma also rests on a basis of fact; we know that ancestral characteristics may be transmitted in a latent condition, and that the process of transmission is bound up with a substance, the idioplasma; there must therefore actually be an inactive stage of idioplasma.

If it could be shown that upon such principles an explanation of heredity is attainable, we should have made a distinct advance upon the ideal theory of pangenesis which is founded on unreal hypotheses. Possibly it is upon the path which I have opened up that we shall gradually attain a satisfactory solution of the numerous questions at issue; possibly further research will show that it is not the right path, and must be abandoned; no one, it appears to me, can foretell this. My reflections on heredity are not a conclusion, but a commencement—no complete theory of heredity which claims to provide a complete solution of all the problems at issue, but *researches* which, if fortunate, may sooner or later, by direct or circuitous paths, lead to a true appreciation of the question, to a "real" theory. In the preface to the English edition of my "Essays" I have stated this expressly.

I have also in that place distinctly insisted that the book was not written as a whole; that it consists rather of a series of researches, the one growing out of the other, and showing the development of my views as they shaped themselves during the course of nearly a decade's work. It is therefore unreasonable to extract ideas from an earlier essay and apply them against a later one. I have left them unaltered, and even "left certain errors of interpretation uncorrected," because, if altered, their internal connection could not have been understood.

I believe that the objections which Prof. Vines makes to my theory of the continuity of germ-plasma rest solely on an unintentional confusion of my ideas, as he compares the opinions expressed in the second essay with those of the later ones, with which they do not tally. I will endeavour to make this clear. In this second essay (1883) I contrasted the body (soma) with the germ-cells, and explained heredity by the hypothesis of a "Vererbungs-substanz" in the germ-cells (in fact the germ-plasma), which is transmitted without breach of continuity from one generation to the next. I was not then aware that this lay only in the nucleus of the ovum, and could therefore contrast the entire substance of the ovum with the substance of the body-cells, and term the latter "somatoplasm." In Essay IV. (1885) I had arrived, like Strasburger and O. Hertwig, at the conviction that the nuclear substance, the chromatin of the nuclear loops, was the carrier of heredity, and that the body of the cell was nutritive but not formative. Like the investigators just named, I transferred the conception of idioplasma, which Nägeli had enunciated in essentially different terms, to the "Vererbungs-substanz" of the ovum-nucleus, and laid down that the nuclear chromatin was the idioplasma not only of the ovum but of every cell, that it was the dominant cell-element which impressed its specific character upon the originally indifferent cell-mass. From then onwards, I no longer designated the cells of the body simply as "somatoplasm," but distinguished, on the one hand, the idioplasm or "Anlagen-plasma" of the nucleus from the cell-body or "Cytoplasma," and, on the other, the idioplasm of the ovum-nucleus from that of the somatic cell-nucleus; I also for the future applied "germ-plasm" to the nuclear idioplasm of ovum and spermatozoon, and "somatic idioplasm" to that of the body-cells (*e.g.* p. 184). The embryogenesis rests, according to my idea, on alterations in the nuclear idioplasma of the ovum, or "germ-plasm"; on p. 186, *et seqq.*, is pictured the way in which the nuclear idioplasm is halved in the first cell-division, undergoing regular alterations of its substance in such a way that neither half contains all the hereditary tendencies, but the one daughter-nucleus has those of the ectoblast, the other those of the entoblast; the whole remaining embryogenesis rests on a continuation of this process of regular alterations of the idioplasma. Each fresh cell-division sorts out tendencies which were mixed in the nucleus of the mother-cell, until the complete mass of embryonic cells is formed, each with a nuclear idioplasm which stamps its specific histological character on the cell.

I really do not understand how Prof. Vines can find such remarkable difficulties in this idea. The appearance of the sexual cells generally occurs late in the embryogeny; in order, then, to preserve the continuity of germ-plasm from one generation to the next, I propound the hypothesis that in segmentation it is not *all* the germ-plasm (*i.e.* idioplasm of the first ontogenetic grade) which is transformed into the second grade, but that a minute portion remains unaltered in one of the daughter-cells, mingled with its nuclear idioplasm, but in an inactive state; and that it traverses in this manner a longer or shorter series of cells, till, reaching those cells on which it stamps the character of germinal cells, it at last assumes the active state. This hypothesis is not purely gratuitous, but is supported by observations, notably by the remarkable wanderings of the germinal cells of Hydroids from their original positions.

But let us neglect the probability of my hypothesis, and consider merely its logical accuracy. Prof. Vines says:—"The fate of the germ-plasm of the fertilized ovum is, according to Prof. Weismann, to be converted in part into the somatoplasm [!] of the embryo, and in part to be stored up in the germ-cells of the embryo. This being so, how are we to conceive that the germ-plasm of the ovum can impress upon the somatoplasm [!] of the developing embryo the hereditary character of which it (the germ-plasm) is the bearer? This function cannot be discharged by that portion of the germ-plasm of the ovum which has become converted into the somatoplasm [!] of the embryo *for the simple reason that it has ceased to be germ-plasm*, and must therefore have lost the properties characteristic of that substance. Neither can it be discharged by that portion of the germ-plasm of the ovum which is aggregated in the germ-cells of the embryo, for under these circumstances, it is withdrawn from all direct relation with the developing somatic cells. The question remains without an answer." I believe myself to have answered this above. I do not recognize the somatoplasm of Prof. Vines; my germ-plasm or idioplasm of the first ontogenetic grade is not modified into the somatoplasm of Prof. Vines, but into idioplasm of the second, third, fourth, hundredth, &c., grade, and every one impresses its character on the cell containing it.

Prof. Vines also attacks my view of the idioplasmatic nature of the *nuclear* substance (the chromatic grains); and maintains that it is as easy to speak of the continuity of the cell-body as of that of the nuclear substance, and that the one may transmit heritable qualities to progeny as well as the other. I quite understand that a botanist may easily be led to this view; and Prof. Vines is not the

only one to hold it. Waldeyer ("Ueber Karyokinese und ihre Beziehung zu den Befruchtungs-vorgänge," *Arch. mikr. Anat.*, xxxii., 1888) has considered the observed facts insufficient to justify the regarding of the nuclear loops as idioplasm; Whitman ("The Seat of Formative and Regenerative Energy," Boston, 1888) among zoologists has expressed himself against this view, and the same occurs in the recent book of Geddes and Thomson ("The Evolution of Sex," London, 1889). The facts which led me to the idea that the nuclear threads were the real carriers of heredity—were, in fact, the idioplasma—are enumerated in Essay IV.; they were primarily the observations of E. van Beneden on the phenomena of fertilization in the ovum of *Ascaria megalocephala*, those of Strasburger on fertilization in the Phanerogams by a mere nucleus, and the researches of Nussbaum and Gruber on division in the Infusoria. One may further cite as of essential importance the facts of karyokinesis *per se*, and the circumstance that, only on the supposition that the nucleus contains the idioplasma can the extrusion of polar bodies from the animal ovum be rendered comprehensible. The latter process divides the nuclear substance of the ovum into two quantitatively equal halves, but the body of the ovum into two unequal halves, the size of which is different in every species. The essential part of the process must therefore be the division of the nuclear substance, not that of the cell-mass. These facts on reflection so completely convinced me that the nucleus alone acts as carrier of hereditary tendencies, that the theory of the physiological equality of the nuclei of the sexual elements which I had propounded ten years before (1873) struck me as a certainty; and I then advanced the theory of fertilization which is contained on p. 246 of Essay IV. I believe that till recently Strasburger and I alone had expressed similar views of the essence of fertilization, at least so far as relates to the homodynamy of the sexual nuclei. That most distinguished observer, E. van Beneden, who has won such renown in the investigation of the process of fertilization, took his stand with regard to its theoretical significance on the platform of the older view, which regarded it as the union of two elements intrinsically and essentially the opposite of each other. He could not free himself from that dominant and deeply rooted idea, that the difference between the sexes is something fundamental, an essential principle of existence. The fertilized oosperm is in his eyes a hermaphrodite object, uniting in itself both male and female essences, an idea in which many other observers (cf. Kölliker, "Die Bedeutung der Zellenkerne für die Vorgänge der Vererbung," *Zeit. wiss. Zool.*, xlii., 1885) have followed him, and of which the logical sequence is that all the cells of the body are to be regarded as hermaphrodite!

Van Beneden was also influenced by the idea which sways the naturalists of so many countries, that fertilization is a process of rejuvenescence, in the sense that without it life cannot be prolonged to the end. Many still hold to this idea; Maupas ("Recherches expér. sur la multiplication des infusoires ciliés," *Arch. zool. exp. gén.*, (2) vi. p. 165) very recently believed that he had found a proof of its correctness, and attempted to show that Infusoria, for a continuance of existence, must from time to time enter into conjugation, or die from internal causes if this conjugation be prevented. Even were his observations correct, they would still fall short of proving his conclusions; they would prove nothing against the immortality of the Protozoa, or for a rejuvenescence in the sense here intended; they would rather state the platitude that ovum and spermatozoon must die, if the condition of their continued existence, namely fusion, inevitable in most species of plants and animals, be prohibited; but this is an accidental, not a natural, death. Richard Hertwig ("Ueber die Conjugation der Infusorien," München, 1889) has also briefly shown that the facts, on which Maupas bases his inference, are not universally true; that Infusoria hindered from conjugation do not die, but increase by division, and may produce whole colonies of animals—nay, that they are generally thus rendered abnormally prolific.

I am distinctly opposed to the rejuvenescence theory, whether applied to unicellular or multicellular organisms; my view is expressed in Essay IV., and may be summarized in this position—we should no longer speak of the conjugating nuclei of the sexual elements as male and female, but as *paternal* and *maternal*, there is no opposition of the one to the other, they are essentially alike, and differ only so far as one individual differs from another of the same species. Fertilization is no process of rejuvenescence, but merely a union of the hereditary tendencies of two individuals; tendencies which are bound up with the matter of the nuclear loops; the cell-body of the ovum and spermatozoon is indifferent in this connection, and plays merely the part of a nutritive matter which is modified and shaped by the dominant idioplasm of the nucleus in a definite way, as clay in the sculptor's hand. The different appearance and function of ovum and spermatozoon, and their mutual attraction, rest on secondary adaptations, qualified to ensure that they shall meet and that their idioplasmata shall come into contact, &c.; and as with the cells, so the differentiation of *persons* into male and female is also secondary; all the numerous differences of form and function which characterize sex in the higher animals, the so-called "secondary sexual characters," which reach even into the highest spiritual regions of mankind, are nothing but adaptations to ensure the union of the hereditary tendencies of two individuals.

These are briefly the views of fertilization which I have indicated since 1873, but have only published in a finished and definite shape since the discovery by van Beneden of the morphological processes in the fertilization of the ovum of Ascaris (Essay IV., 1885). I concluded then with these words:—"If it were possible to introduce the female pro-nucleus of an egg into another egg of the same species, immediately after the transformation of the latter into the female pro-nucleus, it is very probable that the two nuclei would conjugate just as if a fertilizing sperm-nucleus had penetrated [the ovum]. If this were so, the direct proof that egg-nucleus and sperm-nucleus are identical would be furnished. Unfortunately the practical difficulties are so great that it is hardly possible that the experiment can ever be made; but such want of experimental proof is partially compensated by the fact, ascertained by Berthold, that in certain Algæ (Ectocarpus and Scytosiphon) there is not only a female, but also a male parthenogenesis; for he shows that in these species the male germ-cells may sometimes develop into plants, which however are very weakly."

I have since attempted to fertilize one frog's egg with the nucleus of another; the experiment was, as one would expect, not successful, owing to the enormous havoc caused by introducing a cannula into the egg; but Boveri ("Ein geschlechtlich erzeugter Organismus ohne mütterliche Eigenschaften," *Ges. Morph. Physiol. München*, 16 Juli, 1889) was more fortunate, in finding an object which allowed of the converse experiment to mine; following Hertwig's example, he removed the nucleus from an Echinoid ovum by agitation, and brought such denucleated ova to develop by introducing spermatozoa. From the spermatozoan nucleus was formed a regular segmentation-nucleus, the embryogenesis pursued its regular course, and there was formed a complete though small free-swimming larva, which lived for a week. From this experiment alone it follows that the views of Strasburger and myself on fertilization are correct, *viz.* that the sperm-nucleus can play the part of ovum-nucleus and *vice versâ*, and the older view, to which Prof. Vines ("Lectures on the Physiology of Plants," Cambridge, 1886, pp. 638-681) has also sworn allegiance, must be given up.

An interesting and important modification of Boveri's experiment confirmed both this experiment, and also, if it were necessary, the recognition of the nuclear substance as idioplasm, as maintained by O. Hertwig, Strasburger, and myself. If eggs of *Echinus microtuberculatus*, when artificially deprived of their nuclei, be fertilized with the spermatozoa of *Sphærechinus granulatus, larvæ are developed with the true characters of the second species*—that is to say, they have derived everything from the father, nothing from the mother; the nuclear substance alone it is which transmits heredity, and by it the cell-mass is dominated.

I have interpreted the first polar body of the Metazoan ovum as a carrier of ovogenous plasm, which has to be removed from the ovum in order that the germ-plasm may attain the predominance. It is possible that this explanation is not correct; the most recent researches on the conjugation of Infusoria, as expressed in the splendid memoirs of Maupas and R. Hertwig, argue against my interpretation; but the idea which lay at the bottom of this explanation is justified. As it is the nuclear matter which gives to the cell-body its specific character, the ovum must, previous to fertilization, be dominated by a different idioplasm to the sperm-cell, since they are, up to this point, different in appearance and function. On the other hand, when they have united, they contain the same idioplasm—namely, germ-plasm; the consequence is that the first dominant idioplasm is different to that of a later period. This was the idea at the bottom of my explanation of the first polar body, and it is correct. One might perhaps imagine that the idioplasmata of ovum and spermatozoon were originally different, but that both possessed the power of alteration into germ-plasm; but it would be then incomprehensible why parthenogenetic ova should expel one polar body. Both facts, however, are explicable, if ovum and spermatozoon are dominated up to the period of maturation by different histogenetic idioplasmata with which a small quantity of germ-plasm is mingled, and if at a later period the former be removed and the germ-plasm come to rule in both cells. This process would be by no means abnormal and unparalleled, since entirely analogous divisions of the idioplasm into qualitatively dissimilar portions must occur hundreds of times in every embryogenesis. However, I am most willing to allow that the last word has not yet been said on this question, and would only maintain that my theory of heredity is not concerned thereby. It is not the interpretation of the first polar body, but that of the second, which is decisive; and one can none the less easily think of the latter as a halving of the number of ancestral germ-plasmata, even if it be proved that my explanation of the first polar body was erroneous. I would then express the first division merely as introductory to the second, as the necessary first step in the reduction of ancestral plasmata, the necessity for which we should thus perhaps learn to understand.

The regular modification of idioplasma during the ontogeny, which I have maintained and which so many have attacked (Kölliker[1] with special vehemence) will now stand out as justified. If the nucleus of a sperm-cell is capable of impressing on the denucleated mass of an ovum its own inherited tendencies, and of calling into being an organism with specific characteristics purely paternal, it will be found difficult to explain the ontogeny otherwise than as a regular modification of the idioplasm, continuous from one cell-division to another, which stamps on the body of each separate cell at each stage its peculiar character, not only with regard to shape but also to function, and especially with regard to the "rhythm" of cell-division.

[1] "Das Karyoplasma und die Vererbung: eine Kritik der Weismann'sche Theorie von der Continuität des Keimplasma's," *Zeit. wiss. Zool.*, xliv. p. 228, 1886.

A further objection is directed by Prof. Vines against my views on the origin of variation. In the fifth essay I have sought the significance of sexual reproduction in the fact that it alone could have called into existence that multiplicity of form of the higher animals and plants, and that constantly fluctuating union of individual variations, of which natural selection stood in need for the creation of new species. I am still of the opinion that the origin of sexual reproduction depends on the advantage which it affords to the operation of natural selection; nay, I am completely convinced that only through its introduction was the higher development of the organic world possible. Still, I am at present inclined to believe that Prof. Vines is correct in questioning whether sexual reproduction is the *only* factor which maintains Metazoa and Metaphyta in a state of variability. I could have pointed out in the English edition of my "Essays" that my views on this point had altered since their publication; my friend Prof. de Bary, too early lost to science, had already called my attention to those parthenogenetic Fungi which Prof. Vines justly cites against my views; but I desired, on grounds already mentioned, to undertake no alteration in the essays. Bessides, I was well aware when the essay was first committed to paper (1886) that my current view on the radical cause of variation was possibly incomplete; and so, in order to expose the truth of the view as far as possible to a general test, I drove its logical consequences home, and enunciated the statement that species reproducing parthenogenetically could not be modified into new species. I also began myself at that time experiments on the variation of parthenogenetic species which are still being continued, and on which on some future occasion I hope to be able to report.

Even if, however, from our present knowledge it is probable that sexual reproduction is not the sole radical cause of variability of the Metazoa, still no one will dispute that it is a most active means of heightening variations and of mingling them in favourable proportions. I believe that the important part which this method of reproduction has played in calling out the existing processes of selection, is hardly diminished, even if one grants that direct influences upon the idioplasm call forth a portion of individual variability. Prof. Vines even holds it probable "that the absence of sexuality in these plants [Fungi] may be just the reason why no higher forms have been evolved from them, for in this respect they present a striking contrast to the higher Algæ in which sexuality is well marked." But when Prof. Vines says, "there can be no doubt that sexual reproduction does very materially promote variation," he does not mean to say that this is a self-evident proposition; he is well aware that prominent investigators like Strasburger see in sexual reproduction the reverse action, that of maintaining the constancy of the specific character. But I gladly accept his agreement with my view, which confirms the main position of the fifth essay, which runs: Sexual reproduction has arisen by and for natural selection as the sole means by which individual variations can be united and combined in every possible proportion.

With reference also to the problem of the inheritance of acquired (somatogenic) characters, Prof. Vines is again my opponent; he holds that such inheritance is possible. I have denied it, because it did not appear to me self-evident—as was formerly universally assumed—but rather utterly unproven; and because I think that completely unfounded assumptions of such far-reaching consequence should not be made, when requiring a large number of improbable hypotheses for their explication. I have tested all the available evidence for such inheritance as accurately as I could, and have found that none has the value of proof. There is no inheritance of mutilations, and this constitutes up to now the only basis of fact for the supposition of the inheritance of somatogenic variations. If, in the last essay, I have not denied every

possibility of such a transmission, Prof. Vines should interpret that in my favour, not to my discredit; it is not the business of an investigator to set forth a proposition, which on the existing evidence he is compelled to believe, as an infallible dogma. Prof. Vines finds my "statements of opinion so fluctuating that it is difficult to determine what [my] position exactly is," but he could have easily discovered my meaning, if, instead of promiscuously contrasting the eight essays and the eight years of their production, he had merely brought the last of them to the bar of judgment. This essay is especially concerned with "the supposed transmission of mutilations," and at its conclusion my verdict on the state of the problem of the inheritance of acquired characters is thus summarised:—"The true decision as to the Lamarckian principle [lies in] the explanation of the observed phenomena of transformation. . . . If, as I believe, these phenomena can be explained without the Lamarckian principle, we have no right to assume a form of transmission of which we cannot prove the existence. Only if it could be shown that we cannot now or ever dispense with the principle, should we be justified in accepting it." The distinguished botanist De Vries has proved that certain constituents of the cell-body, *e.g.* the chromatophores of Algæ, pass directly from the maternal ovum to the daughter-organism, while the male germ-cell generally contains no chromatophores. Here it appears possible that a transmission of somatogenic variation has occurred; in these lower plants, the separation between somatic and reproductive cells is slight, and the body of the ovum does not require a complete chemical and physical alteration to become the body of the somatic cell of the daughter. But how does this affect the question whether, for instance, a pianoforte player can transmit to his progeny that strength of his finger-muscles which he has acquired by practice? How does this result of practice arrive at the germ-cells? In that lies the real problem which those have to solve who maintain that somatogenic characters are transmissible.

It is proved by the observations of Boveri, quoted above, that among animals the body of the ovum contributes nothing to inheritance. If the transmission of acquired characters should take place, it would have to be by means of the nuclear matter of the germ-cells—in fact, by the germ-plasm, and that not in its patent, but in its latent condition.

To renounce the principle of Lamarck is certainly not the way to facilitate the explanation of the phenomena; but we require, not a mere formal explanation of the origin of species of the most comfortable nature, but the real and rightful explanation. We must attempt, therefore, to elucidate the phenomena without the aid of this principle, and I believe myself to have made a beginning in this direction. A short time ago I tried this in one of those cases where one would least expect to be able to dispense with the principle of modification by use—namely, in the question of artistic endowment.[1] I proposed to myself the question whether the musical sense of mankind could be conceived of as arising without a heightening of the original acoustic faculty by use. But even here I came to the conclusion that, not only do we not need this principle, but that use has actually taken no part in the development of the musical sense.

A. WEISMANN.

THE LIFE AND WORK OF G. A. HIRN.

THE three men who worked at the experimental determination of the mechanical equivalent of heat and at practical Thermodynamics have passed away within a few months of each other—Clausius, Joule, and now Hirn.

[1] "Gedanken über Musik bei Thieren und bei Menschen," *Deutsche undschau*, October 1889.

They were much of the same age, and began their experiments while young at almost the same time; and the practical agreement of the conclusions drawn from their experimental results is our best guarantee of confidence in the modern theory of Thermodynamics which is built upon these results.

Gustave Adolphe Hirn was born at Logelbach, in Alsace, on August 21, 1815, and died on January 14 of this year, a victim to the prevailing epidemic of influenza; but for this, we might have expected still further developments of his scientific theories, as he continued at work on his favourite subjects to the last.

Self-taught, so far as his scientific education was concerned, he found himself, with his elder brother Ferdinand, a manager of the works of Haussman, Jordan, and Co., an establishment for the fabrication of *indiennes*, established in 1772. Finding the machinery antiquated and worn out, Hirn, in setting to work to make the best of it, was really better placed for theorizing and experimentalizing than if he had charge of modern works in first-rate order. The different parts of the works being at a distance from each other, his brother Ferdinand brought out his system of cable transmission of power; and it was Gustave who pointed out theoretically the advantage of a thin light cable run at a high speed.

Hirn also turned his attention to the important economic question of the lubrication of machinery, and upset the previous prejudice against the use of mineral oil for this purpose. He also demonstrated experimentally that, while the old laws of friction enunciated by Morin were sufficiently accurate for the contact of one dry metal against another, these laws are powerfully modified when the surfaces are well lubricated, as with machinery. Now the friction varies as the square root of the pressure, and as the surface and the velocity; so that the theory falls in with that of the viscous flow of liquids. These laws have received confirmation of recent years by the experiments carried out under the auspices of the Institution of Mechanical Engineers.

But it is chiefly for his experiments on a large scale on the steam-engines under his charge that Hirn is best known, and from his varied methods of determining the mechanical equivalent of heat by the friction of metals on metal or water, and finally from observation of the amount of heat consumed by the steam-engine, when every source of gain or loss is carefully followed up.

With this object he investigated experimentally the separate effects of conduction, of jacketing, of initial condensation in the cylinder, and of its prevention by superheating.

If we watch the performance of a modern marine triple-expansion engine, we notice that the high-pressure cylinder appears choked with water from initial condensation, while the intermediate and low-pressure cylinders work comparatively dry. It was considered in the early days of compound engines that this initial condensation was a source of great loss, and superheating was introduced to minimize it. But the superheated steam ruined the packings, and dried up the lubricant, so that the superheater was found practically to do more harm than good. A characteristic story is told of John Elder, the pioneer of compounding in modern marine engines, too long to insert here, which bears on this point.

Nowadays this initial condensation is looked upon as inevitable, and as not really so uneconomical as the books make out, when attendant advantages are considered; but to the theorist such as Hirn this condensation was something to be avoided at any cost, and he worked hard to make its prevention feasible.

Hirn was a man of varied reading, taste, and pursuits, and he worked into his treatises on his favourite subject of Thermodynamics a good deal of speculative metaphysics, which make his books rather curious reading sometimes to modern tastes, and we must go back to the

time of Descartes and Leibnitz, when physical science and moral philosophy went hand in hand, to find an equivalent.

But it must be allowed that the science of Thermodynamics may be treated with advantage from this double point of view; for, after its First Law has been established, that heat and work are equivalent and interchangeable, the rate of exchange being fixed by the mechanical equivalent of Joule and Hirn, when we come to the Second Law, named after Carnot, we are compelled to secure conviction of its truth by an appeal to the arguments of analogy and metaphysics.

Hirn spent the last years of his life at Colmar, in the society of a few congenial friends, much interested in metaphysics and meteorology, but cut off from his native France by international strained relations.

In this age of practical Thermodynamics his work will not be lost sight of; but we are still far from a complete reconciliation of the abstract theories of the books and the observed realities of practice.

A. G. GREENHILL.

NOTES.

THE Croonian Lecture, which will be delivered before the Royal Society on February 27 by Prof. Marshall Ward, will be on "The Relations between Host and Parasite in certain Epidemic Diseases of Plants."

ON Thursday last the Astronomer-Royal was elected by ballot to fill the place of the late Father Perry upon the Council of the Royal Society.

METEOROLOGISTS will be sorry to hear of the death of Prof. C. H. D. Buys-Ballot, on Sunday last. He was born in 1817, and had been Director of the Meteorological Institute, Utrecht, for more than 30 years.

DR. DAVID SHARP, the eminent entomologist, and late President of the Entomological Society of London, has accepted the appointment of Curator in Zoology in the Museum of the University of Cambridge, rendered vacant by the resignation of the Rev. A. H. Cooke, whose labours on the Macandrew Collection in that Museum have been so highly appreciated by conchologists.

SIR WILLIAM GULL, F.R.S., was so distinguished a physician, and his name was so well known, that the tidings of his death excited a widespread feeling of regret. He died on Wednesday, January 29, from paralysis, and the funeral took place on Monday at the churchyard of Thorpe-le-Soken, Essex. He was in his seventy-fifth year.

WE regret to hear of the death of Dr. L. Taczanowski, which took place at Warsaw on January 11. He is best known for his standard work "Ornithologie du Pérou," but his contributions to the ornithology of Poland, of Siberia, and the Corea have also been numerous and important.

GERMAN papers announce the death of Otto Rosenberger, the well-known astronomer. He was born in Courland in 1810, and in 1831 was appointed to the charge of the Observatory at Halle, and at the same time was made Professor of Mathematics. This position he held during the rest of his long life. Rosenberger's name is known chiefly in association with his work relating to Halley's comet.

ANOTHER death which we are sorry to have to record is that of Prof. Neumayr, the geologist, of Vienna. He was only a little over forty years of age, and his death is a great loss.

ON February 15, Lord Rayleigh will begin a course of seven lectures at the Royal Institution. The subject will be electricity and magnetism.

THE Council of the Society of Arts have arranged that a course of lectures on "The Atmosphere" shall be given by Prof. V. Lewes on the following Saturday afternoons: March 8, 15, 22, and 29, at 3 o'clock.

MR. B. A. GOULD, Cambridge, Mass., has been appointed President of the American Metrological Society for the present year. Among the members of the Council of this Society are Messrs. Cleveland Abbe, H. A. Newton, Simon Newcomb, and S. P. Langley. The Society was founded in 1873, and its objects are to improve existing systems of weights, measures, and moneys, and to bring them into relations of simple commensurability with each other; to secure the universal adoption of common units of measure for quantities in physical observation or investigation, for which ordinary systems of metrology do not provide; to secure uniform usage as to standard points of reference, or physical conditions to which observations must be reduced for purposes of comparison; and to secure the use of the decimal system for denominations of weight, measure, and money derived from unit-bases, not necessarily excluding for practical purposes binary or other convenient divisions.

THE Committee of the Cambridge University Antiquarian Society in their fifth Annual Report state that, since the opening of the Archæological Museum in 1884, over 2800 objects and 900 books have been added to the collection. The most important additions have been made in the ethnological department, including (during the past year) General Scratchley's collections from New Guinea, a series of 500 specimens of implements and ornaments from the West Indies, presented by Colonel Fielden, who has also given many rare stone implements and weapons collected in South Africa, and a series of 70 specimens of dresses, weapons, &c., from the Solomon and Banks Islands and from Santa Cruz, presented by Bishop Selwyn. The Curator, Baron von Hügel, reports that during the long vacation he excavated with success a Roman refuse-pit and a burial-place at the eastern side of Alderney. The digging is to be resumed.

THE seventh annual dinner of the Association of Public Sanitary Inspectors was held on Saturday evening at the First Avenue Hotel, Holborn. Dr. B. W. Richardson presided, and proposed the toast of "The Association and its President, Sir Edwin Chadwick." The duties of the Association, he said, were to teach and protect its members, and all sanitary inspectors ought to belong to it. He hoped that the apathy at present shown by too many of them would not last any longer.

DR. A. N. BERLESE, of Padua, has been appointed Professor of Botany to the Royal Lyceum at Ascoli-Piceno; and Dr. J. H. Wakker, of Utrecht, Professor of Botany at the dairy school at Oudshoorn, Holland.

THE *Botanical Gazette* published at Crawfordsville, Indiana, gives some particulars of one of the most magnificent bequests ever made for scientific purposes, that of the late Mr. H. Shaw for the endowment of the Botanic Garden and School of Botany at St. Louis, Missouri, amounting to not less than between three and five million dollars. The trustees have determined to apply the income to the maintenance and increase in the scientific usefulness of the Botanic Garden; to provide fire-proof quarters for the invaluable herbarium of the late Dr. George Engelmann, and to supply means for its enlargement; to secure a botanical museum; and to gradually acquire and utilize facilities for research in vegetable physiology and histology, the diseases and injuries of plants, and other branches of botany and horticulture. To aid in the carrying out of this last purpose, travelling botanical scholarships have been established. The present very able director of the Botanic Garden is Dr. William Trelease.

THE *Kew Bulletin* for February begins with some extracts from the Annual Report on the Government cinchona plantation and factory in Bengal for the year 1888–89. The valuable information presented in these extracts is given for the benefit of persons growing cinchona in countries which the documents for the Government of Bengal are little likely to reach. The new number also deals with the use of maqui berries for the colouring of wine, vine-culture in Tunis, phylloxera in Victoria, the botanical exploration of Cuba, and the sugar production of the world. The section on the last of these subjects relates to statistics brought together in Dr. Robert Giffen's report on the progress of the sugar trade. Commenting on the figures supplied in this report, the writer in the *Bulletin* says that if they "do not justify a gloomy view of the present position of the cane-sugar industry in British colonies, they scarcely justify a very optimistic one. It is obvious that the capital which should be applied to the improvement of manufacturing processes and machinery is, under present circumstances, practically diverted to the mere maintenance of the cultivation. And this in the long run must be a losing game. At present the fact stands that West Indian sugar has to a large extent been driven from the home market to that of the United States. If in time it should lose that, its fate apparently is sealed."

AT the last meeting of the Paris Biological Society, Prof. Raphael Blanchard gave an interesting account of a peculiar pigment, hitherto found in plants only, *carotine*, which he has discovered in a crustacean in one of the Alpine lakes, near Briançon. Its functions are not yet known, but M. Blanchard intends to pursue his study of the subject on the spot. The animals cannot be transported alive to lower levels.

WE are glad to welcome the first number of *The University Extension Journal*. The Society by which it is issued has become important enough to need an organ of its own; and the new periodical, which will appear at the beginning of every month, ought to be of service to all who are in any way interested in the movement.

THE *Engineer* of January 31 contains a leading article on "Colour-blind Engine-drivers," and it is interesting to note what the leading technical journal has to say on the subject: "We do not say that no accident was ever brought about by the inability of a driver to distinguish between a green light and a red one, but we can say that nothing of such an accident is to be met with in the Board of Trade Reports." Our contemporary is of opinion that the testing of the sight of locomotive men should be made under working conditions, *i.e.* with actual signal lights.

A PAPER on mortality from snake-bite in the district of Ratnagherry was read lately before the Bombay Natural History Society by Mr. Vidal, of the Bombay Civil Service. Many of the deaths in that district are, he says, due to a small and insignificant-looking snake, called "foorsa" by the natives. It is a viper rarely more than a foot long, and is so sluggish that it does not move out of the way till trodden on. Thus it is much more dangerous than the stronger and fiercer cobra.

DURING the year 1889 no fewer than 28 bears, 115 wolves, and 45 wolf-cubs were shot in the single district of Travnik, in Bosnia.

Das Wetter for January contains:—(*a*) An article by Dr. R. Assmann on climatological considerations about the prevalent epidemic of influenza. From an experience of many years in dealing with the connection between climatic conditions and the state of health, the author gives the following conditions as the most favourable for spreading organisms in the air: (1) dryness of the soil, (2) deficiency of snow covering, (3) deficiency of rainfall, (4) existence of fog or low-hanging clouds, (5) prevalence of high barometer with a small intermingling of air in the vertical direction; and he shows that these conditions were prevalent in Eastern and Central Europe from the beginning of November; that atmospheric dust existed in great quantities, and was propagated westward by easterly, north-easterly, and south-easterly winds. He considers that changes of temperature had no important relation to the spread of the epidemic. (*b*) A lecture recently delivered to the Scientific Club in Vienna, on the general circulation of the atmosphere, by Dr. J. M. Pernter. He refers to the idea of the conflict of polar and equatorial winds so long supported by Dove and others, and shows that the publication of synoptic charts since the year 1863 has demonstrated that the above theory does not hold good for temperate and northern latitudes, that the circulation there depends upon the positions of the areas of high and low pressures, producing cyclones and anticyclones. Many dark points require explanation, such as the tracks which the cyclones follow, but much new light has recently been thrown upon the subject, especially by the researches of Ferrel, Oberbeck, and Abercromby.

DR. ALBRECHT PENCK, Professor of Physical Geography at the University of Vienna, lately called attention to the fact that no two official accounts of the area of the Austro-Hungarian monarchy agree. The difference between the highest and the lowest estimates amounts to 3313·75 square kilometres. By an examination of the new special map constructed by the Army Geographical Institute, which is on the scale of 1 to 75,000, and occupies 400 sheets, Prof. Penck has satisfied himself that the actual area of the Empire is 3247·12 square kilometres greater than is given in the latest published official account. The error arose chiefly from an incorrect triangulation of the Hungarian portion of the Empire, which is 3054·02 square kilometres larger than has been supposed.

IT has hitherto been generally believed that the Montgolfier or hot-air balloon cannot be used in tropical climates. If this were true, ballooning for war purposes would of course be impossible in places where coal-gas could not be obtained. We learn from the *Times* that Mr. Percival Spencer, who has been making a series of interesting balloon experiments in Central India, has succeeded in showing that the theory is without foundation. At Secunderabad, in presence of the garrison and a crowd of European and native spectators, he lately made an ascent in his patent asbestos balloon. The inflation was effected by the burning of methylated spirit inside the balloon, which was held in place by 25 soldiers of the Bedford regiment until the word to "let go" was given. After rising to a considerable height, the aëronaut descended by means of his parachute. The spot where the ascent was made is over 2000 feet above the level of the sea, and the achievement was all the more remarkable because of the sultry climate and the great rarity of the air.

AN interesting paper on "Some Terraced Hill Slopes of the Midlands," by Mr. Edwin A. Walford, has been reprinted from the Journal of the Northamptonshire Natural History Society. The factors in the formation of these terraced slopes Mr. Walford groups as follows:—(1) The slipping and sliding outwards of the saturated porous marls upon the tenacious clays at the line of drainage, aided doubtless by the pressure of the superincumbent rock bed. (2) Displacements caused by the removal by chemical and mechanical solution of certain constituents of the marls and marlstone by the passage of the surface water through them. (3) The sliding downwards of the surface soil, as described by Dr. Darwin, and latterly illustrated by Mr. A. Ernst. The suggestions offered by Mr. Walford agree in the main, as he himself points out, with those adopted by Mr. A. Ernst in his paper in NATURE, February 28, 1889.

MESSRS. GAUTHIER-VILLARS (Paris) have recently added three new works to their already large list of photographic treatises. One is the "Manuel de Phototypie," by M. Bonnet, giving full details of the various processes for the rapid reproduction of photographs, such as is now demanded for many purposes. The formulæ are stated very clearly, and the apparatus required is sufficiently illustrated by diagrams. The treatise is thoroughly practical, and will be very valuable to all interested in the subject, whether as amateurs or for trade purposes. The second—"Temps de Pose"—is by M. Pluvinel, and deals with the difficult question of the time of exposure. It is shown that what is generally regarded as a rule-of-thumb process can be reduced to a scientific one. The various functions of the duration of the exposure are first considered mathematically, and it is then shown how the results of the investigations are to be applied practically, the method being illustrated by worked-out examples. To simplify matters, tables are given showing the different elements, such as coefficient of brightness, for all ordinary photographic subjects. The treatise is chiefly interesting as a scientific contribution, as few photographers will care to take the trouble of working out the time of exposure, now that they have found that good work can be done by judgment alone. The third book is in two volumes, and treats of the various "film" processes ("Procédés Pelliculaires," by George Balagny). It claims to give a full account of all that has been said and done in connection with the subject since the introduction of photography, and as far as we can judge, this claim is fully justified. Every detail of the subject is considered in a very practical manner. One of the most interesting applications of flexible films mentioned is the registration of flash signals in "optical telegraphy."

THE "Year-book of Photography" (Piper and Castle) for 1890 fully bears out the good reputation gained by its predecessors. In addition to the information relating to the various photographic societies, there are several articles on the advances in photographic processes which have been made during the past year, and other useful notes. One of the most interesting articles is that by the editor on photography in natural colours, from which we learn that "processes of practical value, to achieve the end, are likely to be discovered by the exercise of ability and perseverance." The only important omission we notice is a record of the remarkable achievements in astronomical photography. The volume contains a portrait and short biographical notice of Edmond Becquerel. The whole forms an invaluable book of reference to all photographic matters, with the exception referred to.

MESSRS. GEORGE BELL AND SONS have published "The School Calendar and Hand-book of Examinations, Scholarships, and Exhibitions, 1890." This is the fourth year of issue, and great pains have been taken, as in former years, to secure that the information brought together shall be full and trustworthy. A preface is contributed by Mr. F. Storr.

THE sixteenth part of Cassell's "New Popular Educator" has been issued. It includes a map of Australasia.

THE Proceedings of the International Zoological Congress, held in Paris last summer, will be ready for distribution in a fortnight.

A NEW and very simple method of synthesizing indigo has been discovered by Dr. Flimm, of Darmstadt (*Ber. deut. chem. Ges.*, No. 1, 1890, p. 57). In studying the action of caustic alkalies upon the monobromine derivative of acetanilide, $C_6H_5.NH.CO.CH_2Br$, a solid melting at 131°·5, it was found that when this substance was fused with caustic potash a product was obtained which at once gave an indigo blue colour on the addition of water, and quite a considerable quantity of a blue solid resembling indigo separated out. The best mode of carrying out the operation is described by Dr. Flimm as follows:—The monobromacetanilide is carefully mixed with dry caustic potash in a mortar, and the mixture introduced into a retort and heated rapidly until a homogeneous reddish-brown melt is obtained This is subsequently dissolved in water, and a little ammonia or ammonium chloride solution added, when the liquid immediately becomes coloured green, which colour rapidly changes into a dark blue, and in a short time the blue colouring matter is for the most part deposited upon the bottom of the vessel in which the operation is performed. The fused mass may also conveniently be dissolved in dilute hydrochloric acid, and a little ferric chloride added, when the formation of indigo takes place immediately. The collected blue colouring matter may be readily obtained pure by washing first with dilute hydrochloric acid and afterwards with alcohol. That this blue substance was really common indigo was proved by the fact that it yielded several of the most characteristic reactions of indigotin, such as solubility in aniline, paraffin, and chloroform, its sublimation, and the formation of sulphonic acids, which gave similar changes of colour with nitric acid to those of indigotin. The final proof was afforded by its reduction to indigo white and re-oxidation to indigo blue by exposure to air. Moreover, the absorption spectrum of the colouring matter was found to be identical with the well-known absorption spectrum of indigo. Hence there can be no doubt that indigo is really formed by this very simple process. The chemical changes occurring in the reaction are considered by Dr. Flimm to be the following:—Indigo blue is not produced directly, but first, as a condensation product of the monobromacetanilide, indoxyl is formed, $C_6H_4\left\langle\begin{matrix} NH \\ COH \end{matrix}\right\rangle CH$, or more probably a pseudo-indoxyl of the isomeric constitution $C_6H_4\left\langle\begin{matrix} NH \\ CO \end{matrix}\right\rangle CH_2$. This intermediate substance then passes over by oxidation into indigo, $C_6H_4\left\langle\begin{matrix} NH \\ CO \end{matrix}\right\rangle C{=}C\left\langle\begin{matrix} NH \\ CO \end{matrix}\right\rangle C_6H_4$, two molecules each losing two atoms of hydrogen by oxidation, and then condensing to form indigo. It was not found possible to isolate the intermediate pseudo-indoxyl, owing to its extreme instability; indeed, the all-important point to be observed in the practical carrying out of the synthesis by this method is that the fusion must be performed quickly and the temperature raised rapidly to a considerable height, the whole process occupying only a few minutes. The yield of pure indigo under the conditions yet investigated is not very large, amounting to about four per cent. of the weight of the original anilide.

THE additions to the Zoological Society's Gardens during the past week include thirteen Cuning's Octodons (*Octodon cuningi*) from Chili, presented by Mr. W. H. Newman; five Common Dormice (*Muscardinus avellanarius*), British, presented by Mr. Florance Wyndham; a Large Hill-Mynah (*Gracula intermedia*) from India, deposited; a Dingo (*Canis dingo*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at Greenwich at 10 p.m. on February 6 = 7h. 7m. 56s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G.C. 1515	—	—	7 17 14	+69 14
(2) 51 Geminorum ...	5·5	Yellowish-red.	7 7 3	+16 21
(3) *v* Geminorum ...	4	Yellow.	7 27 26	+32 8
(4) *α* Geminorum ...	2	White.	7 11 48	+16 44
(5) DM. + 3°·1381 ...	9	Reddish-yellow.	6 38 54	+ 3 24
(6) U Monocerotis ...	Var.	Orange.	7 25 32	− 9 33

Remarks.

(1) The spectrum of this nebula has not yet, so far as I know, been recorded, but the observation will not be difficult, if one may judge from the description given by Herschel, namely: "Very bright, pretty large, round, much brighter in the middle, mottled as if with stars."

(2) This star has a spectrum of the Group II. type, Dunér describing it as very beautiful. He states that all the bands, 1–9, are very wide and dark. The observations most likely to extend our knowledge of the group of bodies to which this star belongs are (1) observations of the bright carbon flutings (see p. 305); (2) comparisons with the flame spectra of manganese, magnesium, and lead; (3) observations made with special reference to the presence or absence of absorption lines, of which Dunér makes no mention.

(3) Gothard classes this with stars of the solar type. The usual differential observations are required.

(4) A star of Group IV. The usual observations of the relative intensities of the hydrogen and metallic lines (*b*, D, &c.), as compared with other stars, are required.

(5) A rather faint star of Group VI., in which the character of band 6 (near λ 564), as compared with the other carbon bands (9 and 10), requires further attention. Secondary bands should also be looked for.

(6) This variable is stated by Gore to have a continuous spectrum, but it seems probable that lines or flutings will be found if the star be examined under the most favourable conditions—that is, when near maximum. Rigel was formerly said to have a "continuous" spectrum, but the lines are now by no means difficult to see. The star ranges from magnitude 6 at maximum to 7·2 at minimum, and the period is 31–50 days (Gore).

A. FOWLER.

TOTAL SOLAR ECLIPSE OF 1886.—Dr. Schuster has thus summarized the spectroscopic results he obtained at this eclipse (Phil. Trans., vol. 180, 1889):—

(1) The continuous spectrum of the corona has the maximum of actinic intensity displaced considerably towards the red, when compared with the spectrum of sunlight.

(2) While, on the two previous occasions on which photographs of the spectrum were obtained, lines showed themselves outside the limits of the corona, this was not the case in 1886.

(3) Calcium and hydrogen do not form part of the normal spectrum of the corona. The hydrogen lines are visible only in the parts overlying strong prominences; the H and K lines of calcium, though visible everywhere, are stronger on that side of the corona which has many prominences at its base.

(4) The strongest corona line in 1886 was at $\lambda = 4232{\cdot}8$; this is probably the 4233·0 line often observed by Young in the chromosphere.

(5) Of the other strong lines, the positions of the following seem pretty well established:—

4056·7 **4084·2** 4089·3 **4169·7** **4195·0** **4211·8**
4280·6 4365·4 **4372·2** 4378·1 4485·6 4627·9

The lines printed in thicker type have been observed also at the Caroline Island and Egyptian Eclipses.

(6) A comparison between the lines of the corona and the lines of terrestrial elements has led to negative results.

ANNUAIRE DU BUREAU DES LONGITUDES.—In the volume for 1890, MM. Lœwy and Schulhof contribute a list of the comets which appeared from 1825 to 1835 inclusive, and in 1888, being a continuation of the lists given in former years. M. Lœwy also gives a complete table of the appearances of the planets throughout 1890, and ephemerides of a considerable number of variable stars. An elaborate comparison of the various calendars is from the pen of M. Cornu, and under the head of the solar system a rich store of information is included. With the notices we find an account of the meeting of the permanent committee of the photographic chart of the heavens and the Photographic Congress of September last. This year's *Annuaire* is as completely filled with information as it has ever been and doubtless will be as much appreciated by astronomers.

ANNUAIRE DE L'OBSERVATOIRE ROYAL DE BRUXELLES.—The volume for 1890 is the fifty-seventh annual publication from this Observatory. It contains tables of the mean positions of the principal stars and their apparent right ascensions, of the occultation of stars by the moon, and of eclipses of Jupiter's satellites, mention being also made of remarkable phenomena relating to the moon and the planets. M. Folie gives a biographical sketch of his predecessor, J. C. Houzeau, which is embellished with the portrait of this deceased bibliographer. Considerable attention has been paid to the researches on diurnal nutation and the determination of the constant. M. Spee discusses the tabulated observations of the condition of the sun's surface during 1888, and M. Moreau contributes an interesting note on the movement of a solid about a fixed point. A list is also given of the comets and asteroids discovered in 1889, and some of the particulars relating to their orbits.

ROYAL ASTRONOMICAL SOCIETY.—The annual general meeting of the Fellows of this Society will be held at Burlington House on Friday, the 14th inst., for the purpose of receiving the Report of the Council, electing officers for the ensuing year, and transacting other business of the Society. The chair will be taken at 3 o'clock precisely.

Erratum.—In the elements of companion C of Brook's comet (p. 305), read $\Omega = 17^\circ\ 52'\ 24''{\cdot}5$, and $\log a = 0{\cdot}565059$.

GEOGRAPHICAL NOTES.

BARON NORDENSKIÖLD has announced in the Swedish Academy of Sciences, that he and Baron Oscar Dickson, with assistance from the Australian colonies, will start on an expedition in the South Polar regions next year.

A RECENT telegram from Tashkent announced that Colonel Pevtsoff and M. Roborovsky had discovered a convenient pass to the north-western part of Tibet, from Nia, and had mounted to the great table-land. The plateau has there an altitude of 12,000 feet above the sea, and the country round is desolate and uninhabited, while towards the south the plateau is well watered and wooded. The Tashkent telegram is so expressed that it might be supposed to mean that two separate passes had been discovered by the two explorers. But the news received from the expedition at St. Petersburg on December 26, and dated October 27, shows that both explorers proposed to leave the oasis of Keria (100 miles to the east of Khotan) on the next day, for Nia (65 miles further east) and there to search for a passage across the border-ridge which received from Prjevalsky the name of the "Russian ridge." This immense snow-clad chain separates the deserts of Eastern Turkestan from the trapezoidal space, the interior of which is quite unknown yet, and which is bordered by the "Russian" ridge and the Altyn-tagh in the north-west; the ridges of Tsaidam and those named by Prjevalsky "Columbus" and "Marco-Polo" in the north-east; the highlands (explored by Prjevalsky in 1879–80) at the sources of the Blue River, in the south-east; and a long, yet unnamed ridge which seems to be a prolongation of the Tan-la, in the south-west. The pass leading to that plateau from Nia, and now discovered by the Russian expedition, is situated some 80 miles to the east of the well-known pass across the Kuen-lun Mountains which leads from Southern Khotan to Lake Yashi-kul. M. Roborovsky's intention is evidently next to move up the Tchertchen river and to endeavour to reach the ridges "Moscow" and "Lake Unfreezing" (11,700 feet high), which were visited by Prjevalsky from the east during his last journey. Having succeeded in finding a pass to Tibet in the south of Nia, Colonel Pevtsoff proposes, as soon as the spring comes, to proceed himself by this pass to the table-land, while M. Roborovsky probably will be despatched to explore the same border-ridge further east, in the south of Tchertchen.

THE *Boletin* of the Madrid Geographical Society for the last quarter of 1889 contains a most valuable memoir by Dr. Fernando Blumentritt, on the intricate ethnology of the Philippine Islands. The author classifies the whole of the native population in three broad divisions—Negrito, Malay, and Mongoloid; the last comprising those tribes which in their physical appearance betray certain Chinese or Japanese affinities. All are grouped in an admirably arranged alphabetical table, where their names, race, language, religion, culture, locality, and numbers are briefly specified in seven parallel columns. With a few variants and cross-references this table contains no less than 159 entries, and thus conveys in summary form all the essential particulars regarding every known tribe in the Philippine Archipelago. From it we gather that the Negritoes—that is, the true autochthonous element, variously known as Aetas, Attas, Atés, Etas, Itas, Mamanuas, &c., and physically belonging to the same stock as the Samangs of the Malay Peninsula—

are now reduced to about 20,000, dispersed in small groups over the islands of Luzon, Mindoro, Tablas, Panay, Negros, Cebu, Paragan (Palawan), and Mindanao. A few also appear still to survive in Alabat, Busuanga, and Culioú. Of the Malay peoples by far the most numerous and important are the southern Bisayas (Visayas), and the northern Tagalas, both described as "civilized Christians," and numbering respectively 1,700,000 and 1,250,000. These two peoples are steadily encroaching on all the surrounding tribes, causing them to disappear by a gradual process of absorption or assimilation, and the time is approaching when the whole of the islands will be divided into two great nationalities bearing somewhat the same relation to each other that the High German does to the Low German branch of the Teutonic family.

SMOKELESS EXPLOSIVES.[1]

I.

THE production of smoke which attends the ignition or explosion of gunpowder is often a source of considerable inconvenience in connection with its application to naval or military purposes, its employment in mines, and its use by the sportsman, although occasions not unfrequently arise during naval and military operations when the shroud of smoke produced by musketry or artillery fire has proved of important advantage to one or other, or to both, of the belligerents during different periods of an engagement.

Until within the last few years, however, but little, if any, thought appears to have been given to the possibility of dispensing with or greatly diminishing the production of smoke in the application of fire-arms, excepting in connection with sport. The inconvenience and disappointment often resulting from the obscuring effects of a neighbouring gun-discharge, or of the first shot from a double-barrel arm, led the sportsman to look hopefully to gun-cotton, directly after its first production in 1846, as a probable source of greater comfort and brighter prospects in the pursuit of his pastime and in his strivings for success.

A comparison between the chemical changes attending the burning, explosion, or metamorphosis of gun-cotton and of gunpowder, serves to explain the cause of the production of smoke in the latter case, and the reason of smokelessness in the case of gun-cotton. Whilst the products of explosion of the latter consist exclusively of gases, and of water which assumes the transparent form of highly-heated vapour at the moment of its production, the explosive substances classed as gunpowder, and which consist of mixtures of saltpetre, or another nitrate of a metal, with charred wood or other carbonized vegetable matter, and with variable quantities of sulphur, furnish products, of which very large proportions are not gaseous, even at high temperatures. Upon the ignition of such a mixture, these products are in part deposited in the form of a fused residue, which constitutes the fouling in a fire-arm, and are in part distributed, in an extremely fine state of division, through the gases and vapours developed by the explosion, thus producing smoke.

In the case of gunpowder of ordinary composition, the solid products amount to over fifty per cent. by weight of the total products of explosion, and the dense white smoke which it produces consists partly of extremely finely-divided potassium carbonate, which is a component of the solid products, and, to a great extent, of potassium sulphate produced chiefly by the burning of one of the important solid products of explosion—potassium sulphide—when it is carried in a fine state of division into the air by the rush of gas.

With other explosives, which are also smoke-producing, the formation of the smoke is due to the fact that one or other of the products, although existing as vapour at the instant of its development, is immediately condensed to a cloud composed of minute liquid particles, or of vesicles, as in the case of mercury vapour liberated upon the explosion of mercuric fulminate, or of the aqueous vapour produced upon the ignition of a mixture of ammonium nitrate and charcoal, or ammonium nitrate and picric acid.

Until within the last half-dozen years, the varieties of gunpowder which have been applied to war purposes in this and other countries have exhibited comparatively few variations in chemical composition. The proportions of charcoal, saltpetre, and sulphur employed in their production exhibit slight differences in different countries, and these, as well as the character of the charcoal used, its sources and method of production, underwent but little modification for very many years. The same remark applies to the nature of the successive operations pursued in the manufacture of black powder for artillery purposes in this and other countries.

The replacement of smooth-bore guns by rifled artillery which followed the Crimean war, and the increase in the size and power of guns consequent upon the application of armour to ships and forts, soon called for the pursuit of investigations having for their object the attainment of means for variously modifying the action of fired gunpowder, so as to render it suitable for the different calibres of guns, whose full power could not be effectively, or in some instances safely, developed by the use of the kind of gunpowder previously employed indiscriminately in artillery of all known calibres.

In order to control the violence of explosion of gunpowder, by modifying the rapidity of transmission of explosion from particle to particle, or through the mass of each individual particle, of which the charge of a gun is composed, the accomplishment of the desired results was, in the first instance, and indeed throughout practical investigations extending over many years, sought exclusively in modifications of the size and form of the individual masses composing a charge of powder, and of their density and hardness, it being considered that, as the proportions of saltpetre, charcoal, and sulphur generally employed in the production of gunpowder very nearly correspond to those required for the development of the greatest chemical energy by those incorporated materials, it was advisable to seek for the attainment of the desired results by modifications of the physical and mechanical characters of, rather than by any modification in the proportions and chemical characters of, its ingredients.

The varieties of powder, which, as the outcome of careful practical and scientific researches in this direction, have been introduced into artillery service from time to time, and some of which, at any rate, have proved fairly efficient, have been of two distinct types. The first of these, produced by breaking up more or less highly-pressed cakes of black powder into grains, pebbles, or boulders, of approximately uniform size and shape, the sharp edges and rough surfaces being afterwards removed by attrition (reeling and glazing), are simply a further development of one of the original forms of granulated or corned powder, represented by the old F. G., or small arms, and L. G., or cannon powder. Gunpowder of this class, ranging in size from about 1000 pieces to the ounce, to about six pieces to the pound, have been introduced into artillery service, and certain of them, viz. R. L. G. (rifle large grain), which was the first step in advance upon the old cannon-powder (L. G.); pebble-powder (P.), and large pebble or boulder-powder (P. 2), are still employed more or less extensively in some guns of the present day.

The other type of powder has no representative among the more ancient varieties; it has its origin in the obviously sound theoretical view that uniformity in the results furnished by a particular powder, when employed under like conditions, demands not merely identity in regard to composition, but also identity in form, size, density, and structure of the individual masses composing the charge used in a gun. The practical realization of this view should obviously be attained, or at any rate approached, by submitting equal quantities of one and the same mixture of ingredients, presented in the form of powder of uniform fineness and dryness, to a uniform pressure for a fixed period in moulds of uniform size, and under surrounding conditions as nearly as possible alike. The fulfilment of these conditions would, moreover, have to be supplemented by an equally uniform course of proceeding in the subsequent drying and other finishing processes to which the powder-masses would be submitted.

The only form of powder, introduced into our artillery service for a brief period, in the production of which these conditions were adhered to as closely as possible, was a so-called pellet powder, which consisted of small cylinders having semi-perforations with the object of increasing the total inflaming surface of the individual masses.

Practical experience with this powder, and with others prepared upon the same system, but with much less rigorous regard to uniformity in such details as state of division and condition of dryness of the powder before its compression into cylindrical, or other forms, showed that uniformity in the ballistic properties

[1] Friday Evening Discourse delivered by Sir Frederick Abel, F.R.S., at the Royal Institution of Great Britain, on January 31, 1890.

of black powder could be as well and even more readily secured by the thorough blending or mixing together of batches presenting some variation in regard to density, hardness, or other features, as by aiming at an approach to absolute uniformity in the characters of each individual mass composing a charge.

At the time that our attention was first actively given to this subject of the modification of the ballistic properties of powder, it had already been to some extent dealt with in the United States by Rodman and Doremus, and the latter was the first to propose the application, as charges for guns, of powder-masses produced by the compression of coarsely grained powder into moulds of prismatic form. In Russia the first step was taken to utilize the results arrived at by Doremus, and to adopt a prismatic powder for use in guns of large calibre.

Side by side with the development and perfection of the manufacture of prismatic powder in Russia, Germany, and in this country, new experiments on the production of powder-masses suitable, by their comparatively gradual action, for employment in the very large charges required for the heavy artillery of the present day, by the powerful compression of mixtures of more or less finely broken up powder-cake into masses of greater size than those of the pebble, pellet, and prism powders, were actively pursued in Italy, and also by our own Government Committee on Explosives, and the outcome of very exhaustive practical investigations were the very efficient Fossano powder, or *poudre progressif*, of the Italians, and the boulder and large cylindrical powders known as P^2 and C^2, produced at Waltham Abbey, which scarcely vied, however, with the Italian powder in the uniformity of their ballistic properties.

Researches carried out by Captain Noble and the lecturer some years ago with a series of gunpowders differing considerably in composition from each other, indicated that advantages might be secured in the production of powders for heavy guns by so modifying the proportions of the constituents (*e.g.* by considerably increasing the proportion of charcoal and reducing the proportion of sulphur) as to give rise to the production of a much greater volume of gas, and at the same time to diminish the heat developed by the explosion.

These researches served, among other purposes, to throw considerable light upon the cause of the wearing or erosive action of powder-explosions upon the inner surface of the gun, which in time may produce so serious a deterioration of the arm as to diminish the velocity of projection considerably, and so affect the accuracy of shooting, a deterioration which increases in extent in an increasing ratio to the size of the guns, in consequence, obviously, of the large increase in the weight of the charges fired.

Several causes undoubtedly combine to bring about the wearing away of the gun's bore, which is especially great where the products of explosion, while under the maximum pressure, can escape between the projectile and the bore of the gun. The great velocity with which the very highly heated gaseous and liquid (fused solid) products of explosion sweep over the heated surface of the metal gives rise to a displacement of the particles composing it, which increases as the surface becomes roughened by the first action upon the least compact portions of the metal, and thus opposes greater resistance; at the same time, the effect of the high temperature to which the surface is raised is to reduce its rigidity and power of resisting the force of the gaseous torrent, and lastly some amount of chemical action upon the metal, by certain of the highly heated non-gaseous products of explosion, contributes towards an increase in the erosive effects. A series of careful experiments made by Captain Noble with powders of different composition, and with other explosives, afforded decisive evidence that the material which furnished the largest proportion of gaseous products, and the explosion of which was attended by the development of the smallest amount of heat, exerted least erosive action.

It is probable that important changes in the composition of powders manufactured by us for our heavy guns would have resulted from those researches, but in the meantime, two eminent German gunpowder manufacturers had occupied themselves independently, and simultaneously, with the important practical question of producing some more suitable powder for heavy guns than the various new forms of ordinary black powder, the rate of burning of which, especially when confined in a close chamber, was, after all, reduced only in a moderate degree by the increase in the size of the masses, and by such increase in their density as it was practicable to attain. The German experimenters directed their attention not merely to the proportions in which the powder ingredients are employed, but also to a modification in the character of charcoal, and the success attending their labours in these directions led to the practically simultaneous production, by Mr. Heidemann at the Westphalia Powder Works, and Mr. Düttenhofer at the Rottweil Works near Hamburg, of a prismatic powder of cocoa-brown colour, consisting of saltpetre in somewhat higher proportion, of sulphur in much lower proportion, than in normal black powder, and of very slightly burned charcoal, similar in composition to the charcoal (*charbon roux*) which Violette, a French chemist, first produced in 1847 by the action of superheated steam upon wood or other vegetable matter, and which he proposed for employment in the manufacture of sporting powder. These brown prismatic powders (or "cocoa-powders," as they were termed from their colour), are distinguished from black powder not only by their appearance, but also by their very slow combustion in open air, by their comparatively gradual and long-sustained action when used in guns, and by the simple character of their products of explosion as compared with those of black powder. As the oxidizing ingredient, saltpetre, is contained, in brown or cocoa powder, in larger proportion relatively to the oxidizable components, sulphur and charcoal, than in black powder, these become fully oxidized, while the products of explosion of the latter contain, on the other hand, larger proportions of unoxidized material, or only partially oxidized products. Moreover, there is produced upon the explosion of brown powder a relatively very large amount of water-vapour, not merely because the finished powder contains a larger proportion of water than black powder, but also because the very slightly charred wood or straw used in the brown powder is much richer in hydrogen than black charcoal, and therefore furnishes by its oxidation a considerable amount of water. The total volume of gas furnished by the brown powder (at 0° C. and 760 mm. barometer) is only about 200 volumes per kilogramme of powder, against 278 volumes furnished by a normal sample of black powder, but the amount of water-vapour furnished upon its explosion is about three times that produced from black powder, and this would make the volume of gas and vapour developed by the two powders about equal if the heat of its explosion were the same in the two cases; the actual temperature produced by the explosion of brown powder, is, however, somewhat the higher of the two.

Although the smoke produced upon firing a charge of brown powder from a gun appears at first but little different in denseness to that of black powder, it certainly disperses much more rapidly, a difference which is probably due to the speedy absorption, by solution, of the finely divided potassium salts by the large proportion of water-vapour distributed throughout the so-called smoke.

This class of powder was substituted with considerable advantage for black powder in guns of comparatively large calibre; nevertheless it became desirable to attain even slower or more gradual action in the case of the very large charges required for guns of the heaviest calibres, such as those which propel shot of about 2000 pounds weight. Accordingly, the brown powder has been modified in regard to the proportions of its ingredients to suit these conditions, while, on the other hand, powder intermediate with respect to rapidity of action between black pebble powder and the brown powder, has been found more suitable than the former for use in guns of moderately large calibre.

The recent successful adaptation of machine guns and comparatively large quick-firing guns to naval service, more especially for the defence of ships against attack by torpedo boats, &c., has rendered the provision of a powder for use with them, which would produce comparatively little or no smoke, a matter of very considerable importance, inasmuch as the efficiency of such defence must be greatly diminished by the circumstance that, after a very brief use of the guns with black powder, the objects against which their fire is destined to operate, become more or less completely hidden from those directing them, by the dense veil of powder-smoke produced. Hence much attention has been directed during the last few years to the production of smokeless, or nearly smokeless powders for naval use in the above directions. At the same time, the views of many military authorities regarding the importance of dispensing with smoke in land engagements has also created a demand, the apparent urgency of which has been increased by various circumstances,

for a smokeless powder suitable for field artillery and small arms.

The properties of ammonium nitrate, of which the products of decomposition by heat are, in addition to water-vapour, entirely gaseous, have rendered it a tempting material to work upon in the hands of those who have striven to produce a smokeless powder, but its deliquescent character has been the chief obstacle to its application as a component of an explosive agent susceptible of substitution for black powder for service purposes.

A German chemical engineer, F. Gäus, conceived that, by incorporating charcoal and saltpetre with a particular proportion of ammonium nitrate, he had produced an explosive material which did not partake of the hygroscopic character common to other ammonium-nitrate mixtures, and that, by its explosion, the potassium in the saltpetre formed a volatile combination with nitrogen and hydrogen, a *potassium amide*, so that, although containing nearly half its weight of potassium salt, it would furnish only volatile products. The views of Mr. Gäus regarding the changes which his so-called *amide powder* undergoes upon explosion were not borne out by existing chemical knowledge, while the powder compounded in accordance with his views proved to be by no means smokeless, and was certainly not non-hygroscopic. Mr. Heidemann has, however, been successful, by modifications of Gäus's prescription and by application of his own special experience in powder-manufacture, in producing an ammonium-nitrate powder possessed of remarkable ballistic properties, furnishing comparatively little smoke, which speedily disperses, and exhibiting the hygroscopic characteristics of ammonium-nitrate preparations in a decidedly less degree than any other hitherto prepared. The powder, while yielding a very much larger volume of gas and water-vapour than black or brown powder, is considerably slower than the latter; the charge required to produce equal ballistic results is less, while the chamber-pressure developed is lower, and the pressures along the chase of the gun are higher, than in the case of brown powder.

The ammonium-nitrate powder contains, in its normal, dried condition, more water than even brown powder; it does not exhibit any great tendency to absorb moisture from an ordinarily dry or even a somewhat moist atmosphere, but if the amount of atmospheric moisture approaches saturation, it will rapidly absorb water, and when once the process begins it continues rapidly, the powder-masses becoming speedily quite pasty. The charges for quick-firing guns are enclosed in metal cases, in which they are securely sealed up; the powder is therefore prevented from absorbing moisture from the external air, but it has been found that if the cartridges are kept for long periods in ships' magazines, in which, from their position relatively to the ships' boilers, the temperature is more or less elevated, sometimes for considerable periods, the expulsion of water from some portions of the powder-masses composing the hermetically sealed charge, and its consequent irregular distribution, may give rise to want of uniformity in the action of the powder, and to the occasional development of high pressures. Although, therefore, this ammonium-nitrate powder may be regarded as the first successful advance towards the production of a comparatively smokeless artillery powder, it is not uniformly well adapted to the requirements which it should fulfil in naval service.

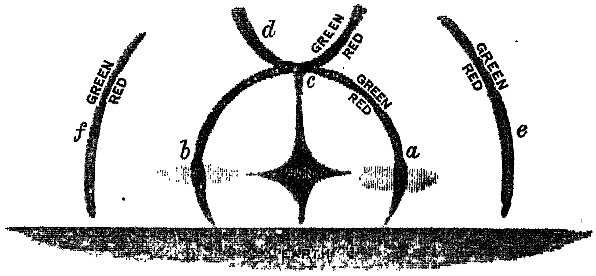
Attention was first seriously directed to the subject of smokeless powder by the reports received about four years ago of remarkable results stated to have been obtained in France with such a powder for use with the magazine rifle (the Lebel) which was being adapted to military service. These reports were speedily followed by others, descriptive of marvellous velocities obtained with small charges of this powder, or some modifications of it, from guns of very great length. As in the case of mélinite, the fabulously destructive effects of which were much vaunted at about the same time, the secret of the precise nature of the smokeless powder was so well preserved by the French authorities, that surmises could only be made on the subject even by those most conversant with these matters. It is now well known, however, that more than one smokeless explosive has succeeded the original powder, the perfection of which was reported to be beyond dispute, and that the material now adopted for use in the Lebel rifle bears, at any rate, great similarity to preparations which have been made the subject of patents in this country, and which are still experimental powders in other countries.

(*To be continued.*)

SOLAR HALOS AND PARHELIA.

THE recent appearance of solar and lunar halos, parhelia, and paraselene, has called forth a considerable amount of correspondence from all parts of the country, and the accompanying figure may be taken as a composite representation of the solar phenomenon observed. A glance at the times at which the halos were observed on the 29th ult., makes it apparent that they occurred earliest in places of highest latitudes. At Driffield, in lat. 54°, the halo, with its attendant parhelia, was observed at 1.34 p.m., and the whole phenomenon disappeared at 2.8 p.m.; at Burton-on-Trent, lat. 52° 48′, the halos and parhelia were first observed at 2 p.m., and lasted more or less distinctly until 3 p.m.; whilst about a degree south of this, at Oxford, Colnbrook, and Walton-on-Thames, the phenomena occurred from about 3.30 to 4.30. The uniform difference in the times when the halos were observed at the places of different latitudes necessarily follows from the fact that they are formed by the action upon solar rays of prismatic crystals of ice suspended in the air by the ascending currents which especially occur in the spring and autumn. Those prisms that are in such positions that the rays from the sun in transmission through them suffer minimum deviation are the cause of the formation of halos, and since the angular distance of the sun equal to minimum deviation is about 22°, this must be the radius of the halo, and the external circle, being produced by two such refractions in succession, has a radius of about 46°.

The halos recently observed do not differ in the main from those frequently seen in higher latitudes, and consisting of (1) a first circle or halo concentric with the sun, red within, violet without, and at an angular distance of 22° or 23°; (2) a second circle or halo, similar to the preceding, but at an angular distance of 46°; (3) a portion of the *parhelic* circle appearing horizontal and diametral, and at the points of junction of this circle with the two halos, there is increased luminosity, which have been taken for images of the sun; (4) horizontal arcs, tangents to the circular halos, and a vertical line making a cross with the horizontal portion of the parhelic circle.



a was seen at 3.35 p.m.; *b* at 3.45 p.m.; *c* and *d* at 3.50 p.m.; *e* at 4.0 p.m; *f* at 4.10 p.m.

Mr. John Lovell thus describes the phenomena observed at Driffield:—"A splendid solar halo, with its attendant parhelia, was observed this afternoon at 1.34 local time. The halo (diameter 45°) was almost perfect, the lower part only being slightly obliterated by the thick atmosphere near the horizon. Attached to the upper side, an inverted portion of a similar halo appeared, brilliantly illumined on the concave side, the lower part giving out a dull red light. Again, 22½° above this, and also inverted, about 60° of arc beautifully coloured with rainbow colours was clearly visible, the red side lowest. This arc, if it had been produced, would have circled the zenith. The mock lights on each side of the halo were drawn out into long cones of intensely bright light, while the inner sky of the halo was of a very dark shade. The most noteworthy feature of the display was a brilliant patch of pure white light in the north-western sky, at a distance of 90° from the western mock sun, and undoubtedly emanating from it, and which remained visible for nearly ten minutes. The whole phenomena disappeared at 2.8 p.m., the sky then being covered with streaky cirro-stratus haze from the north-north-west."

The patch of white light referred to by Mr. Lovell was doubtless produced by the junction of the parhelic circle with one of the halos concentric with the sun. It is perhaps hardly necessary to note the relation that exists between halos and cirro-

stratus clouds, and that the space included within the halo is frequently of a more intense grey, or of a deeper blue than the rest of the sky.

The son of Sir W. Herschel observed the phenomena at Oxford, and noted :—"The sun was near the horizon. On either side of it, at a distance of five or six diameters of the sun, was a mock sun, not very bright, of the colours of the rainbow, the one on the right being the brighter. There was a scarcely perceptible rainbow, of which red was the only colour visible, joining the two mock suns. This rainbow was brightest directly over the sun. As far off again as the first was a second rainbow, hazy, but fairly bright, which was equally visible from earth to earth. Vertically above the sun, a third, a very bright rainbow, touched the second, being inverted, and having its centre straight overhead. It did not look quite as large as the second. The weather was clear, but the clouds on and above the horizon were of a uniform grey colour, fading off gradually to a nearly clear sky overhead. There seemed always to be a much lighter shade of grey in the clouds where the sun and the two mock suns were."

The coloured parhelia observed indicates the refraction and dispersion of solar light by vertical prisms, whilst the phenomena of inverted arches are produced by the light which passes through horizontal crystals, at different azimuths.

Mr. Frank E. Lott, at Burton-on-Trent, observed a third parhelia on the part of the first halo vertically above the sun, whilst Mr. H. G. Williams, of Caterham, observing the phenomena about 4 p.m., noted that the sun appeared about 10° above the horizon. So far, the observations of two or three parhelia with two halos and two inverted arches agree with many former descriptions. In the diagram appended, however, and in the majority of sketches received, the inverted arch is not given as the arc of a circle, but hyperbolic.

Mr. A. J. Butler, observing at Walton-on-Thames, remarks: "The hyperbolic band above the sun was carefully noted ;" and Mr. C. A. Carus-Wilson, in the following observation made at Staines, supports this view :—

"The sun was just setting behind a bank of hazy mist, appearing as a crimson disk enveloped in blue grey cloud; I first noticed a distinct bow, of light grey tint, and coloured for a short distance at its left extremity with the ordinary rainbow tints—red inside. There then appeared a part of a second bow outside the other, coloured throughout the whole length visible—red inside. From the sun vertically upwards to the first bow, there was a band of white light, quite distinct from the light grey tint of the lower bow, and above the lower bow this band continued as a hyperbolic brush of white light: this brush was much brighter and better defined than the vertical band. A hasty measurement, with a pocket sextant, of the angular radii of the two bows, gave 46° and 23° for the outside and inside bows respectively."

Mr. H. W. Pyddoke also remarks :—"The most noticeable thing of all was the shape of the upper bow, which was like a hyberbole except at its ends where it bent round again very slightly ;" and other correspondents concur in this description of the shape of the first inverted arch.

From the descriptions and figures given it is evident that the two parhelia on the parhelic circle are the respective centres of halos similar to the first halo concentric with the real sun; the intersection of these two circles with that surrounding the sun gives the appearance of a hyperbolic curve at the top of it. An exactly similar appearance was drawn by Pastorff as occurring on December 29, 1789, and is found in his "Beobachtungen der Sonnenflecke"; and *L'Astronomie* for August 1889 contains a drawing and description of a very similar appearance.

Lunar halos followed the solar halos on the 29th ult., and on the following day Mr. G. B. Buckton, F.R.S., observed three fine parhelia and a halo at Haslemere, and describes them as follows :—

"The sun shone brightly, but through a moderate haze. On the right and on the left, at equal altitudes with the sun, an oblong bright patch of light appeared. That on the left was the brightest, and formed a blurred image of the sun with all the prismatic colours of the rainbow, but the colours were reversed in order. The upper and lower parts of these mock suns were drawn out, and formed portions of a large circle of about (by eye estimate) 20° radius. These images were connected with the haze, but a lower stratum of finely striated cloud came between the eye and these patches. Immediately above the true sun a third patch of light occurred, through which a portion of an inverted circle was seen, the greater part of which was lost in the blue of the sky above. The right-hand mock sun was fainter than the other, on account of the grey haze being more dense."

Mr. Buckton's observation is a demonstration of the principle laid down—namely, that parhelia always appear at the same elevation as the true sun, and are united to each other by a white horizontal circle, whose pole is the zenith. This circle changes in elevation with the true sun; and the apparent semidiameter is always equal to the distance of the luminary from the zenith.

Mr. Nagel, of Trinity College, Oxford, notes that :—"The solar halos on the afternoon of January 29 were very clearly seen in Oxford; the tangential arc to the outer halo was extremely brilliant, and the two mock suns at the extremities of the horizontal diameter of the inner halo were well marked. During part of the time the halos lasted, a whitish incomplete circle was seen about 80° from the sun, and consequently beyond the zenith. This circle seemed to correspond to that first described by Helvelius in 1661."

It is evident from the descriptions given that the parhelia are not, as is sometimes supposed, images of the real sun at all, but only the junctions of two of the circles formed. The upper and the lower parts of these mock suns were drawn out and connected with the first halo, whilst their sides were observed to be drawn out and to merge into the parhelic circle.

THE INSTITUTION OF MECHANICAL ENGINEERS.

THE forty-third annual general meeting of the Institution of Mechanical Engineers took place on January 29, 30, and 31, in the theatre of the Institution of Civil Engineers.

The papers down for reading and discussion were as follows: on the compounding of locomotives burning petroleum refuse in Russia, by Mr. Thomas Urquhart, Locomotive Superintendent, Grazi and Tsaritsin Railway, South-East Russia; on the burning of colonial coal in the locomotives on the Cape Government railways, by Mr. Michael Stephens, Locomotive Superintendent; and on the mechanical appliances employed in the manufacture and storage of oxygen, by Mr. Kenneth S. Murray, of London. The latter paper was communicated through Mr. Henry Chapman.

Mr. Urquhart's paper is one of a series of excellent and thoroughly useful descriptions of work done by that gentleman on his railway, and had been for some time promised to the Institution. In order to satisfy himself as to the utility and saving of fuel in compound locomotives, he obtained the sanction of the Government for altering one locomotive by way of experiment. The altered engine was put to work, and the driver was allowed over a month's running to get fully acquainted with the handling in regular service. Comparative trials were then made of the compound against a non-compound locomotive with the same weight of train, on the same days, so as to expose them both to the same circumstances in regard to weather. It was clearly proved that the compound burnt 22 per cent. less of the petroleum refuse used as fuel than the non-compound engine, and the author's experience has left no doubt in his own mind that compound locomotives are the engines of the future in all countries. Mr. Urquhart's results are thoroughly borne out by those obtained in this country by Messrs. Worsdell and Webb. Some engineers suppose that this great economy in fuel is due to the higher working steam pressure, and therefore greater expansion in the compound engines as compared with the non-compound engines.

The paper by Mr. Michael Stephens is a description of the South African coal-fields, their discovery, and general working within the last sixteen years. It appears from the paper that the local coal cannot be burned to advantage without a special arrangement of fire-bars—as may be well imagined, since it contains nearly 30 per cent. of incombustible matter.

Mr. Kenneth S. Murray gives an interesting account of the commercial preparation of oxygen from the atmosphere by means of the alternate heating and cooling of the monoxide of barium. About thirty years ago the eminent French chemist Boussingault made the discovery that, at a temperature of about 1000° F., the monoxide of barium would absorb oxygen readily from the atmosphere, with the resulting formation of the dioxide;

and that at a higher temperature of about 1700° F. the oxygen thus absorbed would be given off again, and the monoxide would apparently be restored to its original condition. The paper clearly describes the machinery required for the manufacture of oxygen by means of barium oxide.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—The lecture lists for this term include the following courses:—Prof. Clifton, Magnetism; Mr. Baynes, Thermodynamics; Prof. Odling, Diacidic Olefine Acids; Mr. Veley, Physical Chemistry. Prof. Burdon-Sanderson has resumed his lectures, and Mr. Gotch is treating of the Physiology of Muscle. Dr. Tylor lectures on the Development of Religions.

An open Fellowship in Mathematics at Christ Church has been awarded to Mr. C. H. Thompson, Queen's College, Lecturer in Mathematics at Lampeter. No other mathematical Fellowship has been awarded for about seven years.

The arrangement of the Pitt-Rivers anthropological collection at the Museum is proceeding as rapidly as the constant acquisition of new material allows, and a large portion of the collection is now open for public inspection.

CAMBRIDGE.—At the next meeting of the Cambridge Philosophical Society, on Monday, February 10, the following papers will be read:—

(1) W. Bateson (St. John's), on the perceptions and modes of feeding of fishes.

(2) A. C. Seward (St. John's), notes on Lomatophloios.

(3) S. F. Harmer (King's), on the origin of the embryos in the ovicells of Cyclostomatous Polyzoa.

Prof. Stuart has communicated to the Vice-Chancellor his intention of resigning the Chair of Mechanism and Applied Science before the end of the current academical year.

SCIENTIFIC SERIALS.

American Journal of Mathematics, vol. xii., No. 2 (Baltimore, January 1890).—The number opens with the concluding part of Mr. Forsyth's paper on "Systems of Ternariants that are Algebraically Complete" (pp. 115–160). It is illustrated with numerous tables and closed with a useful abstract of contents.—In the following memoir (pp. 161–190), by Prof. Franklin, on "Some Applications of Circular Co-ordinates," the author investigates, with the aid of these co-ordinates, some interesting theorems relating to the orientation of systems of lines given in a recent volume (vol. x. p. 258) by M. Humbert. Several further illustrations are given, and the memoir closes with a discussion of the curve given by the equation $\sin xdx = \sin ydy$.—Mr. F. N. Cole writes (pp. 191–212) on "Rotations in Space of Four Dimensions." The present article is preliminary to a second paper on groups of rotations in four-dimensional space which is to follow.

Bulletins de la Société d'Anthropologie, tome xii., série iii., fasc. 3 (Paris, 1889).—Continuation of M. Dumont's paper on the natality of Paimpol, in which he treats at great length of the causes which influence the ratio of marriages contracted in every hundred of the population in the maritime districts of Brittany, and of the number of children born in each family. In both these respects the means rank amongst the lowest for all France. One cause for this may be the preponderance of women over men, a large number of the latter being engaged as seamen, or taking part in the Iceland and other distant fisheries. Another factor in this problem is probably the subdivision of property among all the members of a family, who in the peasant and small burgher classes, not uncommonly remain together all their lives, and avoid marriage in the fear of diminishing their individual shares of the patrimony. This, coupled with the repugnance, so common among the French peasantry, against large families, leads indirectly to late marriages or to celibacy, and has thus exercised a baneful influence on the normal increase of the population.—An essay on the classification of human races, based entirely on physical characters, by M. Denniker. Believing in the long persistence of types in spite of the constant intermixture of races, the author thinks that it is only by a careful study of the typical characteristics in a so-called ethnic group that we can arrive at any correct idea of the affinities between different races. In an elaborate synoptical table he enumerates the thirteen races which he proposes for his classification, adding separate remarks on the varieties of each.—The dog, by M. G. de Mortillet. Assuming from negative evidence the non-existence of the dog in the Quaternary age, the author traces his presence onwards from the Kjökkenmöddings, in which abundant remains of this animal are to be found. Passing from the prehistoric ages in Europe he considers at length the evidence that can be advanced of the existence of several varieties of the dog among the Egyptians, and later on among the ancient Greeks and Romans; and in the fact of the innumerable varieties of *Canis domesticus*, M. de Mortillet believes we have one of the most conclusive proofs of evolution.—Observations, on the skeletons of two young orangs, by M. Hervé.—Pre-Columbian ethnography of Venezuela, by Dr. Marcano. The most interesting report in this treatise is that referring to the Grotto de Cerro de Luna, owing to the almost absolute certainly that it had never been entered since Guiana was first visited by white men. Here Dr. Marcano recovered fifty-two male, and forty-three female skulls, with five of children, together with numerous long bones. Among these skulls many were painted red, and others had obviously been embalmed. The general mean of their cephalic index was 79, while the facial characters were mesorrhinic and prognathic.—On correlative variations in the biceps, by M. G. Hervé.—A report of the Seventh Conference on Transformism, by M. M. Duval. The author here gives an interesting biographical notice of the great French *savant* Lamarck, entering at the same time fully into the character and scope of his researches, and showing how far his views differed from, or approximated to, those of Darwin. As a *résumé* of what Lamarck attempted on the same lines of inquiry so successfully followed by Darwin, M. Duval's report presents much interest for the English reader.—On the menhirs of Morbihan, by M. Gaillard.—On the discovery of Robenhausian flint implements near Macon, by M. Lafay.—Comparison of three sub-species of man, by M. Lombard.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, January 23.—"On a Photographic Method for determining Variability in Stars." By Isaac Roberts, F.R.A.S. Communicated by Prof. J. Norman Lockyer, F.R.S.

Some of the uncertainties which necessarily attend the determination of variability in the brightness of stars by eye observations are removed by the application of photographic methods, and particularly by that of giving two or more exposures of the same photographic plate to a given sky space, with intervals of days or weeks between each exposure.

In this way any errors caused by atmospheric, actinic, or chemical changes, together with those due to personal bias, are eliminated, and the study of stellar variability can be pursued under conditions that admit of the necessary exactitude.

As an illustration of the applicability of this dual photographic method, the enlargement on paper from the negative is now submitted. It shows the results obtained by two exposures of the same plate to the sky in the region of the great nebula in Orion. The first exposure was of two hours' duration on January 29, and the second of two and a half hours on February 3, 1889. The stellar images formed during the two exposures are 0·0122 of an inch apart, measured from centre to centre, and therefore comparable with each other in the field of a microscope. When the images are examined in the manner thus indicated, and their diameters also measured by means of a suitably made eye-piece micrometer, it is found that at least ten of the photographed stars, the magnitudes of which are estimated to range between the 7th and 15th, have changed to a considerable extent in the short interval of five days.

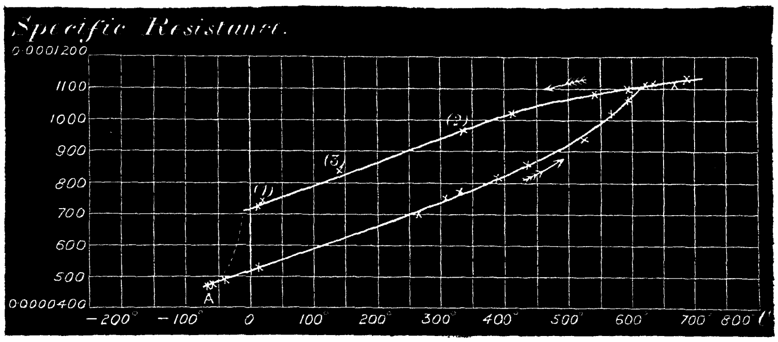
The ten stars referred to are to be found within an area of less than two square degrees of the sky, and in the table given are the co-ordinates of their positions with reference to θ Orionis. The measurements of the diameters of their photo-images on a scale of 0·00002 of an inch are also given.

"Physical Properties of Nickel Steel." By J. Hopkinson, D.Sc., F.R.S.

Mr. Riley, of the Steel Company of Scotland, has kindly sent me samples of wire drawn from the material concerning the magnetic properties of which I recently made a communication

to the Royal Society.[1] As already stated, this material contains 25 per cent. of nickel and about 74 per cent. of iron, and over a range of temperature from something below freezing to 580° C. it can exist in two states, magnetic and non-magnetic.

The wire as sent to me was magnetizable as tested by means of a magnet in the ordinary way. On heating it to a dull redness, it became non-magnetizable, whether it was cooled slowly or exceedingly rapidly by plunging it into water. A quantity of the wire was brought into the non-magnetizable state by heating it, and allowing it to cool. The electric resistance of a portion of this wire, about 5 metres in length, was ascertained in terms of the temperature; it was first of all tried at the ordinary temperature, and at temperatures up to 340° C. The specific resistances at these temperatures are indicated in the curve by the numbers 1, 2, 3. The wire was then cooled by means of solid carbonic acid; the supposed course of change of resistance is indicated by the dotted line on the curve; the actual observations of resistance, however, are indicated by the crosses in the neighbourhood of the letter A on the curve. The wire was then allowed to return to the temperature of the room, and was subsequently heated, the actual observations being shown by crosses on the lower branch of the curve; the heating was continued to a temperature of 680° C., and the metal was then allowed to cool, the actual observations being still shown by crosses. From this curve, it will be seen that in the two states of the metal, magnetizable and non-magnetizable, the resistances at ordinary temperatures are quite different. The specific resistance in the magnetizable condition is about 0·000052, in the non-magnetizable condition it is about 0·000072. The curve of resistance in terms of the temperature of the material in the magnetizable condition has a close resemblance to that of soft iron, excepting that the coefficient of variation is much smaller, as, indeed, one would expect it to be in the case of an alloy; at 20° C. the coefficient is about 0·00132, just below 600° C. it is about 0·0040, and above 600° it has fallen to a value less than that which it had at 20° C. The change in electrical resistance effected by cooling is almost as remarkable as the change in the magnetic properties.



Samples of the wire were next tested in Prof. Kennedy's laboratory for mechanical strength. Five samples of the wire were taken which had been heated and were in the non-magnetizable state, and five which had been cooled and were in the magnetizable state. There was a marked difference in the hardness of these two samples; the non-magnetizable was extremely soft, and the magnetizable tolerably hard. Of the five non-magnetizable samples the highest breaking stress was 50·52 tons per square inch, the lowest 48·75; the greatest extension was 33·3 per cent., the lowest 30 per cent. Of the magnetizable samples, the highest breaking stress was 88·12 tons per square inch, the lowest was 85·76; the highest extension was 8·33, the lowest 6·70. The broken fragments, both of the wire which had originally been magnetizable and that which had been non-magnetizable, were now found to be magnetizable. If this material could be produced at a lower cost, these facts would have a very important bearing. As a mild steel the non-magnetizable material is very fine, having so high a breaking stress for so great an elongation at rupture. Suppose it were used for any purpose for which a mild steel is suitable on account of this considerable elongation at rupture, if exposed to a sharp frost its properties would be completely changed—it would become essentially a hard steel, and it would remain a hard steel until it had been heated to a temperature of about 600° C.

Geological Society, January 22.—W. T. Blanford, F.R.S., President, in the chair.—The following communication was read:—On the crystalline schists and their relation to the Mesozoic rocks in the Lepontine Alps, by Prof. T. G. Bonney, F.R.S. In the debate upon the paper on two traverses of the crystalline rocks of the Alps (read December 5, 1888) it was stated that rocks had been asserted on good authority to exist in the Lepontine Alps, which contained Mesozoic fossils, together with garnets, staurolites, &c., and thus were undistinguishable from crystalline schists regarded by the author as belonging to the presumably Archæan *massifs* of that mountain-chain. In reply the author stated that he regarded this as a challenge to demonstrate the soundness or unsoundness of the hypothesis to which he had committed himself. The present paper gives the result of his investigations, undertaken in the month of July 1889, in company with Mr. James Eccles, to whom the author is deeply indebted for invaluable help. The paper deals with the following subjects:—(1) *The Andermatt Section.* By the geologists aforesaid, a highly crystalline white marble which occurs on the northern side of the Urserenthal trough, at and above Altkirch, near Andermatt, is referred to the Jurassic series (members of which undoubtedly occur at no great distance, almost on the same line of strike). The author describes the relation of the marble to an adjacent black schistose slate, and discusses the significance of some markings in the former which might readily be considered as organic, but to which he assigns a different origin. He shows that there are most serious difficulties in regarding these two rocks as members of the same series, and explains the apparent sequence as the result of a sharp and probably broken infold, as in the case of the admitted band of Carboniferous rock at Andermatt itself. That the section is a difficult one on any hypothesis the author admits, but in regard to the former of these, after a discussion of the evidence, he concludes, "that tendered on the spot demands a verdict of 'not proven'—that obtainable in other parts of the Alps, will compel us to add, 'not provable.' (2) *The Schists of the Val Piora.* These schists, already noticed by the author in his Presidential address to the Society in 1886, occur in force near the Lago di Ritom, and consist of two groups—the one dark mica-schists, sometimes containing conspicuous black garnets, banded with quartzites, the other various calc-mica schists; between them, apparently not very persistent, occurs a schist containing rather large staurolites or kyanites. On the north side is a prolongation of the garnet-actinolite (Tremola-) schists of the St. Gothard and then gneiss, on the south side gneiss. There is also some rauchwacké. This rock, at first sight, appears to underlie the Piora schists, and thus to be the lowest member of a trough. If so, as it is admittedly about Triassic in age, the Piora schists would be Mesozoic. The author shows that (1) the latter rocks do not form a simple fold; (2) they are, beyond all question, altered sediments; (3) they have often been greatly crushed subsequent to mineralization; (4) the garnets, staurolites, &c. (if not injured by subsequent crushing) are well developed and characteristic, and are authigenous minerals. (3) *The Rauchwacké and its Relation to the Schist.* (*a*) *The Val Piora Sections*: The author shows that the rauchwacké, which

[1] See Address to the Institution of Electrical Engineers (NATURE, January 23, p. 274).

at first sight seems to underlie the dark mica-schist, is inconstant in position (on the assumption of a stratigraphical sequence); that its crystalline condition does not resemble that of the schist-series, but is rather such as is common in a rock of its age; that it contains mica and other minerals of derivative origin, and in places rock-fragments which precisely resemble members of the Piora schist series. (*b*) *The Val Canaria Section:* This section, described by Dr. Grubenmann, is discussed at length. It is shown that the idea of a simple trough is not tenable, for identical schists occur above and below the rauchwacké; that there is evidence of great pressure, which, however, acted subsequently to the mineralization of the schists; and that in one place the rauchewacké is full of fragments of the very schists which are supposed to overlie it. (*c*) *Nufenen Pass, &c.:* Other cases, further to the west, are described, where confirmatory evidence is obtained as to great difference in age between the rauchewacké and the schists, and the antiquity of the latter. The apparent interstratification is explained by thrust-faulting. (4) *The Jurassic Rocks, containing Fossils and Minerals.* The author describes the section on the Alp Vitgira, Scopi, and the Nufenen Pass. Here indubitable Belemnites and fragments of Crinoids occur in a dark, schistose, somewhat micaceous rock, which is often very full of "knots" and "prisms" of rather ill-defined external form, something like rounded garnets and ill-developed staurolites. These rocks at the Alp Vitgira appear to overlie, and in the field can be distinguished from the black garnet schists. In one place the rock resembles a compressed breccia, and among the constituent fragments is a rock very like a crushed variety of the black-garnet mica-schist. These Jurassic "schists" are totally different from the last-named schists, to which they often present considerable superficial resemblance; for instance, their matrix is highly calcareous, the other rock mainly consisting of silicates. Some of the associated mica may be authigenous, but the author believes much of it and other small constituents to be derivative. There is, however, a mineral resembling a mica, exhibiting twinning with (?) simultaneous extinction, which is authigenous. The knots are merely matrix clotted together by some undefinable silicate, and under the microscope have no resemblance to the "black garnets." The prisms are much the same, but slightly better defined; they present no resemblance to the staurolites, but may be couseranite, or a mineral allied to dipyre. Hence, though there is rather more alteration in these rocks than is usual with members of the Mesozoic series, and an interesting group of minerals is produced, these so-called schists differ about as widely as possible from the crystalline schists of the Alps, and do not affect the arguments in favour of the antiquity of the latter. In short, they may be compared to rather poor forgeries of genuine antiques. Incidentally the author's observations indicate (as he has already noticed) that a cleavage-foliation had been produced in some of the Alpine schists anterior to Triassic times. After the reading of this paper, Dr. Geikie stated that he had sent to Prof. Heim an abstract of the paper read by Prof. Bonney to the British Association at Newcastle, and Dr. Heim had favoured him with a *résumé* of his views on the subject of the present discussion. Having read a translation of this *résumé*, Dr. Geikie complimented the author on his courage in returning to this difficult ground, but, notwithstanding the arguments so skilfully brought forward that evening, he was not convinced of an error on the part of the Swiss geologists. Even the author's own sections gave some countenance to their views, since the dark garnetiferous schists might quite well be part of the same series as the Belemnite-schists. In metamorphic regions there must be some line, on one side of which fossils are recognizable, on the other not so. In the Alps, as Heim and his associates contend, the Belemnite-schists, which have become markedly crystalline, may be less altered portions of masses from which all trace of fossils has been generally obliterated. Remarks were also made by Mr. Eccles, Mr. Teall, Dr. Irving, Prof. Hughes, the Rev. E. Hill, and Prof. Bonney.

Entomological Society, January 15.—Fifty-seventh Annual Meeting.—The Right Hon. Lord Walsingham, F.R.S., President, in the chair.—An abstract of the Treasurer's accounts, showing that the finances of the Society were in a thoroughly satisfactory condition, was read by Dr. Sharp, one of the Auditors, and the Report of the Council was read by Mr. H. Goss. It appeared therefrom that the Society had lost during the year several Fellows by death and had elected 24 new Fellows; that the volume of Transactions for the year extended to nearly 600 pages, and comprised 23 memoirs, contributed by 20 authors and illustrated by 17 plates; and that the sale of the Society's Transactions and other publications is largely on the increase. It was then announced that the following gentlemen had been elected as Officers and Council for 1890:—President, The Right Hon. Lord Walsingham, F.R.S.; Treasurer, Mr. Edward Saunders; Secretaries, Mr. Herbert Goss and the Rev. Canon Fowler; Librarian, Mr. Ferdinand Grut; and as other Members of Council, Mr. J. W. Dunning, Captain H. J. Elwes, Mr. F. DuCane-Godman, F.R.S., Dr. P. B. Mason, Prof. R. Meldola, F.R.S., Mr. R. South, Mr. Henry T. Stainton, F.R.S., and Mr. Roland Trimen, F.R.S. Lord Walsingham nominated Mr. J. W. Dunning, Captain Elwes and Mr. F. DuCane-Godman, Vice-Presidents for the Session 1890-91, and he then delivered an address. After remarking on the attractive beauty of some of the larger diurnal Lepidoptera, and the brilliant metallic colouring of certain species of Coleoptera, the influence that such magnificent examples of the wealth of design in Nature might have upon artistic taste, and the consequent refinement and increased enjoyment of life, Lord Walsingham referred, in illustration of the practical usefulness of entomological studies, to the successful importation into California of the Australian parasites infesting the scale insect (*Icerya purchasi*), which had proved so noxious to the orange plantations. Through the efforts of Prof. Riley, upwards of 10,000 parasites had been distributed and had since spread very widely, so that in many localities the orange and other trees hitherto thickly infested with this noxious insect had been practically cleared of it by their aid. He also referred to the successful fertilization of red clover in New Zealand by the importation of impregnated queens of the common humble-bee, and to the uses to which the silk produced by various exotic species of Bombycidæ had now been successfully applied. Reference was then made to the investigation instituted by Mr. Francis Galton, F.R.S., and to the experiments of Mr. F. Merrifield, with the view of determining the percentage of hereditary transmission to successive offspring by different generations of successors, and to the valuable auxiliary such experiments and the researches of Prof. Weismann, Mr. Poulton, F.R.S., and others might prove to the study of the laws of heredity, protective resemblance, and natural selection. It was then observed that even if the study of entomology could claim to have conferred no greater benefits upon the human race than to have afforded to many members of our urban population an inducement to improve their minds and recreate their bodies, it would have contributed in no small degree to the sum of human health, happiness, and morality; in connection with these remarks he quoted the words of the Abbé Umhang in the obituary notice of Henri de Peyerimhoff, "J'ai connu plus d'un jeune homme qui s'est passionné pour une branche de l'histoire naturelle, et je n'en ai vu aucun s'écarter du chemin de la vertu et de l'honneur." Attention was then drawn to the enormous numbers of species of Insecta as compared with the numbers of species of other orders of the animal kingdom, and an approximate estimate was made of the extent of the field of entomology, and of its relation to other branches of biological study. In connection with the subject of the principal works in entomology continued or completed during the year, special mention was made of the "Biologia Centrali Americana," by Messrs. Godman and Salvin, and the "Revisio Insectorum Familiæ Mantidarum," by Prof. Westwood. In conclusion, Lord Walsingham referred to the losses by death during the past year of several Fellows of the Society and other entomologists, mention being made of Mr. F. Bond, Dr. Signoret, Mons. Puls, Colonel C. J. Cox, Pastor Holmgren, Dr. Franz Löw, Dr. Karl Venus, and the Rev. J. G. Wood. Votes of thanks having been passed to the President, Secretaries, and Librarian, Lord Walsingham, Mr. H. Goss, Canon Fowler, and Mr. Grut replied.

Linnean Society, January 16.—Mr. J. G. Baker, F.R.S., Vice-President, in the chair.—Mr. Clement Reid exhibited and made some remarks upon a collection of fruit of *Trapa natans*, from the Cromer Forest bed at Mundesley.—Mr. J. G. Baker exhibited and described a collection of cryptogamic plants from New Guinea, upon which Mr. A. W. Bennett and Captain Elwes made some critical remarks.—In the absence of the author, Mr. A. Barclay, a paper was read by Mr. B. D. Jackson on the life-history of a remarkable Uredine on *Jasminum grandiflora*. A discussion ensued in which Mr. A. W. Bennett and Prof. Marshall Ward took part.—This was followed by a paper from Mr. Edward E. Prince, on certain protective provisions in some larval British Teleosteans.

Royal Microscopical Society, January 8.—Rev. Dr. Dallinger, F.R.S., Vice-President, in the chair.—Mr. T. F. Smith exhibited to the meeting, by means of the oxyhydrogen lantern, a series of photomicrographs of various diatoms taken with Zeiss's apochromatic objectives and projection eye-pieces, giving powers of 1000 to 7500 diameters. At the conclusion of the exhibition Mr. Smith presented the series of slides—52 in number—to the Society for future use and reference.—Mr. T. C. White exhibited a specimen of a parasite found in the cockroaches which infest sugar-ships; also a slide containing bacilli in large numbers from a urinary deposit.—A paper by Dr. R. L. Maddox, on a small glass rod illuminator, was read.—Owing to the lateness of the hour, the reading of papers by Mr. Michael and Dr. Czapski was postponed until the March meeting.

Chemical Society, January 16.—Dr. W. J. Russell, F.R.S., in the chair.—The following papers were read:—A new method of estimating the oxygen dissolved in water, by Dr. J. C. Thresh. The process is based on the fact that whereas, in the absence of oxygen, nitrous acid and hydrogen iodide interact, forming iodine, water, and nitric oxide, in the presence of oxygen the nitric oxide becomes re-oxidized, and, serving as a carrier of the oxygen, brings about an additional separation of iodine, equivalent in amount to the oxygen present; hence, deducting the amount of iodine liberated by the nitrous acid and by the oxygen dissolved in the solutions used from the total amount, the difference will be that corresponding to the oxygen dissolved in the water examined. The apparatus required is a very simple one, the analytical operations are conducted in an atmosphere of coal gas, and the results in the case of freshly distilled water agree closely with those recently published by Sir H. E. Roscoe and Mr. Lunt (Chem. Soc. Trans., 1889, 552).—Note on a milk of abnormal quality, by Mr. F. J. Lloyd. The author gave the results of an examination of the milk of two cross-bred shorthorns, and called attention to the abnormally low proportion of solid constituents other than fat.—The sulphates of antimony, by Mr. R. H. Adie.

Zoological Society, January 14.—Prof. A. Newton, F.R.S., Vice-President, in the chair.—The Secretary read a report on the additions that had been made to the Society's menagerie during the month of December 1889.—Mr. Sclater exhibited and made remarks on a specimen of a very singular duck from North-East Asia, apparently referable to the genus *Tadorna*, sent to him for determination by Dr. Lütken, of Copenhagen. After a careful examination Mr. Sclater was inclined to think that it was probably a hybrid between *Tadorna casarca* and *Querquedula falcata*.—Mr. Sclater exhibited and made remarks on a set of small birds' bones obtained from beneath some deposits of nitrate in Southern Peru, transmitted to the Society by Prof. W. Nation.—Mr. David Wilson-Barker exhibited and made remarks on some specimens of Teredos taken from submarine telegraphic cables off the Brazilian coast.—Prof. F. Jeffrey Bell exhibited and made remarks on some living specimens of *Bipalium*, transmitted to the Society by the Rev. G. H. R. Fisk, of Capetown.—A communication was read from Mr. R. Lydekker, containing an account of a new species of extinct otter from the Lower Pliocene of Eppelsheim. The author described part of the lower jaw, which he had previously referred to *Lutra dubia*, from the deposits indicated. Having, however, now seen a cast of the type of the latter, he found that the present specimen indicated a distinct species, for which the name *L. hessica* was proposed.—A communication was read from Prof. Bertram C. A. Windle and Mr. John Humphreys, on some cranial and dental characters of the domestic dog. The paper was based on the results of the measurements of a large number of dogs' skulls of various breeds. Its object was to ascertain whether cranial and dental characteristics afforded sufficient information to permit of a scientific classification of the breeds, or would throw any light upon their origin. The conclusion so far arrived at was that interbreeding had been so extensive and complicated as to make it impossible to distinguish the various forms scientifically from the characters examined. Several points with regard to the shape of head and palate and the occasional occurrence of an extra molar were also touched upon.—Mr. G. A. Boulenger read the fourth of his series of contributions to the herpetology of the Solomon Islands. The present memoir gave an account of the last collection brought home by Mr. C. M. Woodford. Besides known species, this collection contained examples of a new snake, proposed to be called *Hoplocephalus elapoides*.—A second paper by Mr. Boulenger contained a list of the reptiles, batrachians, and freshwater fishes collected by Prof. Moesch and Mr. Iversen in the districts of Delhi and Langkat, in North-Eastern Sumatra.—Dr. Günther, F.R.S., read a paper entitled "A Contribution to our Knowledge of British Pleuronectidæ." The author described the true *Arnoglossus grohmanni*, a Mediterranean scald-fish, recently discovered by the Rev. W. S. Green on the Irish coast, and quite distinct from *Arnoglossus lophotes*. Dr. Günther also stated that the Mediterranean lemon-sole (*Solea lascaris*) was specifically identical with the British species (*Solea aurantiaca*), but was distinct from that of the Canary Islands and Madeira (*Solea scriba*); and gave it as his opinion that the Mediterranean *Solea lutea* and British *Solea minuta* cannot be separated by any constant character.

EDINBURGH.

Royal Society, January 6.—Lord Maclaren, Vice-President, in the chair.—Bailie Russell read an obituary notice of the late Sir James Falshaw, Bart.—Prof. Tait read a paper on the effect of friction on vortex-motion.—Dr. A. Bruce described a connection (hitherto undescribed) of the inferior olivary body of the medulla oblongata, which has a function in the maintenance of equilibrium of the body.—Dr. W. H. Perkin read a paper on the internal condensation of some diketones.—A photograph of a group of sun-spots and of the surface of the sun was presented by Mr. James Naismith. The photograph was from a drawing made in 1864.

PARIS.

Academy of Sciences, January 27.—M. Hermite in the chair.—On clasmatocytes, by M. L. Ranvier. The author gives this name (from κλάσμα, fragment, and κύτος, cell) to certain elements which are easily detected under the microscope in the thin connective membranes of the vertebrates when they are prepared by a process here described. They are not migratory cells, but have their origin in the leucocytes, or lymphatic cells, though it is not to be supposed that all leucocytes develop into clasmatocytes.—On the theorem of Euler in the theory of polyhedrons, by M. de Jonquières. The paper deals with Lhuilier's objection, accepted by Gergonne, against the generalization of Euler's formula, which is shown to be applicable to all polyhedrons, whether convex or not. It is further placed beyond doubt that Euler not only enounced, but gave a full demonstration of the formula in question.—On the roots of an algebraic equation, by Prof. A. Cayley. Assuming $\int(u)$ to be a rational and integral function, with real or imaginary coefficients, of the n order; and supposing that the equation $\int'(u) = 0$, of the order $n - 1$, has $n - 1$ roots, then it is shown that the equation $\int(u) = 0$ will have n roots. The demonstration rests on the same principles as those of Gauss and Cauchy.—Researches on the cultivation of the potato, by M. Aimé Girard. The author communicates the results of his experiments, continued for three years at the Ferme de la Faisanderie, Joinville-le-Pont, with the variety of the potato known as Richter's Imperator, which is shown to yield a far larger crop of starch-bearing tubers than any other variety cultivated in France. The paper was supplemented by some remarks by M. P. P. Dehérain, who stated that his own experiments fully confirmed those of M. Girard. There could be no doubt as to the great superiority of Richter's Imperator, especially as a starch-producing tuber.—Remarks on the *Annuaire du Bureau des Longitudes* for 1890, by M. Faye. In presenting a copy of this valuable annual for 1890, M. Faye remarked that the astronomic section of the work became more important every year. The present volume contains a table of the planetary phenomena, the most accurate available data for the variable stars, a catalogue of the chief stars whose magnitudes correspond to Pickering's photometric scale, papers on the use of the aneroid barometer, on the elasticity of solids and the neutral temperature of thermo-electric couples, together with the magnetic elements for France and its seaports on January 1, 1890, and at various Mediterranean stations for 1887.—On the simply rational transformations of algebraic surfaces, by M. Paul Painlevé. In this paper the author extends to the transformations in question M. Picard's method relative to the birational transformations of algebraic surfaces.—On the substitution of the salts in mixed solutions, by M. A. Etard. In his previous researches the

author determined the lines of complete solubility for a mixture of potassium and sodium chlorides, varying the quantity of the *metals* saturated by the same metalloid as a function of the temperature. He studies the reverse case here, determining the results when in a solution of the same metal the *metalloids* are varied.—On the different states of iodine in solution, by MM. Henri Gautier and Georges Charpy. Iodine solutions are usually divided into two classes—brown (alcohol, ether, &c.) and violet (sulphur of carbon, chloroform, benzine, &c.). The molecular weights have been determined by Raoult's method, and results were obtained varying from 330 to 489, according to the solvent; Loeb's results are thus confirmed and amplified. —Calorimetric study of the phosphites and pyro-phosphite of soda, by M. L. Amat. These researches fully confirm the author's previous conclusion that the acid phosphite of soda, $PO_3H.NaH$, may, by the simple process of drying, lose water and become transformed into pyrophosphite of soda, a substance differing in many of its properties from the acid phosphite.—A study of the pneumococcus occurring in the fibrine pneumonia consecutive to *la grippe* (influenza), by MM. G. Sée and F. Bordas. From these clinical researches, made on a large number of patients in the Hôtel-Dieu, the authors conclude that pneumonia is not only a local affection caused by infection, but that it is itself infecting in the sense that it may invade other organs.—Papers were read by M. Chr. Bohr, on pulmonary respiration; by M. Abel Dutartre, on the poison of the land salamander; by M. Ch. Musset, on "selenotropism" (influence of moonlight on plants); by M. A. de Schulten, on the artificial reproduction of malachite all but identical in density, hardness, and crystallization with the natural stone; by M. A. de Grossouvre, on the presence of Alpine fossils in the Callovian formation of the west of France; and by M. Ch. V. Zenger, on the magnetic storms and auroræ boreales of the years 1842–57.

DIARY OF SOCIETIES.

LONDON.

THURSDAY, FEBRUARY 6.

ROYAL SOCIETY, at 4.30.—A New Theory of Colour-blindness and Colour-perception: Dr. Edridge Green.—Memoir on the Symmetrical Functions of the Roots of Systems of Equations: Percy A. MacMahon, Major R.A.

LINNEAN SOCIETY, at 8.—On the Stamens and Setæ of Scirpeæ: C. B. Clarke, F.R.S.—On the Flora of Patagonia: John Ball, F.R.S.

CHEMICAL SOCIETY, at 8.—Ballot for the Election of Fellows.—The Oxides of Nitrogen: Prof. Ramsay, F.R.S.—Studies on the Constitution of Tri-Derivatives of Naphthalene: Dr. Armstrong and W. P. Wynne.—On the Action of Chromium Oxychloride on Nitrobenzole: G. G. Henderson and J. Morrow Campbell.

ROYAL INSTITUTION, at 3.—Sculpture in Relation to the Age: Edwin Roscoe Mullins.

FRIDAY, FEBRUARY 7.

PHYSICAL SOCIETY, at 5.—Annual General Meeting.—On Galvanometers: Prof. W. E. Ayrton, F.R.S., T. Mather, and W. E. Sumpner.—On a Carbon Deposit in a Blake Telephone Transmitter: F. B. Hawes.

GEOLOGISTS' ASSOCIATION, at 7.30.—Annual General Meeting.—Notes on the Nature of the Geological Record: The President.

SOCIETY OF ARTS, at 5.—The Utility of Forests and the Study of Forestry: Dr. Schlich.

INSTITUTION OF CIVIL ENGINEERS, at 7.30.—Reclamation of Land on the River Tees: Colin P. Fowler.

ROYAL INSTITUTION, at 9.—The London Stage in Elizabeth's Reign: Henry B. Wheatley.

SATURDAY, FEBRUARY 8.

ROYAL BOTANIC SOCIETY, at 3.45.

ROYAL INSTITUTION, at 3.—The Natural History of the Horse, and of its Extinct and Existing Allies: Prof. Flower, C.B., F.R.S.

MONDAY, FEBRUARY 10.

ROYAL GEOGRAPHICAL SOCIETY, at 8.30.—Search and Travel in the Caucasus; an Account of the Discovery of the Fate of the Party lost in 1888: Douglas W. Freshfield (illustrated by Photographs by Signor V. Sella and H. Woolley).

SOCIETY OF ARTS, at 8.—The Electromagnet: Dr. Silvanus P. Thompson.

TUESDAY, FEBRUARY 11.

ANTHROPOLOGICAL INSTITUTE, at 8.30.—Exhibition of some Skulls, dredged by G. F. Lawrence from the Thames, in the Neighbourhood of Kew: Dr. Garson.—Characteristic Survivals of the Celts in Hampshire: T. W. Shore.

SOCIETY OF ARTS, at 8.—Cast Iron and its Treatment for Artistic Purposes: W. R. Lethaby.

INSTITUTION OF CIVIL ENGINEERS, at 8.—Bars at the Mouths of Tidal Estuaries: W. H. Wheeler.

ROYAL INSTITUTION, at 3.—The Post-Darwinian Period: Prof. G. J. Romanes, F.R.S.

UNIVERSITY COLLEGE BIOLOGICAL SOCIETY, at 5.15.—Some Aberrant Coleoptera: S. V. Tebbs.

WEDNESDAY, FEBRUARY 12.

ROYAL MICROSCOPICAL SOCIETY, at 8.—Annual Meeting.—President's Address.

SOCIETY OF ARTS, at 8.—Modern Improvements in Facilities for Railway Travelling: George Findlay.

THURSDAY, FEBRUARY 13.

ROYAL SOCIETY, at 4.30.

MATHEMATICAL SOCIETY, at 8.—Concerning Semi-invariants: S. Roberts, F.R.S.—Ether-Squirts: Prof. K. Pearson.

INSTITUTION OF ELECTRICAL ENGINEERS, at 8.

ROYAL INSTITUTION, at 3.—The Three Stages of Shakspeare's Art: Rev. Canon Ainger.

FRIDAY, FEBRUARY 14.

ROYAL ASTRONOMICAL SOCIETY, at 3.—Anniversary Meeting.

ROYAL INSTITUTION, at 9.—Problems in the Physics of an Electric Lamp; Prof. J. A. Fleming.

SATURDAY, FEBRUARY 15.

ROYAL INSTITUTION, at 3.—Electricity and Magnetism: Right Hon. Lord Rayleigh, F.R.S.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Medical Electricity and Massage: H. N. Lawrence (Gill).—A Theory of Lunar Surfacing by Glaciation: S. E. Peal (Thacker).—Einleitung in die chemische Krystallographie: Dr. A. Fock (Leipzig, Engelmann).—Elemente der Paläontologie, 2 Hälfte: Dr. G. Steinmann and Dr. L. Doderlein (Leipzig, Engelmann).—L'Évolution du Système Nerveux: H. Beaunis (Paris, J. B. Baillière).—A Theory of Gravitation: T. Wakelin (Petherick). —The Psychology of Attention: T. Ribot (Chicago, Open Court Publishing Company).—English Intercourse with Siam in the Seventeenth Century: Dr. J. Anderson (K. Paul).—Contributions to the Fauna of Mergui and its Archipelago, 2 vols. (Taylor and Francis).—Report of the Commissioner of Education for the Year 1887–88 (Washington).—The Library Reference Atlas of the World: J. Bartholomew (Macmillan).—Science and Scientists: Rev. J. Gerard (London).—Le Climat de la Belgique en 1889: A. Lancaster (Bruxelles).—Tylar's Practical Hints and Photographic Calendar, 1890 (Tylar, Birmingham).—Results of Astronomical Observations made at the Melbourne Observatory in the Years 1881–84 (Melbourne).—Babbage's Calculating Engines (Spon).—Practical Hints for Electrical Students, vol. i: Kennelly and Wilkinson (*Electrician* Office).—Lehrbuch der Meteorologie: Dr. W. J. Van Bebber (Stuttgart, Enke).—Is the Copernican System of Astronomy True?: W. S. Cassedy (Kittanning, Pa.).—New Zealand for the Emigrant, Invalid, and Tourist: J. M. Moore (S. Low).—Fauna der Gaskohle und der Kalksteine der Permformation Böhmens, Band 2, Heft 4: Dr. Ant. Fritsch (Prag).—The Extermination of the American Bison: W. T. Hornaday (Washington).—Iowa Weather Report, 1878-79-80-82-83-84-85-87 (Des Moines, Iowa).—U.S. Commission of Fish and Fisheries; Part XIV., Report of the Commissioner for 1886 (Washington).—Report on Insect and Fungus Pests, No. 1: H. Tryon (Brisbane, Beal).—La Photographie à la Lumière du Magnésium: Dr. J. M. Eder (Paris, Gauthier-Villars).—Notes upon a Proposed Photographic Survey of Warwickshire: W. J. Harrison (Birmingham).—Chinese Games with Dice: S. Culin (Philadelphia).—Ancient Symbolism among the Chinese: Dr. J. Edkins (Trübner).—Journal of the Royal Statistical Society, December (Stanford).—Charts showing the Normal Monthly Rainfall in the United States (Washington).

CONTENTS.

THURSDAY, FEBRUARY 13, 1890.

RELIGIOUS INSTITUTIONS OF THE SEMITES.

Lectures on the Religion of the Semites. The Fundamental Institutions. By W. Robertson Smith. (Edinburgh: Black, 1889.)

THE volume before us contains the first series of lectures on "the primitive religions of the Semitic peoples, viewed in relation to other ancient religions, and to the spiritual religion of the Old Testament and of Christianity," which the Trustees of the Burnett Fund asked Prof. Robertson Smith to deliver at Aberdeen in the year 1887. As may be readily imagined, the selection of Prof. R. Smith as lecturer on the subject which, of all men in England, he had made peculiarly his own, was approved of by Semitic scholars and by the more liberal-minded of the clergy of all denominations. There were and are, of course, many who will view the publication of these lectures in a book form with anything but favour; still it is quite certain that they must, if honestly read and candidly thought over, bring many of this class over to the view, which is gaining ground with great rapidity, that, if the Hebrew Scriptures are to be properly understood by us, and their value accurately gauged, we must bring to their consideration the same amount of commonsense, the same critical investigation, and the same weighing of evidence, which we should bring to bear upon any piece of general history. The Bible is a unique work, and is the production of many writers who lived at different periods. In it we have a mixture of historical facts fused with legend, poetry, folk-lore, stories, and traditions, deeply devotional religious hymns, prophecies, and descriptions of scenes in the life and history of the sons and descendants of Abraham. Anyone who knows the Oriental character will understand at once why the book is such a favourite with the Eastern Semites, and will see that it is precisely the kind of work which their writers could not help producing; it is the greatest mistake possible, however, to assume that the book could only be the production of a certain branch of the Semitic race. This is what has been thought for centuries by clergy and laity alike, and as a result its value has been much underrated and its evidence only partly understood; also, for hundreds of years the value of the Hebrew text from the point of view of comparative philology was rendered useless because a powerful section of the Church declared that the vowel-points were an integral part of the text itself, and not an addition to it made by the Rabbis of Tiberias because the true pronunciation of the language was dying out and was not generally understood. The Bible has lost nothing in the eyes of scholars because it has been proved that the vowel-points are not fourteen hundred years old, and that the learned men who added the points made mistakes themselves! It is hard to say what provoked the intense opposition of certain sects of the Church a few years ago to historical research as applied to the New Testament. It may be that the *manner* in which the German philologists and commentators carried on their investigations, and expressed their opinions, caused the narrow-minded, and we may add unlearned, theologians of the English Church to abhor and detest all such works; nevertheless, we venture to believe that, in spite of all the so-called destructive criticism of Kuenen and Wellhausen, the Bible has gained more by the labours of the critical school, of which these two scholars are brilliant examples, than it has lost. It is but a few years since Prof. Robertson Smith defended his views on historical research as applied to the Old Testament before the courts of his Church, in which bigotry and ignorance of modern research were curiously blended, and in a very few years it will be difficult to believe that such a trial—the only result of which was the loss to his Church of its most learned member—ever took place.

The lectures printed in the first volume of Prof. Robertson Smith's work are eleven in number, and they relate to the fundamental institutions of the Semitic race as a whole, viz. sanctuaries, sacrifices, first-fruits, tithes, the blood covenant, fire sacrifices, sacrificial gifts, &c. The introductory lecture explains clearly the method of inquiry into the subject, and states the lines upon which this inquiry is to be based. Practically speaking, Prof. Robertson Smith says:—We have the Bible with its remarkable accounts of the institutions of the ancient Jews, and of the ancestors of these Jews. We want to find out a great deal more about them than is stated in it, because the writers, taking for granted that its readers would understand not only their arguments but the facts which led up to them, and the history and manners and customs of the race to which they belonged, only made sufficient reference to them to make the point under discussion perfectly clear. The Jews were a small nation, belonging to the great Semitic race, which had a great deal in common with the other peoples of the race, viz. Assyrians, Babylonians, the dwellers of Syria, &c., whom we have been taught to look upon as heathen outside the pale of the salvation of the Jewish God. Now the Jews have left behind them fewer remains than any other nation belonging to the great Semitic race; the other nations of this race, however, have left behind them inscriptions, buildings, books, &c., the study of which will cast much light upon the manners and customs of the peoples described in the Old Testament. The last sixty years have made us acquainted with the languages which these people spoke, we have learned the relationships of these nations to each other, we have certain fixed points in their chronology, and we know a great deal about their religion and their public and private life. Let us then compare the records of all these various families of the Semitic race, and see how much they have in common, where they differ, and if possible let us try and find out how they differ. With a mind well stocked by the study of the native records of the great Semitic nations, Prof. Robertson Smith begins this difficult task. At the outset he distinguishes between Judaism, Christianity, and Islam, which he calls *positive* religions, and the systems of ancient heathenism. Each of the positive religions, however, was built upon the beliefs and customs of ancient heathenism, and we can only understand a system of positive religion when we understand the principles of the religion which preceded it. The Hebrews had many religious conceptions and usages in common with many kindred peoples; and as the matter is pithily put by

Prof. Robertson Smith, "those who had no grasp of spiritual principles, and knew the religion of Jehovah only as an affair of inherited usage, were not conscious of any great difference between themselves and their heathen neighbours, and fell into Canaanite and other foreign practices with the greatest facility. . . . Traditional religion is handed down from father to child, and therefore is in great measure an affair of race. Nations sprung from a common stock will have a common inheritance of traditional belief and usage in things sacred as well as profane, and thus the evidence that the Hebrews and their neighbours had a large common stock of religious tradition falls in with the evidence which we have from other sources, that in point of race the people of Israel were nearly akin to the heathen nations of Syria and Arabia." Prof. Robertson Smith, in common with the general opinions of the best scholars, is inclined to place the original home of the Semitic race in the Arabian peninsula, and it is pretty certain that, from time immemorial, the tract of land bounded by the Mediterranean on the west, Persia on the east, the Armenian mountains on the north, and the Indian Ocean on the south, was peopled by tribes who spoke Semitic dialects. It must not be forgotten that the so-called Babylonians had their territory invaded by a horde of warlike but intelligent men from the east who eventually succeeded in imposing upon them the cuneiform writing. After all the nonsense which has been talked during the last few years about the so-called "Hittites" being identical with the Hittites of the Bible, it is refreshing to find a scholar like Prof. Robertson Smith stating plainly that the "Hittites of the Bible . . . were a branch of the Canaanite stock, and that the utmost concession that can be made to modern theories on this subject is that they may for a time have been dominated by a non-Semitic aristocracy." It is as well to say at once that no successful attempt has yet been made to decipher the "Hittite" inscriptions, and none can be made until a bilingual inscription has been found. The "boss" of Tarkondemos is, no doubt, a forgery; but, even granting that it is not, no one can certainly say what or how many of the signs in the centre of the "boss" represent one of the words in cuneiform around it.

Prof. R. Smith is quite right not to place too much trust in the traditions of the Babylonian religion as made known to us by the cuneiform inscriptions. It is true that these are the oldest Semitic inscriptions known to us, but it is to be remembered that the writing itself and many of the religious myths and traditions known to the Babylonians were either forced upon them by, or borrowed from, their conquerors from the east. Just as the Arabic language is the right point to start from in the study of comparative Semitic mythology, so the traditions of the old, heathen inhabitants of Arabia are those which must form the ground-work of any comparative inquiry into the traditions of Semitic religion generally. The remainder of the first lecture is occupied with general statements of an important nature, which no reviewer could do justice to in an ordinary review. Lecture II. describes the primitive Semitic society and its religion; the oldest Semitic communities and their gods; the fatherhood of the gods, and the kinship of gods and men; monarchy and monotheism, &c. Lecture III. discusses the gods, jinn, totems, and Semitic totemism; Lecture IV., holiness, taboo, the sanctuary, and the jealousy of the god; Lecture V., sanctuaries, holy waters, trees, caves, and stones; Lecture VI., sacrifice in all its various forms; Lecture VII., first-fruits, tithes, and sacrificial meals; Lecture VIII., the original significance of animal sacrifice; Lecture IX., the sacrificial efficacy of animal sacrifice, the blood covenant, &c.; Lecture X., the development of sacrificial ritual and fire sacrifices; Lecture XI., the special ideas involved in piacular sacrifices. A series of "additional notes" (A—N) and a good index complete the volume. Prof. Robertson Smith's arguments are sound, and they are carefully reasoned out; but as new material comes to hand some of the details may require alteration. The work deserves the careful study of all scholars who are anxious to meet with a straightforward, unbiassed statement upon the difficult subject of ancient Semitic religion; where it has been necessary to combat opposite opinions, the discussion is carried on with fairness to the scholars concerned, and consequently with credit to the author of these lectures. The works of Kuenen, Wellhausen, and Goldziher, repel, rather than attract, many readers; we do not imagine that any honest seeker after truth, be he theologian or lay reader, will turn away from the perusal of these lectures, having once begun to read them. It is to be hoped that Bible commentators will at once embody in their works the explanations of the large number of Scriptural passages which have, up to the present, been simply not to be understood. It is also to be hoped that Prof. R. Smith will soon be enabled to give to the world the concluding part of his valuable work, the publication of which is a sign of the times in England.

ALGEBRA.

Algebra: an Elementary Text-book for the Higher Classes of Secondary Schools and for Colleges. By G. Chrystal, M.A. Part II. (Edinburgh: Adam and Charles Black, 1889.)

THE work before us is the realization of the hope with which we concluded our notice of the first part (NATURE, vol. xxxiv. p. 614).

The author apologizes for the delay in its appearance. The occupation of a busy life would be to most men a sufficient *raison d'être* for such delay, and to this has been added a further source of delay arising from circumstances of a private character. Students, however, have gained hereby, for the work has grown in the progress of its construction. It has not, "as some one prophesied, reached ten volumes," for this is the concluding volume; but it has, we are told, cost the writer infinitely more trouble than he expected. The first instalment extended to 542 pages; this one, with answers and index of names (which we are glad to have), is comprised in 588 pages. The prominent features of the exposition as to its "singular ability and freshness of treatment" are as conspicuous here as in Part I., and we need not repeat the praise which we accorded to it (*l.c.*).

Let us hearken to Prof. Chrystal, for he always writes to the point:—

"The main object of Part II. is to deal as thoroughly as possible with those parts of algebra which form, to

use Euler's title, an 'Introductio in Analysin Infinitorum.' A practice has sprung up of late (encouraged by demands for premature knowledge in certain examinations) of hurrying young students into the manipulation of the machinery of the differential and integral calculus before they have grasped the preliminary notions of a *limit* and of an *infinite series*, on which all the meaning and all the uses of the infinitesimal calculus are based. Besides being to a large extent an educational sham, this course is a sin against the spirit of mathematical progress. The methods of the differential and integral calculus, which were once an outwork in the progress of pure mathematics, threatened for a time to become its grave. Mathematicians had fallen into a habit of covering their inability to solve many particular problems by a vague wave of the hand towards some generality, like Taylor's theorem, which was supposed to give 'an account of all such things,' subject only to the awkwardness of practical inapplicability. Much has happened to remove this danger and to reduce d/dx and $\int dx$ to their proper place as servants of the pure mathematician. . . . For the proper understanding of this important branch of modern mathematics [*i.e.* function-theory], a firm grasp of the doctrine of limits and of the convergence and continuity of an infinite series is of much greater moment than familiarity with the symbols in which these ideas may be clothed. It is hoped that the chapters on inequalities, limits, and convergence of series [chapters xxiv.–xxvi.], will help to give the student all that is required both for entering on the study of the theory of functions and for rapidly acquiring intelligent command of the infinitesimal calculus. In the chapters in question, I have avoided trenching on the ground already occupied by standard treatises: the subjects taken up, although they are all important, are either not treated at all or else treated very perfunctorily in other English text-books."

No student who masters the present treatise will pass such judgment upon these chapters, or, indeed, upon any part of the work. What the writer aims at, and succeeds in achieving, is thoroughness.

The first part occupied twenty-two chapters; the second part occupies chapters xxiii.–xxxvi.

Following on the lines of our previous notice (*l.c.*), we give a brief analysis of the chapters:—23, permutations and combinations (with applications to binomial and multinomial theorems, distributions and derangements, and the theory of substitutions); 24–26, see extract above; 27, binomial and multinomial theorems for any index: 28, exponential and logarithmic series (with an account, and applications, of Bernoulli's numbers); 29, 30, summation of the fundamental power-series for complex values of the variable, and general theorems regarding the expansion of functions in infinite forms—these are two splendid chapters, which the author says

"may be regarded as an elementary illustration of the application of the modern theory of functions. They are intended to pave the way for the study of the recent works of Continental mathematicians on the same subject. Incidentally, they contain all that is usually given in English works under the title of analytical trigonometry. If anyone should be scandalized at this traversing of the boundaries of English examination subjects, I must ask him to recollect that the boundaries in question were never traced in accordance with the principles of modern science, and sometimes break the canon of common-sense. . . . The timid way, oscillating between ill-founded trust and unreasonable fear, in which functions of a complex variable have been treated in some manuals, is a little discreditable to our intellectual culture. Some expounders of the theory of the exponential function of an imaginary argument, seem even to have forgotten the obvious truism that one can prove no property of a function which has not been defined."

Chapter 30, moreover, closes with "a careful discussion of the reversion of series and of the expansion in power-series of an algebraic function—subjects which have never been fully treated before in an English text-book, although we have in Frost's *curve-tracing* an admirable collection of examples of their use" (this is a work often referred to with high commendation in the text). To resume our analysis, chapter 31 is on the summation and transformation of series in general; 32–34 gives a thorough discussion of continued fractions and their applications; 35 gives numerous general properties of integral numbers; and 36 is on probability, or the theory of averages. In this last chapter the author has "omitted certain matter of doubtful soundness and of questionable utility; and filled its place by what I hope will prove a useful exposition of the principles of actuarial calculation."

The student of the present day knows that "things are not always what they seem," so when he hears that an elementary text-book of algebra occupies more than a thousand octavo printed pages, he is prepared to find that the "elementary" is comparative, and the "algebra" comprises some other subjects, in ordinary parlance, called by other names. He will find the present work most readable, provided he comes to the perusal with the requisite knowledge and ability, and when he has got to the end of the course he will have an excellent foundation for all his after mathematical reading. Prof. Chrystal gives good advice, which we copy. "When you come on a hard or dreary passage, pass it over; and come back to it after you have seen its importance or found the need for it further on. To facilitate this skimming process, I have given, after the table of contents, a suggestion for the course of a first reading." There are numerous "historical notes," which form a conspicuous and useful feature of the whole work.

The author uses the expression (see above) "dreary passage": we have not come across these, but we can certify with regard to the first part, that we have taken it up again and again, and have always found it difficult to rest contented with a brief glance, and the part before us appears, in some respects, to be even more attractive.

FERMENTATION WITH PURE YEAST.

The Micro-organisms of Fermentation, practically considered. By Alfred Jörgensen. Edited from the German by G. Harris Morris, Ph.D., F.C.S., F.I.C., &c. With an Introduction by Horace T. Brown, F.C.S., F.I.C. (London: F. W. Lyon, 1889.)

DURING the past ten years in which the investigation of micro-organisms and their functions has been so actively pursued there has been a conspicuous absence of any work dealing with the progress made in our knowledge of those particular forms which are of industrial importance. Thus whilst numerous text-books in various languages have appeared embodying the latest discoveries in the relationship of micro-organisms to disease, the only noteworthy treatise on the technological side of

bacteriology since Pasteur's "Etudes sur le Vin, le Vinaigre, et la Bière," the last of which was published in 1876, is Alfred Jörgensen's "Micro-organismen der Gährungsindustrie" (1886), of which the volume before us is an edited translation. This lack of text-books is doubtless in great measure due to the industrial aspects of micro-organisms having been comparatively neglected during the time that Pasteur, Koch, and their numerous disciples have been busily engaged in the investigation of questions of still more absorbing human interest. But whilst the great majority of bacteriologists have during this past decade been thus occupied in establishing or endeavouring to establish the connection between numerous diseases and specific organisms, a few more silent workers have been patiently engaged upon the less sensational though no less arduous task of placing the fermentation industries on a more scientific basis, adding in fact to the structure which had been commenced by Pasteur in his "Etudes" referred to above. The foremost in this field of research has unquestionably been Christian Hansen of the now world-famed Carlsberg Laboratory near Copenhagen, and to a concise and most lucid description of whose successful labours the present volume is chiefly devoted. The principal addition which has been made to our knowledge of the fermentation organisms by Hansen has been the precise characterization of a number of different "races" of yeast and the determination of the specific features of the fermentation induced by each particular race. Thus whilst Pasteur attributed the various diseases in wine and beer to the presence of organisms other than yeast, Hansen has shown that certain races of yeast itself are capable of bringing about most serious disturbances in the fermentation process. The lines on which Hansen has differentiated these several races of yeast, and the methods by which their pure culture may be effected are clearly though briefly described in this work, with which latest developments of brewing technology, both the author and translator have already identified themselves in the past.

The influence which has been exerted by the researches of Pasteur and Hansen on the practical conduct of the fermentation industries is quite analogous to that which has resulted in surgery from the investigations of Lister and Koch, in both cases the principle of rigid scientific cleanliness has become the order of the day. Thus we read, "the air in the fermenting-room may contain a world of germs which, in the fermentation industries, bring with them the most calamitous results; it is, however, possible to obtain air free from these invisible germs, and it admits of no doubt that, on the one hand, the purification of the air in the fermenting-room by passing it through a salt-water bath, and, on the other hand, the most rigidly executed order and cleanliness in the cellars of the Old Carlsberg brewery, stand in direct relation to the results."

From a practical point of view, the chief merit due to Hansen is that he has not only shown how pure growths of yeast may be obtained in the laboratory, but that he has further devised methods by which these pure cultures may actually be employed on the largest brewery scale. This brewing with pure yeast has already assumed very large dimensions on the Continent where a continually increasing number of breweries receive regular supplies of pure material. We have ourselves visited the laboratories of the Wissenschaftliche Stationen für Brauerei und Brennerei at Berlin and at Munich, and can testify to the impressiveness of witnessing the careful preparation on the manufacturing scale of different forms of pure yeast, each possessed of specific fermenting properties, which are then transmitted to various parts of Europe according to the special requirements of different breweries. These experimental brewing-stations, like so many other similar institutions on the Continent, are directly or indirectly subsidized by the State and number amongst their staff men of universal reputation in their particular departments. As we should anticipate, this method of scientific brewing with pure yeast has so far taken no root in this country, although we are glad to know that the translator, along with Mr. Horace Brown, has for some time past been engaged upon its experimental trial, and we learn from the latter in his introduction to this book "that, in a more or less modified form, pure yeast culture will play a very important part in the brewing of the future in this country."

This little work, which is condensed into 166 pages, and profusely illustrated and provided with an admirable bibliography, should receive the most careful attention from practical men, for whom it is mainly intended. Even the purely scientific student will find much in its pages that should prove of service to him.

PERCY F. FRANKLAND.

OUR BOOK SHELF.

An Epitome of the Synthetic Philosophy. By F. Howard Collins, with a Preface by Herbert Spencer. (London: Williams and Norgate, 1889.)

THE aim and scope of this work cannot be more tersely or more accurately conveyed than by quoting *in extenso* the "compiler's preface."

"The object of this volume is to give in a condensed form the general principles of Mr. Herbert Spencer's Philosophy as far as possible in his original words. In order to carry out this intention each section (§) has been reduced, with but few exceptions, to one-tenth; the five thousand and more pages of the original being thus represented by a little over five hundred. The Epitome consequently represents 'The Synthetic Philosophy' as it would be seen through a diminishing glass: the original proportion holding between all its varied parts.

"Should this volume lead the general reader to a better acquaintance with Mr. Spencer's own works, I shall feel amply repaid for my labour.

"My warmest thanks are due to Mr. Spencer for his invaluable preface; and also to Miss Beatrice Potter, and Mr. Henry R. Tedder, F.S.A., the able and accomplished secretary and librarian of the Athenæum Club, for their valuable suggestions while the work has been in progress."

The desirability of such an undertaking, supposing it to have been successfully accomplished, is both manifest and manifold. Mr. Spencer's works are so voluminous that it is impossible to acquire a knowledge of his system of philosophy as a whole without devoting to it an expenditure of time which is practically impossible for most men who are not specially engaged in philosophic studies. Moreover, even to a reader who is thus specially engaged, and who therefore desires fully to master this system, no small difficulty is experienced from the fact that hitherto there has not been so much as an index to guide his

studies through these reams and reams of paper. Consequently, the first class of readers have hitherto for the most part been satisfied to gain their knowledge of Spencer through the "Cosmic Philosophy" of Fiske, while the latter class have experienced a hitherto hopeless difficulty in refreshing their memories upon particular points, or in finding passages to which they may wish to refer in publications of their own. Speaking for ourselves, we are conscious of often having done a negative injustice to Mr. Spencer on this account, simply because, in order to avoid the possibility of any positive injustice in the way of misrepresentation, we have deemed it wisest not to allude to him at all.

Now, the epitome which Mr. Howard Collins has supplied so admirably satisfies all the requirements of the case that henceforth the general reader will be able to acquire a clear knowledge of Mr. Herbert Spencer's philosophy in one-tenth of the time that it has hitherto been necessary to expend, while—as Mr. Spencer himself observes in his highly commendatory preface—more serious students will find that "a clear preliminary conception is more readily obtained from a small outline-map than from a large one full of details." Lastly, for all purposes of reference, this epitome leaves nothing to be desired; for not only does it run parallel with the original —chapter by chapter and section by section—but it is also furnished at the end with an alphabetical index of subject-matter: so that, if a man is writing upon any of the innumerable topics which Mr. Spencer has handled, he can immediately ascertain all that Mr. Spencer has said with regard to them.

For these reasons we cordially recommend this most painstaking epitome to every class of readers; and we cannot doubt that its publication will greatly promote the diffusion of Mr. Spencer's thought in all the English-speaking communities of the world. G. J. R.

The Earth and its Story. Edited by Robert Brown, Ph.D., F.L.S. (London: Cassell and Co., 1889.)

THE continued publication of good and popularly written scientific works is one of the most gratifying signs of the times; it testifies, in no uncertain manner, to the growth of a taste for scientific knowledge in the mind of the general public, and hence is a matter of congratulation.

Of all the sciences none may perhaps be made more interesting than physical geography, or its modern equivalent physiography. The desire to know something about the earth's position in the universe, its formation, and its inhabitants, is and always has been innate in man, and we are glad, therefore, to welcome works that may satisfy this craving after light. The one before us deals in a comprehensive manner with the geographical distribution of plants and animals, and the agents concerned in their dispersion; with the physics of the sea, waves, currents, and tides; with terrestrial magnetism; climate and the causes affecting its distribution; rainfall and precipitation in general. A considerable amount of space is given to descriptions of geological formations and the fossils they contain, whilst ideal landscapes with restored animals are plentifully figured. We regret, however, that only a very meagre description is given of the earth as a planet. It must be remembered that astronomy is a very important part of physiography, even when looked at from a utilitarian point of view. The reason why the movements of the heavenly bodies have been studied from time immemorial is that a knowledge of them was necessary in order to meet the vicissitudes of life, and even before primitive man had inquired into the constitution of the earth he had arrived at crude conceptions as to the constitution of the universe from uncritical observations of celestial phenomena. The priority of these conceptions demonstrates their importance, and therefore, in a work intending to convey earth knowledge, the verification of the earth's rotation and revolution and the determination of its true size and shape should certainly be included. The measurements of arcs of meridian, whereby the exact size and shape of the earth may be found, are easy to describe, and preferable to the proofs of the earth's rotundity known in the time of Peate; besides which, such investigations essentially belong to physical geography. But, excepting these omissions, the work is one of sterling value; it is profusely illustrated, each of the two volumes containing twelve coloured plates and about 270 woodcuts, and the explanatory text is very readable and interesting throughout. Such a production will naturally gravitate to the free public libraries and similar institutions, and will be of great use in extending scientific knowledge.

Steam. By William Ripper, Professor of Mechanical Engineering in the Sheffield Technical School. (London: Longmans, Green, and Co., 1889.)

THIS volume consists of an elaboration of notes of lectures given by the author to an evening class of young mechanical engineers. For its size, it contains much useful information; and the simplicity of expression, and the absence of elaborate calculation, throughout the chapters help to make it suitable for elementary classes. The author gives special prominence to the principles involved in the economical use of steam. This part of the book is particularly lucid and concise, being perfectly clear to the average student. He also describes well the compound, triple, and quadruple expansion engines, especially dealing with the general idea of the expansion and course of the steam through the cylinders on its way to the condenser, as well as with the general laws regulating the volumes of the cylinders. Although the subject is treated in an elementary manner, there is much sound work in the book. Text-books on steam have greatly improved of late years from an engineer's point of view, and the present volume is a good example of the way in which the subject should be handled for the benefit of budding engineers.

The illustrations and diagrams are good, the former being taken from engines in actual practice. Fig. 134, however, does not represent particularly good practice. The flat crown of the fire-box of locomotive type of marine boilers is probably seldom stayed after the manner shown; the crown stays being generally screwed through the shell of the boiler, and either rivetted over or fastened with a nut and a copper washer. Assuming that these stays are screwed through the fire-box crown sheet, it would be interesting to know how the author proposes to place them in position, as shown in the figure. Fig. 137 represents a Ramsbottom locomotive safety valve. Although correct in principle, it is quite a curiosity in point of design, the valve in general use being very different in appearance, as the reader may observe by referring to the one shown on the locomotive boiler illustrated in Fig. 132. We may say in conclusion that a fuller index would have added considerably to the value of the book. N. J. L.

Australia Twice Traversed. By Ernest Giles. In Two Vols. (London: Sampson Low and Co., 1889.)

THE narrative presented in these volumes has been compiled by Mr. Giles from the journals written by him during five exploring expeditions into and through central South Australia and Western Australia from 1872 to 1876. The materials of the book are not, therefore, very fresh, but this ought not to detract much from their interest, as hitherto only fragmentary accounts of Mr. Giles's travels have been printed. It must be admitted that records of wanderings in the interior of Australia are not usually very fascinating. Mr. Lumholtz's book, which we lately reviewed, is a brilliant exception to the general rule. We cannot say that Mr. Giles's work rises to an equal height above the ordinary level; for it lacks that fine insight into

native life and temperament which is the special and most valuable characteristic of the Danish explorer's record. Moreover, Mr. Giles had to pass through much desert country, the description of which could have been invested with charm only by a writer of genius. The book, however, shows that he has the courage, resource, and spirit of enterprise which are absolutely essential to an explorer, and here and there his story is lighted up by what he has to say about the few well-watered and pleasant tracts of land through which he passed during his various journeys. His explorations were necessary links in the chain of Australian geographical research, and he has acted wisely in preparing a full and accurate account of them. The value of the work is considerably increased by maps and illustrations.

New Zealand for the Emigrant, Invalid, and Tourist. By John Murray Moore, M.D. (London: Sampson Low and Co., 1890.)

DR. MOORE spent nine years in New Zealand, and not only enjoyed his stay, but derived from it renewed health and vigour. When, therefore, he began to set down the results of his observation and experience, he was in the right mood for the production of a genial and appreciative record; and his book ought to be of considerable service to each of the three classes mentioned on the title-page. The most original parts of the work are two chapters, in one of which he indicates the various climatic zones into which New Zealand as a health-resort is divisible, while in the other he presents a full account of the characters and therapeutic achievements of the principal thermal springs of the North Island. Both of these chapters will be read with interest by medical men, and by invalids who may feel disposed, as the author puts it in the rhetorical style he sometimes affects, to "fly on the wings of steam to the realm of the Southern Cross." He gives a good description of Auckland, "the Naples of New Zealand," and sets forth pleasantly and effectively the impressions produced upon him during excursions to the hot lakes and terraces, and to the west coast Sounds. An instructive chapter is devoted to the volcanic eruption of Mount Tarawera; and Dr. Moore offers much valuable information about self-government in New Zealand, and the settlement of the land; and about social life, public works and institutions, productions and industries. The volume includes several maps, in one of which are shown New Zealand's climatic zones.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

A Key to the Royal Society Catalogue.

IN his anniversary address to the Royal Society, the President, referring to the great catalogue of scientific papers, used these words:—"The utility of the work would obviously be much increased if it could be furnished with some sort of key, enabling persons to find what had been written on particular subjects. I am not without hopes that this very desirable object may yet be accomplished, notwithstanding the magnitude of any such undertaking." Almost everyone engaged in scientific research must have felt the want of such a key, and will join in the President's hopes. My present object is to suggest a scheme for supplying the want at comparatively little trouble and expense.

A complete subject index, arranged in alphabetical order, would indeed be a great undertaking. The subdivisions being minute, most of the papers would have to be catalogued more than once, and, even if the references were only to the name of the author and the number of the paper in the present catalogue, the new catalogue would probably be as large as the old. The key that I suggest would be much smaller, and yet in many cases more convenient. The proposal can hardly be novel, but its advantages may not have been fully realized. Divide up the whole of science into some 5000 heads, classified in their natural order under the various branches—pure mathematics, astronomy, physics, chemistry, &c. Under each head place the names of the writers who have treated of the subject, with the dates of their earliest and latest papers thereon. If the heads are skilfully selected it will seldom be necessary to classify a paper under more than one head.

Some idea of the size of the suggested work may be gained from the following considerations. In the eight volumes of the catalogue at present published (1800–63 and 1863–73) are the names of about 57,000 authors, treating the names in the second part as entirely new. Of these, about 30,000 have only one paper each, and the remaining 27,000 average about eight papers each. In view of the tendency of all writers to devote themselves to special subjects, three heads seem a fair allowance for the papers of each of the 27,000 authors. We have thus 111,000 authors' names to be catalogued under 5000 heads, giving an average of about 22 names to each head. Such a list, printed in the style of the present catalogue, but with three columns instead of two in a page, would fill a volume of about 800 pages. Each of the present volumes contains about 1000 pages, and is sold at 20*s.*, which we are told covers the cost of the paper and printing. If the sections devoted to the various sciences—chemistry, geology, &c.—were published separately, the sale would probably be large.

With regard to the use of this list, the labour of looking up 20 or even 50 names in the main catalogue would generally be trifling compared with the unavoidable labour of reading the actual papers when the references had been found. In many cases the dates would show at once that certain authors need not be referred to. Even if we had a complete alphabetical subject index, it would be necessary to think of every possible word by which the particular subject in question might be denoted, so that the classified list, though more troublesome at first, would often prove more satisfactory in the end. With 5000 heads for the whole of science, perhaps 750 might be allotted to physics, and of these, 150 to light. This would admit of such subdivisions as velocity of light, colour sensation, fluorescence, selective reflection, magnetic rotation of the plane of polarization, &c. Those subdivisions should be selected, into which the actual papers most naturally fall, rather than those which seem ideally correct.

The labour of preparing such a list as I propose would be in itself considerable, but, compared with the colossal enterprise which the Royal Society has already carried out, it would be small, and the service to science would be great.

Hotel Buol, Davos. JAMES C. MCCONNEL.

Osteolepidæ.

THE letter of your correspondent "R. L. + E." somewhat misses the issue raised in the passage to which he refers. In that passage the question was not raised whether or no we are right in making family names from the inflected form of the generic ones, the sole contention being for uniformity in this respect. Thus, if we are right in making *Rhizodontidæ* (and not *Rhizodidæ*) from *Rhizodus*, we clearly ought to have *Osteolepididæ* (and not *Osteolepidæ*) from *Osteolepis*, both these generic names being precisely analogous compounds. If, on the other hand, your correspondent is right in saying that we should regard all such names as adjectival, then we ought at once to abolish family names like *Macropodidæ*, *Dasypodidæ*, *Octodontidæ*, &c., in favour of *Macropidæ*, *Dasypidæ*, and *Octodidæ*. R. L.

THERE can be no question that "R. L. + E." is himself mistaken in his arbitrary assumption of a rule for the formation of compound adjectives in Greek. Sometimes the lengthened genitive is used as the stem, as in *δισώματος* ("disomatus"); sometimes the short nominative stem is employed, as in *δίστομος* ("distomus"); and sometimes both forms occur side by side, as *φιλαίματος* ("philæmatus") and *φίλαιμος* ("philæmus"), the former seeming to be preferred. These are words actually in use in Greek writers, and any lexicon will give plenty of other instances. But his whole argument is beside the point; the question is not whether an adjective is formed from the lengthened genitive, but whether an adjective, formed from a noun

which lengthens its genitive, lengthens its own genitive. It does so in every instance; *e.g.* we have *καλλίθριξ* with genitive *καλλίτριχος*, *μικροπτέρυξ* with genitive *μικροπτέρυγος*. Hence, in the Lepidoptera, we rightly call the family, of which *Micropteryx* is the type, the *Micropterygidæ*.

Osteolepis, though not occurring in Greek writers, is not "of questionable form," but as good a word as *φιλόπολις* and *φιλόπατρις*; and just as the latter actually forms a genitive *φιλοπάτριδος*, so also *ὀστεόλεπις* would form *ὀστεολέπιδος*, and the family name would be *Osteolepididæ*. Finally, it is to be remembered that the family name is not formed from a "possible" generic name, but from an existing one; so that *Ostiolepus* is out of the question, and indeed is only "possible" because there happens to be a word *λέπος* from which it can be derived.

I must apologize for troubling you at this length, but my fellow-workers in science are not unfrequently so hazy on the subject of classical nomenclature that there is a need for the setting forth of sound doctrine. E. MEYRICK.

The College, Marlborough, January 25.

As to the facts of word-formation in Greek, Mr. Meyrick is, as was indeed to be expected, quite right, and might have put the case even more strongly. The short forms, like *πολύστομος*, are much rarer than those in which the full stem is found, like *πολυσώματος*. They are, indeed, unless I mistake, found only with the neuter stems in *-ατ-*, as in *δερμα(τ-)*, *στομα(τ-)*, *σωμα(τ-)*, *ἁιμα(τ-)*, *σπερμα(τ-)*, and appear to be a speciality of that class of nouns, where they occur beside, but not to the exclusion of, the full normal forms. There is no ground for thinking that a derivative form in *-lepos* could be formed from the noun *λεπις*, *λεπιδ-*, or a derivative in *-ornos* from *ὀρνις*, *ορνῖθ-*. **Osteolepus* and its alleged pl. **Osteolepi*, may certainly be pronounced impossible on Greek analogies; and could not even be grounded on the by-form of the noun, *λέπος*, stem *λεπε(σ)-*, since the adjective from that *-os*, *-ες* stem, would necessarily end in *-λεπης*, *-λεπες*. As, therefore, *Osteolepid-* is the stem of the noun, the name of the family, on Greek analogies, is necessarily *Osteolepid-idæ*.

But I do not myself think that it is *always* necessary to conform to Greek analogies; I think that the convenience of English needs is also to be considered. In *Osteo'lepis, Osteo'lepi'didæ*, I think English needs are fairly answered; but it is not always so; some formations of the kind are hardly pronounceable, or when pronounced, through shifting of accent, presence of mute letters, pronunciation of *c*, *sc*, as *s*, and the like, do not in the least suggest their meaning.

Indeed, I think it very desirable that the Linnean and other learned Societies should establish a Committee of Nomenclature, who should consider every new name proposed, and pass or reject it, after taking into consideration not merely etymological correctness of formation, but what I think far more important, capability of being pronounced, distinctness from other existing names, and fitness for yielding derivatives, if needed. I entirely disagree with the notion that every discoverer of a genus has a right to confer a *name* upon it which he himself has never considered how to pronounce. I have had occasion repeatedly to ask inventors of such names, how they pronounced them, and have more than once been told that they had never thought of that, only of getting the Greek form right, and that I, forsooth, must settle the pronunciation! Such men were, of course, utterly unfit to confer names, however eminent as scientists. Every name that does not lend itself to a distinct and easy pronunciation, or which, when pronounced, is undistinguishable from some other word spelt quite differently (*e.g.* words in *cœno-*, *cæno-*, *sceno-*, *seno-*, &c.), ought to be rejected. Better invent new words off at the ground, having *no etymology*, than put together Greek roots in combinations unsuitable for modern mouths and modern ears. Why must modern knowledge be confined within the swaddling-bands of a nomenclature 2000 years younger? J. A. H. MURRAY.

Oxford, January 28.

Compounds of Selenium.

IN your issue of the 23rd ult. (p. 284) you insert a paragraph describing experiments by M. Chabrié on compounds of selenium. While fully acknowledging the value of his work on the phenyl derivatives of selenium, I think it right to state that much of M. Chabrié's investigation has been anticipated by Mr. F. P. Evans and myself as long ago as 1884; and that several of his assertions are incomplete and incorrect. The tetrachloride, $SeCl_4$, as we then showed, exists in vapour as such between 180° and 200°; with rise of temperature it dissociates, but even at 360°, dissociation is incomplete. In our paper (*Trans. Chem. Soc.*, 45, 62) the progress of the dissociation is followed.

We do not agree with M. Chabrié's suggestion that the products of dissociation are the other chloride, Se_2Cl_2, and chlorine, for the very good reason that Se_2Cl_2 itself is an extremely unstable body. Instead of, as he asserts, having a constant boiling-point at 360°, it begins to boil at 145°; and temperature rises to 173°, while a mixture of Se_2Cl_2 and $SeCl_4$ distils over, leaving a residue of selenium. The vapour-density of Se_2Cl_2 was found by us apparently normal; but this is caused in reality by the fact that it also dissociates completely on vaporization into selenium and chlorine without change of volume, according to the equation $Se_2Cl_2 = Se_2 + Cl_2$.

A revision of the experimental work of previous investigators is obviously to be desired; but it should be undertaken as a revision, else inaccurate conclusions may be drawn from incomplete work, as they have been in this case.

Perhaps I may be allowed to take this opportunity of inquiring by what reaction selenophenol, C_6H_5SeH, is produced from the red oil, $Se_2(C_6H_5)_3C_6H_4Cl$, out of which it is said to deposit on standing? WILLIAM RAMSAY.

University College, Gower Street, February 3.

Royal Victoria Hall and Morley Memorial College.

I HAVE only just read the article on Polytechnics for London in your number for January 16 (p. 242). I hope it is not too late to offer a few words of comment on it. Nothing is said of that part of the Commissioners' scheme which applies to the Royal Victoria Hall and Morley Memorial College, probably because the amount intended for them is comparatively small—£6000 down for structural alterations, and £1000 a year to be divided between Hall and College. But it derives an importance beyond what is due to the amount of the grant, from the fact that it is no castle in the air, but a going concern, and had begun its useful life long before the Commissioners had planned their scheme. Moreover, many of your strictures do not apply to this particular part of it. You say there will be, under the new scheme, "no People's Palaces—only *Young* People's Institutes." You object to limitation of age, and to smoking being forbidden, and you conclude by urging most truly that "life should come first, then buildings," for life develops from within.

May I therefore, in as few words as possible, give an account of the history and present position of the Hall and College, with the object of showing that the truths you urge have been already laid to heart?

The Hall (formerly the "Old Vic." Theatre) was opened 9 years ago as a temperance music hall, to compete with the degrading attractions of ordinary music halls, about which there was less stir in those days than now. At first we had variety entertainments every night, but before long the experiment was tried of introducing something better on certain nights. There is no need to enter into the ups and downs through which experience was gained; suffice it to say that we still have "variety" pure and simple on Saturdays, when our gallery boys, as well as well as their elders, enjoy themselves to their hearts' content, to the number of 1800 or so; and a modification of this kind of entertainment takes place before a much smaller audience on Mondays and Wednesdays. But on Tuesdays (as your readers know from the occasional notes which appear in your paper) we have popular illustrated lectures from many of our leading scientific men, who continually express their gratification at the appreciative attention of the audience. On Thursdays we have ballad and operatic concerts, at which (interspersed among operatic selections) tableaux, representing scenes from operas, are given. And on Fridays there are temperance entertainments.

All this will be left unchanged by the new scheme; and is not this something very like a "Palace of Delight"? Smoking is and will be freely carried on (except in certain parts of the house on concert nights), and anyone, without distinction of age, can come in by payments ranging from twopence on Thursdays and Saturdays, and from a penny other nights.

But this is not all. A little more than four years ago, classes were started in the unused dressing-rooms at the back of the stage, in response to a demand for more systematic instruction from some of those who had attended the lectures. The first

class began with four students, but soon the number was as great as the rooms could conveniently accommodate, and excellent work was done in spite of many inconveniences, one of the greatest of which was the impossibility of excluding the sounds of the entertainments in the Hall. From time to time *soirées* were held, and the students informally consulted as to what additional classes they wished for. Where a demand existed, every effort was made to obtain the supply.

Then came the offer of the Commissioners to meet a subscription with an equivalent endowment, and the freehold was bought, in memory of one of the truest friends of the work, Mr. Samuel Morley. Finally, the waste space which had been occupied by dressing-rooms and stores of old scenery was cleared of its dangerous wooden staircases, a sound-proof, fire-proof wall was built to divide it from the theatre, and large convenient class-rooms were built; and on the last day of September the Morley Memorial College was opened, for working men and women; Miss Goold (the well-known head of the Queen Square College) having consented to take the office of Principal here also.

Already there are 680 students on the books. Many criticisms may be made on the arrangements, but no one can say that there is a want of life in the place. The builder's men are hardly yet out of it, and the fittings are at present of the scantiest (the result of want of funds, for the delay in passing the Commissioners' scheme through Parliament has caused unlooked-for and very embarrassing delay in the receipt of the help expected from that quarter) but the enclosed prospectus will show ample signs of life. Admission to the gymnasium, smoking, and recreation rooms can only be gained by *bonâ fide* attendance on at least one class, a rule which the Committee consider very important, and which they adopted in consequence of their experience with a club which met at one time in some of the old rooms belonging to the Hall. No new students are admitted under 17, for the simple reasons that it does not answer to mix boys and men, and that the boys are provided for by the Recreative Evening Schools Association; but there is no limit of age at the other end. When the Borough Road Polytechnic is started, the College will probably take those students who want advanced literary and scientific teaching, excluding "technological classes," for which neither space nor funds would suffice. In fact, the College will be in all probability the advanced branch of the Polytechnic. At all events, it is intended that the two institutions should play into each other's hands and avoid overlapping.

You say most truly that life develops from within. I would go further, and say that "*omne vivum ex vivo*" is as true of moral and social as it is of organic life. No institution can grow and flourish unless life has been given in its service, and this is emphatically the case with that of which we are speaking. To mention names would not interest outsiders, and to those who have watched the Hall from its very beginning, nine years ago, it is well known whose heart work as well as head work has been devoted to it and kept it alive through its troubled infancy. This it is which has drawn other workers to help in doing what one alone could never accomplish, and given spirit to the whole. They have allowed life to develop from within, watching for what was practicable instead of airing preconceived theories, and this is why so little has had to be done twice over. Help of all kinds is greatly needed, for the concern is only in its early childhood yet, but one thing is certain—whatever wants have to be supplied and defects remedied, this is *not* an "architectural white elephant." Probably that could never be true of any institution which had so much heart as well as head devoted to it, but let those who doubt come and see for themselves!

February 5. A MEMBER OF COMMITTEE.

Galls.

IN NATURE of November 28, 1889 (p. 80), Prof. G. J. Romanes speaks of galls as "unequivocal evidence of a structure occurring in one species for the exclusive benefit of another," and states that "it is obvious that natural selection cannot operate upon the plants directly." Nevertheless, there is one way in which galls may be supposed to have been evolved as beneficial—or rather, less harmful—to the plants. Every farmer is aware of the great loss to vegetation caused annually by larvæ of insects boring within the branches and twigs of trees. Now suppose that all internal plant feeders were originally borers or leaf-miners—and this is highly probable,—but that some had a tendency to cause swellings in which they fed. These latter would be less injurious to the plants, and the greater the vitality of the plants the more nourishment for them; and so by degrees the globular and other highly specialized and least harmful galls would be developed, by natural selection, for the benefit not only of the insect, but also of the plant. And known galls, which I need not here enumerate, furnish us with all the steps of this evolution. T. D. A. COCKERELL.

West Cliff, Colorado, U.S.A., January 23.

Foreign Substances attached to Crabs.

THE Compound Ascidian referred to by Dr. R. v. Lendenfeld in yesterday's NATURE (p. 317) is one of the Polyclinidæ, and probably a new species. It belongs to the genus *Atopogaster*, and is closely related to *A. informis* (*Challenger* Report, Part ii. p. 171).

I have before me now five good specimens of the crab and Ascidian (the crab in this case is *Dromia excavata*, Haswell), dredged in Port Jackson, and sent by the Australian Museum, Sydney; they measure as follows:—

Specimen.	Crab (greatest diameter). cm.	Ascidian (length, breadth, and height). cm. cm. cm.
A	4	10 × 8 × 5
B	3·5	10 × 6 × 5
C	2·5	8 × 6 × 5·5
D	2·5	6 × 6 × 5
E	2·5	5·5 × 4·5 × 3

In the largest of them the Ascidian seems to be quite twenty times the size of the crab.

I notice in these specimens that the last pair of thoracic legs in the crab, which are much larger than the preceding pair, are turned up dorsally, and are so firmly embedded and attached by their sharp claws in the test of the Ascidian that it is easier to disarticulate them than to loosen their hold.

To those who dredge much round our coasts, a crab covered with foreign substances is no unusual sight. Specimens of *Hyas* are often found so overgrown with Algæ, Sponges, Zoophytes, and Polyzoa that almost the whole of the body and legs is hidden, and the animal is scarcely recognizable except by its movements. W. A. HERDMAN.

Liverpool, February 7.

The Ten and Tenth Notation.

IT is no doubt difficult for anyone to really conceive enormously great or infinitely small quantities. This difficulty is, however, much minimized by the ten and tenth notation. Indeed, if systematically used, I believe one's mental power of estimation would be practically perfect. But is it so used? I have before me three books—I only take this as an example of what frequently occurs—in which Joule's equivalent is given is—

$$\left.\begin{matrix} 42 \times 10^6 \\ 4{\cdot}2 \times 10^7 \\ 0{\cdot}42 \times 10^8 \end{matrix}\right\} \text{respectively.}$$

B. A. MUIRHEAD.

Pall Mall Club, Waterloo Place, S.W., February 8.

P.S.—The natural uniform notation, at any rate for text-books, seems obvious.

EARTH TREMORS FROM TRAINS.

AMONG the writings of those who love to speculate on the future of our planet there is probably somewhere (though we have not had time to discover it) an essay on the cosmical changes which man will be able to produce in the earth. The data for solving this problem are striking. In a few centuries man has acquired all those powers over large and solid objects represented by his knowledge of explosives, and his use of steam. Multiply the centuries, and with them the history, by convenient figures (a familiar process in this kind of problem) and there is no reason why the earth's axis of rotation should not be shifted considerably by human agency.

For the present, however, we are concerned with a more

modest inquiry—to wit, how far the railways which jar the nerves of Mr. Ruskin so terribly, are desirable neighbours for anyone who prefers the earth under his feet to be firm and steady, as it was aforetime, and as it is now sometimes in remote parts of the country on Sundays. We have all noticed, when standing near a passing train, the vibration of the ground under our feet. Though this vibration decreases as we recede from the train, and may at a distance of 50 or 100 yards become insensible to such a coarse test as the actual jarring of our body, we can understand that it may be sufficient to disturb delicate instruments at a considerable distance; and thus affect the use of instruments requiring a steady foundation. Pre-eminent among such are astronomical instruments, and it was very early in the history of railways that astronomers found themselves compelled to fight for the retention of that steadiness of ground in their neighbourhood which is of vital importance to them, and with which no human agency had previously suggested an interference. It was in 1835 that the question of taking a railway near an Observatory was first raised, in connection with the Royal Observatory, Greenwich; and an animated discussion resulted in the defeat of the railway company.

But they have several times since returned to the charge, for Greenwich has always been an attractive centre for excursions, and there are many reasons why railway companies find it continually cropping up in their schemes; indeed, it is only a few months ago that the latest application of the kind was refused by Parliament.

On June 19, 1835, the Secretary of the Admiralty wrote to the Astronomer-Royal, Mr. Pond, asking for his comments on the proposed scheme for a Greenwich-Gravesend railway, passing in a tunnel under a part of Greenwich Park, in which the Royal Observatory is situated. Mr. Pond replied that he had no experience in such matters; but "the most important observations made at the Royal Observatory are those in which the stars are seen by reflection from a horizontal surface of mercury. It appears to me highly probable, by what I have experienced from slighter causes, that the passage of heavy carriages, even at the distance of the intended tunnel, might produce sufficient tremor on this surface to destroy the accuracy of these observations." On receiving this reply, Captain Beaufort, then Hydrographer to the Admiralty, wrote to a friend, Commander Denham, asking him to make experiments near one of the few existing lines of railroad—that between Liverpool and Manchester—with a sextant and artificial horizon. After explaining the object of the experiments, he says:—"It would be childish to be guided by opinions and suggestions, when the facts can be distinctly ascertained by means of the Liverpool and Manchester Railroad, and I therefore want you to take your artificial mercury horizon to that railroad, and watch the contact of a star or the sun in altitude with a telescope when the train is passing, at two or three different distances, till you come to the outer limit of vibration, or, in other words, to the distance at which the mercury is no longer affected. After you have tried this on the surface, I wish you would then try the same experiment in the neighbourhood of the tunnel, as I presume that the results will be very different."

Commander Denham's reply is as follows:—"I find the vibration of trains of 120 tons, at a speed of 25 miles an hour, affect the mercury as far as 942 feet laterally with the rails, on the same level, and on equal substratum; but vibration perfectly ceases at 1110 feet, whilst directly over the tunnel no vibration is detectable at 95 feet distance, though quite discernible at 65 feet vertical distance. . . . I am indebted to the co-operative accommodation of the directors, who allowed trains of extra weight, and at extra speed, to pass down at night hours when the busy hum (of carting carriages and bustle) was completely suspended."

In the printed report of this correspondence the Hydrographer notes on this letter: "It is proper to remark on the above that Commander Denham's experiments depended on observations with a sextant, and that the limits of tremors in the mercury would be far more extensive if viewed by the high magnifying powers used with the mural circle."

We have quoted this case in detail not only because it was the first experiment of the kind, but because the accuracy of the results, as interpreted by the Hydrographer's note, has been confirmed by later experiments. This report was adverse to the railway company, who wished to approach within 650 feet of the Observatory; but they did not relinquish their scheme at once. They suggested various plans—of running trains at slow speeds, or stopping them altogether if the Royal Observatory signalled that an important observation was just going on, and so forth—all of which were open to the objection of looking too well on paper. Meanwhile Mr. Pond had been succeeded by Mr. (afterwards Sir George) Airy, who, in 1836 January, repeated Commander Denham's experiments in the Glebe Meadow, near the Greenwich Railway, but using a small telescope instead of a sextant. He found that "a disturbance in the clearness of the image (in mercury) was perceptible when the train was 1106 feet from the mercury, and the image was almost lost from the violence of the agitation when the train was about 700 feet from the mercury. When the train was 500 feet from the mercury it was impossible to know whether there ought to be any object visible at all."

The question was ultimately resolved into a decision upon the minimum distance from the Observatory at which a railway could be allowed; and under strong pressure, Sir George Airy was induced to define this distance as something over 700 feet; but the position to which the line was thus removed was found to bring it near other buildings, and the project was ultimately shelved. The Astronomer Royal's troubles were, however, only just commencing. In 1840 the London and Chatham Railway Company asked for leave to go through the Park; being promptly followed by a similar application from the South-Eastern Company; and he must needs repeat his experiments and protests.

His experiments in March 1846 near the Kensal Green tunnel showed that tremor was sensible in the compact clay of Kensal Green to a distance of 1700 feet, but that the tremor was very much diminished where the railway enters a tunnel. Dr. Robinson, of Armagh, made independent experiments on the Dublin and Kingstown Railway. He mounted a mural circle on an ash post driven deeply into the ground, at a distance of 1655 feet from the nearest point of the line; and found that the vibration of passing trains gradually shook the instrument away from any position in which it was clamped, so that an object would not remain bisected by the cross wires. His reflection observations were numerous, and he sums them up as follows: "On these facts it is, I presume, unnecessary to offer any comment, except the simple remark that they show clearly that, in a soil such as I have described, a train of no uncommon weight or velocity can produce, at an oblique distance of two miles, such disturbance as ought never to be tolerated in an Observatory."

Sir James South also made experiments, and concludes his report to the Admiralty thus:—"To the observations of *right ascension made by reflection*, the more immediate object of this communication, let me then entreat your Lordships' serious attention, convinced, as I am, that, did they stand *alone*, they would justify your Lordships in saying to *present* as well as to *future* railroad applicants, 'WITHIN THIS PARK STANDS THE ROYAL OBSERVATORY OF ENGLAND, AND WITHIN THIS PARK'S WALLS A RAILROAD SHALL NEVER COME'" (The italics and capitals are as in the original.)

These strong protests had the desired effect for the time being, and it was not till 1853 that another attempt was made to bring a railway within the Park. This was by the South-Eastern Company, and being postponed for a year, was not heard of again. In 1863, however, the London, Chatham, and Dover Company proposed a line from Dulwich to Epsom passing within 700 feet of the Observatory; and the South London, Greenwich, and Woolwich Railway another passing within 600 feet. Sir George Airy was at first inclined to think that, if these railways were laid in tunnels, they might be permitted. But as facilities for making experiments had meantime increased with the multiplicity of lines, he renewed his investigations at the suggestion of the Hydrographer, and found that the protection of the tunnel was by no means established; and in other respects he had been if anything too lenient in assigning minimum distances. His conclusions from the experiments were:—

"I. It is indispensable that the railway pass through the Park in a covered tunnel.

"II. It is indispensable that its minimum distance from the transit circle of the Royal Observatory exceed 1000 feet."

The result of all these independent experiments seem to be that even with small instruments, such as a sextant or a small telescope, vibration is sensible at 1000 feet distance; and that though a tunnel may be a protection in some cases (we shall presently find reason to question this more seriously) the reasons are not sufficiently understood to enable us to predict the influence of individual tunnels. All the observations, except one of Dr. Robinson's, have reference to reflection observations; but it does not follow that these are the only observations disturbed, as is made abundantly clear by the single observation of Dr. Robinson's referred to, where the telescope was practically shaken to another position against the clamp. It is in reflection observations that the vibration is most easily discernible, but errors introduced into other observations are no less serious because they are not readily detected. Observation with mercury is a delicate test; but it is quite possible that we may very soon find even a more delicate test necessary. We are, for instance, only on the threshold of photographic experiments for which the most perfect steadiness is essential; and it is of the utmost importance to make sure that our large Observatories are so protected as to be available for such work as is gathering shape in the mists of the near future. If any mistake has been made in dealing with railway proposals, it has been that of being too lenient; firstly, from the desire to yield as far as possible in matters affecting public convenience; and, secondly, perhaps from not fully appreciating the remark of Captain Beaufort in 1835, that the results obtained with small instruments must be properly magnified for dealing with large ones. This point has been made clear by the last case we shall quote, also from the history of the Royal Observatory. Proposals for an adjacent railway were renewed, as we have said above, in 1888. It had been already noticed that the lines which had been permitted were not sufficiently remote to prevent disturbance, and accordingly experiments were now made with the transit circle itself instead of with a small instrument. An observer was stationed at the transit circle prepared for a nadir observation, and for an hour noted the times when the images were steady, when partially disturbed, and when so agitated as to prevent observation. These times were noted carefully by a standard clock to within a few seconds. Other observers were furnished with watches set to standard time, and travelling on the various lines of railway in the neighbourhood noted the exact times of stopping and starting of all trains, entries into tunnels, &c. The observations were made near midnight when other traffic was stopped. On the following day the independent records of the transit circle observer and the train observers were compared. These operations were repeated on five separate nights. The result of the series of observations may be gathered from the following extract from the Report of the Astronomer-Royal to the Board of Visitors, 1888 June 2:—

"It resulted from these experiments that trains on the Greenwich-Maze Hill Railway caused great disturbance during their passage, not only on the section between Greenwich and Maze Hill, the nearest point of which is 570 yards from the transit circle, but also on the line beyond Greenwich on the London side, and beyond Maze Hill on the Woolwich side. The distances of the Greenwich and Maze Hill Stations from the Observatory are about 970 and 670 yards respectively . . . The disturbance was very great during the passage between Greenwich and Maze Hill, the reflected image being invisible while the train was in the tunnel, at a minimum distance of 570 yards, and there was considerable disturbance during the passage of trains through the Blackheath-Charlton tunnel, at a distance of a mile, the reflected image becoming occasionally invisible."

It thus appears that the tunnels increased rather than diminished the disturbance; and that the minimum distance for insensible tremor had been considerably underestimated. But the interference with the work of the Observatory is not serious. By the vigorous action of Sir George Airy and his successor the national Observatory has been saved from the misfortunes which have befallen Paris and Berlin, where traffic has been allowed to make certain classes of observation impossible.

H. H. TURNER.

TITANOTHERIUM IN THE BRITISH MUSEUM.

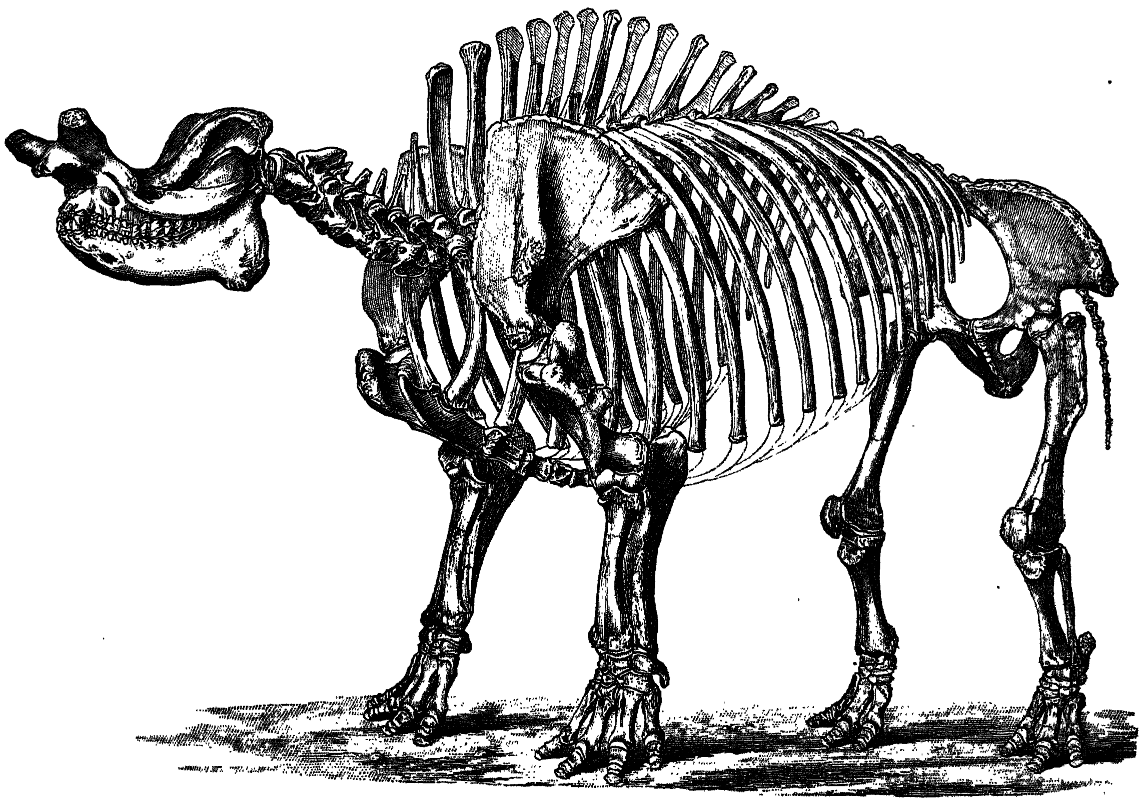
To those English zoologists who have not had the good fortune to visit the palæontological museums of the United States the huge Miocene mammals forming the family *Titanotheriidæ* have been hitherto known only by description and small-sized figures of the skull and skeleton, which, however excellent they may be, afford but a very inadequate idea of the proportions of these most remarkable Perissodactyle Ungulates. Recently, however, Prof. O. C. Marsh, of New Haven, to whose generosity our National Museum is already much indebted, has presented that institution with a beautifully executed model of the skull of one of these mighty brutes, which is now exhibited in the front palæontological gallery, below the head of the skeleton of the Kentucky mastodon. By singular good fortune the Keeper of the Geological Department of the Museum has been enabled at the same time to purchase associated examples of the teeth of another member of the family, which are placed alongside of the cast, and thus enable us to see the actual state of preservation in which the remains of these creatures are found.

The *Titanotheriidæ* were first made known to science from the evidence of specimens of the dentition described years ago by the French naturalist Pomel, by whom the name *Menodus* was proposed for their owner. Unluckily, however, this name was preoccupied by the earlier *Menodon;* and we are therefore compelled to adopt for the type member of the family the name *Titanotherium,* which is the first of the numerous terms proposed by American writers. The species of which the skull has been presented to the Museum is made by Prof. Marsh the type of a distinct genus under the name of *Brontops.* The chief distinction of this form from the type of *Brontotherium,* which seems inseparable from *Titanotherium,* appears to be the reduced number of incisors, but if writers like the Director of the Museum are right in regarding such variations in the allied group of the

Rhinoceroses as of not more than specific importance, this species should be included in the type genus.

These Titanotherioids appear to have been most nearly allied to the Rhinoceroses among existing forms, as is at once apparent from the contour of the skull. According to Prof. Marsh they were larger than the Dinocerata of the Eocene, and nearly equalled in size the existing elephants. The skull differs from those of the rhinoceroses, however, in that instead of having one or two horns placed in the middle line of the nasal region and having no sort of bony connection with the skull itself, it has two large processes of solid bone in a transverse line immediately over the nose, which were probably invested with a horny sheath.

The molar teeth are, moreover, unlike those of the rhinoceroses, having excessively low crowns, and an arrangement of the tubercles and ridges very similar to that obtaining in the Tertiary genera *Limnohyus* and *Chalicotherium*; the first of which is certainly, and the latter probably, a Perissodactyle, although the recent discovery that the peculiar claws upon the evidence of which the supposed Edentate genus *Macrotherium* was founded are referable to it, render it a most aberrant type.



Restoration of the skeleton of *Titanotherium robustum* ($\frac{1}{21}$ nat. size). After Marsh.

The skeleton to which the original of the cast presented to the Museum pertains was found in 1874 by the donor in those beds of the Dakota Miocene known as the Brontotherium beds, and it appears to be the best preserved example yet known. A restoration is given in the accompanying woodcut. According to Prof. Marsh these deposits are several hundred feet in thickness, and may be separated into horizons, characterized by peculiar species of *Titanotheriidæ*. The remains of several hundred individuals of this exclusively American group have already been secured by the palæontologists of New Haven, and their English *confrères* look forward to the publication of the sumptuous monograph in which Prof. Marsh promises to illustrate these specimens with much interest.

NOTES.

There is some talk of a Committee of the Royal Society being appointed to investigate the subject of colour-blindness, and the proper methods of testing the colour-vision of *employés* on railways.

We may remind our readers that all applications for assignments from the Government Grant must be sent to the Assistant Secretary of the Royal Society on or before the last day of February. Applications received after that date will not be considered by the Committee of this year.

An influential Committee has been formed for the purpose of securing that the scientific and other friends of the late Dr. McNab, Professor of Botany in the Royal College of Science, Dublin, shall have an opportunity of expressing their appreciation of his work and their respect for his memory. Through no fault of his own, Prof. McNab was unable to make adequate provision for his wife and five children; and it is proposed that the memorial shall consist of a fund, sufficiently large to be of real service to his family. A good many subscriptions have already been received or promised, and we hope that many more may be forthcoming. Mr. Greenwood Pim, Easton Lodge, Monkstown, Co. Dublin, acts as hon. secretary; Prof. W. N. Hartley, F.R.S., Royal College of Science, Dublin, as hon. treasurer. As Prof. Hartley has been obliged to leave Dublin for some time, all communications should be addressed, and cheques made payable, to the hon. secretary.

We have already (p. 207) called attention to the fact that a committee has been formed in Paris for the purpose of making arrangements for the erection of a statue of the late M. Boussingault. His work marked an era in the history of the agricultural sciences, and we have no doubt there will be a prompt and liberal response to the committee's appeal for subscriptions. M. Pasteur is the honorary president of the committee. The acting president is M. Schlœsing, and the following are the vice-presidents: MM. Berthelot, Duchartre, Laussedat, Peligot, Risler, and Tisserand. MM. Müntz and Sagnier are the secretaries, and M. Liébaut is treasurer.

The death of M. Sébastien Vidal, Director of the Botanic Garden at Manilla, is announced. He was well known for his researches on the flora of the Philippine Islands.

The scheme of the Senate of the University of London, drawn up in accordance with the recommendations of the recent Royal Commission, does not at all commend itself to the authorities of the provincial Colleges. They are convinced that it would be most injurious to the interests of places of education outside the capital. This view was strongly expressed last autumn at a meeting of representatives of the provincial Colleges at Birmingham, and yesterday (Wednesday) it was pressed upon the attention of Lord Cranbrook by a deputation which waited upon him at the Privy Council Office.

To-morrow afternoon (Friday), at the Royal United Service Institution, Mr. H. Dent Gardner will read a paper on "The Ship's Chronometer—its History and Development." The paper will be divided into four parts: (1) historical, (2) historical-descriptive (the building up of the chronometer), (3) the chronometer of to-day, and (4) methods of testing and rating chronometers.

The Ben Nevis Observatory Monthly Report for January is of more than usual interest. The rainfall during the month amounted to 29·42 inches, being 15·10 inches above the mean of the month since the Observatory was opened in 1883. A measurable quantity fell every day, and on 11 days over an inch was recorded each day, while on the 14th, 3·88 inches fell. The total bright sunshine amounted to only 4 hours, being the smallest number hitherto recorded. Lightning occurred on 5 days. The storm of the 5th was peculiarly severe, on which occasion the telegraph cable was damaged and communication stopped. St. Elmo's Fire was seen on the 21st and 25th, under the same relations to the cyclones then passing over North-Western Europe as described recently in Nature.

We have received from Mr. C. L. Wragge, Government Meteorologist of Queensland, his first Annual Report of the Meteorological Branch of the Post and Telegraph Department for the year 1887. It is divided into three sections. Section 1 gives an account of the organization, inspections, &c., containing a list of the recommendations originally made by Mr. Wragge, and a general statement as to how far each of them has been carried out. This synopsis shows that, while he has accomplished much during the year 1887, more still remains to be done. Section 2 contains abstracts of reports for each month from the rainfall stations, with climatological and other tables from the stations which are supplied with instruments. These abstracts contain very interesting data upon the state of the country, and will become more valuable in proportion as the number of verified instruments to be supplied year by year increases. As Mr. Wragge himself points out, any conclusions from so short a series of observations would be premature. Section 3 contains a graphic record of the chief meteorological elements for Brisbane, with seasonal wind charts and cloud charts for Queensland, and specimen wind charts for Australasia. These form the most interesting portion of the Report, and give promise of valuable materials for scientific study. In Western Australia, however, the weather charts show that there are vast tracts of country with apparently no meteorological stations.

The last issue of the *Memoirs* of the Tashkent Observatory (Part 3) contains a most valuable magnetical map of part of Central Asia, based on the recent measurements of MM. Sharnhorst and Schwarz.

We have already mentioned some of the conclusions as to the secular upheaval of the coasts of Finland which may be drawn from the accurate measurements made since 1858 under the direction of the Finska Vetenskaps-Societeten. We have now an elaborate paper on this subject, contributed by A. R. Bonsdorf to the *Izvestia* of the Russian Geographical Society (vol. xxv. 5). It appears from the mathematical analysis to which the measurements have been submitted that the average upheaval of the coasts of South-West Finland is 55 centimetres per century; and that the rate of upheaval increases from Ut-ö (in the Åland Islands) towards the north, and towards the east as far as Porkala (not far from Helsingfors), whence it decreases again towards the east. The interpolation formulæ better correspond to actual measurements if the changes of the level of the Baltic Sea resulting from the changes of atmospheric pressure are taken into account.

Globus reports that the Russian Geographical Society has presented a memorial to the Minister of Marine urging that scientific investigations of various kinds should be undertaken in connection with the Black Sea. Amongst other things, the Society points out that more exact soundings are needed in several parts of that sea, and that it is especially desirable they should be taken in the western part between Odessa and Constantinople.

One of the problems presented by the frightful eruption of Mount Bandai in Japan, two years ago, was the manner in which a large number of holes in the earth in the neighbourhood of the mountain were formed. It was suggested that they owed their existence to the falling of rocks and stones cast up the eruption, while another theory was that they were formed by forces beneath the surface. At the last meeting of the Seismological Society of Japan, Dr. Knott read a paper on the first theory, in which he demonstrated that it was quite insufficient to account for the phenomena. Prof. Milne, it may be added, has expressed the same view from the beginning.

Last Friday a valuable paper on "The Utility of Forests and the Study of Forestry" was read before the Indian Section of the Society of Arts by Dr. W. Schlich, Professor of Forestry at the Royal College of Engineering, Cooper's Hill. In the course of his remarks Dr. Schlich gave an account of the instruction in forestry at Cooper's Hill, and mentioned that the authorities were thinking of appointing a second professor of the subject, and thus doubling the amount of instruction now given. After the reading of the paper Major-General Michael, C.S.I., who presided, made some interesting observations. No one, he said, who had visited the great forest regions of Germany, Austria, and France could fail to be impressed with the visible effects of good management, and to wish they were more generally apparent in England and Scotland. There were signs that the education and practical training of foresters were being more thought of at the present time in England, and he ventured to predict that Dr. Schlich would shortly have a good many students under him who were destined for home employment and not for India only. Personally he knew more about the value of forestry and the life of a forester in India, having spent seven or eight of the happiest and perhaps the most useful years of

his youth as a forest officer. That was more than 40 years ago, before the time arrived for experts like Dr. Schlich and his distinguished predecessor Sir Dietrich Brandis to come to the country. He could therefore tell any of Dr. Schlich's students who might be present that the life of a forester in India was not only a career of importance, but that it was one full of interest and of real enjoyment. The formation of the department in which they would serve had justly been characterized by Sir Richard Temple as one of the greatest achievements effected in India during the Queen's reign.

THE Royal Society of New South Wales offers its medal and a prize of £25 for the best commnication (provided it be of sufficient merit) containing the results of original research or observation upon each of the following subjects:—(To be sent in not later than May 1, 1890)—The influence of the Australian climate (general and local) in the development and modification of disease; on the silver ore deposits of New South Wales; on the occurrence of precious stones in New South Wales, with a description of the deposits in which they are found. (To be sent in not later than May 1, 1891)—The meteorology of Australia, New Zealand, and Tasmania; anatomy and life history of the Echidna and Platypus; the microscopic structure of Australian rocks. (To be sent in not later than May 1, 1892)—On the iron ore deposits of South Wales; on the effect which settlement in Australia has produced upon indigenous vegetation, especially the depasturing of sheep and cattle; on the coals and coal measures of Australasia. The competition is not confined to members of the Society, nor to residents in Australia.

M. LIGNIER has been appointed Professor of Botany to the Faculty of Sciences at Caen; and Mr. G. C. Druce, author of the "Flora of Oxfordshire," succeeds Dr. Schönland as Curator of the Fielding Herbarium at Oxford.

HERR JADIN, of Montpellier, has undertaken a voyage for the investigation of the algal flora of the islands Mauritius and Réunion; and Prof. P. L. Menyhardt, who has been appointed to a mission on the Zambesi, is intending to make a collection of plants in the region between the Zámbesi and the sources of the Congo.

FOR the purpose of growing plants under more natural conditions than those usually afforded by the soil and surroundings of ordinary botanic gardens, M. G. Bonnier, the Director of the Botanic Garden in Paris, has obtained from the Director for Higher Education in Paris the grant of a piece of land in the Forest of Fontainebleau, as an annexe for experimental culture. It has been placed under the special charge of M. Cl. Duval.

AT the meeting of the Royal Botanic Society on Saturday a sweet-scented fern, from the Society's garden, was exhibited. The perfume, which closely resembles that of fresh hay, is retained after the frond is dry, and lasts for many months, if not years, imparting its fragrance to anything in contact with it. The secretary thought it might be grown as a source of perfume by amateurs, if not commercially. As yet it appeared to be little known in collections of exotic ferns. Some fine blooms of scarlet anemone, gathered from plants growing in the open air in Rutland, were shown by Mr. T. H. Burroughes.

IT is a good sign that the present building of the Bethnal Green Free Library has become quite inadequate for the needs of the institution, and that much larger premises are, if possible, to be erected. The sum of £20,000 is required, and many donations have already been received or promised. We may note that a largely attended meeting at the Bethnal Green Free Library lately started as tudents' union, for the study of various branches of science and art, in connection with the evening classes.

IN his "History of Barbados," published in 1848, Sir Richard Schomburgk says of the Barbados monkey that it was found in large numbers by the first settlers. From the appearance of a living specimen he considered it "to be *Cebus capucinus*, Geoff., the Sai or Weeper, or a very closely allied species." In the current number of the *Zoologist* Col. H. W. Feilden presents a wholly different view. He asserts that the Barbados monkey is an Old World form, the Green Monkey, *Cercopithecus callitrichus*, Is. Geoffr., and that its original habitat is West Africa. "This," he says, "undoubtedly proves its introduction to Barbados by the Guinea trading-ships." Col. Feilden cannot discover any warrant for Schomburgk's statement that this animal was found in large numbers by the first settlers on their arrival. The subject is interesting because of its bearing on the general view set forth by Col. Feilden, that Barbados has had no continental connection since the introduction of its present flora and fauna, but has received its terrestrial animals and plants from the effects of ocean currents, winds, accidental occurrences, or by the agency of man.

THE Council of the Ceylon Asiatic Society, in its last Report, urges on the Government the importance of systematically collecting, transcribing, and publishing the manuscripts of the ancient literature of the island which are scattered about in the libraries of temples, as well as in private houses. The researches which have already been made by individuals, or on behalf of the Government, show that manuscripts of great value may be found. During the last three years, private exertions have secured 69 of these; but what is needed is that the work should be undertaken as carefully and systematically as in India, where the duty of preserving the ancient literature of the country has been recognized by the Government, and where the collection of ancient manuscripts has for years past been conducted by a large staff of officers.

SUGAR seems to be losing its attractions for Lepidoptera. Mr. Joseph Anderson writes to the *Entomologist* from Chichester that his experience agrees with all that has been written on this subject lately. In the trees surrounding his house, and in those of his neighbour's garden, he has good sugaring grounds, and in former years they brought him a satisfactory return for the trouble expended on them, his captures numbering about fifty different species. "Now," he says, "for three or four years past, night after night, sugaring has been almost of no avail. Can it be a case of inherited instinct? And are the rising generation of moths getting too wise to be trapped by the sugaring baits?"

WITH the aid of an apparatus called a *periscope*, the submarine boat *Gymnote* was lately, it will be remembered, piloted safely in Toulon harbour. This enables the officer directing the movements to have a wide view around; and it consists of a vertical telescopic arrangement, with a lenticular total reflection prism at the top held between the tube and a cover above. After reflection in the prism, the rays converge at a certain point, and are received by a lens, the principal focus of which coincides with this point; thus a vertical cylindrical beam is formed, which meeting a mirror below, inclined at 45°, is directed horizontally to the eye-piece. A diaphragm, having a small radiating tongue, and moved by a tangent screw, enables one to intercept the view of the vertical plane in which the sun is, the tongue being brought to coincide with the plane. The system is said to work admirably.

EXPERIMENTING lately on the sense of smell, Dr. Zwardemaaker, of Utrecht, devised an olfactometer, which consists simply of a glass tube with upward curving part to be inserted in the nostril. A short movable cylinder made of some odoriferous substance fits over the outer straight end of the tube.

On inhaling, one perceives no odour so long as this cylinder does not project beyond the inner tube; but the further it is pushed out, the larger is the scented surface presented to the entering air, and the stronger the odour perceived. The author studies mixture of odours by applying a cylinder saturated with a scented body to the end of the olfactometer, and varying the length of the two odoriferous substances. But he considers a double olfactometer better (one tube for each nostril). With this, one may easily experience how one odour will overwhelm another; rubber, *e.g.*, causing the smells of paraffin, wax, and tolu to disappear. Even with very strong excitants, there is never a mingling of sensations. Either the one or the other odour is perceived, till by careful equilibration of the two, no sensory effect at all is perceived. Sensibility is quite eliminated.

THE Verein für Erdkunde, of Halle, is arranging for a hydrographical and zoological investigation of the Lake of Ploen, in Holstein.

VIENNA and Berlin will shortly be connected by telephone.

A PRETTY and convenient celluloid paper knife is being sent by Messrs. Woodhouse and Rawson United, Limited, to their clients. No one who uses it can doubt that celluloid may for some purposes be a very good substitute for ivory.

MESSRS. WILLIAM WESLEY AND SON have issued No. 99 of their "Natural History and Scientific Book Circular." It consists of a list of works in astronomy, mathematics, and physics.

A PAPER upon phosphorus trifluoride is contributed by M. Moissan to the February number of the *Annales de Chimie et de Physique*. In a previous communication it was shown that this interesting gas could be obtained either by heating a mixture of lead fluoride and copper phosphide, or by the action of arsenic trifluoride upon phosphorus trichloride. Since that time it has been found that a regular and more rapid evolution of phosphorus trifluoride occurs when a mixture of zinc fluoride and phosphorus tribromide is gently warmed, and this appears to be by far the most convenient way of obtaining the gas in quantity. Zinc fluoride reacts much more rapidly than lead fluoride, and is best prepared by the action of pure hydrofluoric acid upon zinc carbonate. The insoluble fluoride thus obtained is washed with distilled water and dried at 200° C. It is important not to raise the temperature beyond this point, as further heating renders it much less easily attacked by phosphorus tribromide. The dry zinc fluoride is then placed in a brass tube closed at one end and fitted at the other with a double bored ordinary cork, well paraffined, and through which pass two tubes, one a delivery tube of lead, and the other a kind of dropping funnel, from which the tribromide of phosphorus is allowed to slowly fall upon the gently warmed fluoride of zinc. As soon as the temperature of the latter has begun to rise, the action becomes very energetic, and in a few moments several litres of the gas may be collected. In order to free the phosphorus trifluoride from admixed vapour of phosphorus tribromide, it is quite sufficient to allow it to bubble through a little water contained in a small wash bottle, after which it may be dried by passing through tubes containing pumice, which has been boiled in strong oil of vitriol, and heated until only the minimum quantity of sulphuric acid remains adhering to it, inasmuch as the strong acid absorbs a notable quantity of phosphorus trifluoride. The gas is finally collected over mercury. The reaction occurring during the preparation is stated to be as follows:—

$$3ZnF_2 + 2PBr_3 = 2PF_3 + 3ZnBr_2.$$

Gaseous trifluoride of phosphorus as thus prepared possesses a very sharp odour, but does not fume in the air. It is very slowly absorbed by water, but is decomposed immediately by solutions of chromic acid or potassium permanganate. As the above reaction appears to yield the gas in a very pure state, M. Moissan has made determinations of its density, and finds it to be 3·03. The calculated density of PF_3 is 3·08. When a measured quantity of the gas is heated over mercury in a closed glass vessel, it is totally decomposed by the silica of the glass, and the volume diminishes by one-fourth, four molecules of PF_3 becoming converted into three molecules of gaseous silicon tetrafluoride, SiF_4.

THE additions to the Zoological Society's Gardens during the past week include a Ring-tailed Lemur (*Lemur catta*) from Madagascar, presented by the executors of Dr. Allen; a Vulpine Phalanger (*Phalangista vulpina* ♀) from Australia, presented by Mr. W. H. Seward; a Hamster (*Cricetus frumentarius*) from Russia, presented by Mr. Harold Hanauer, F.Z.S.; an Alligator (*Alligator mississippiensis*) from Florida, presented by Mr. A. B. Archer; a Hoffmann's Sloth (*Cholopus hoffmanni*) from Panama, deposited.

OUR ASTRONOMICAL COLUMN.

OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at Greenwich at 10 p.m. on February 3 = 7h. 35m. 32s.

Name.	Mag.	Colour.	R A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G.C. 1546	—	—	7 29 42	+35 28
(2) DM. + 14° 1729...	6	Yellowish-red.	7 35 51	+14 28
(3) β Geminorum ...	2	Yellowish-white.	7 38 36	+28 18
(4) α Canis Minoris ...	1	Bluish-white.	7 33 30	+ 5 31
(5) 89 Schj.	7	Yellowish-red.	7 2 55	- 11 47
(6) S Hydræ	Var.	Reddish-yellow.	8 47 50	+ 3 29

Remarks.

(1) The General Catalogue description of this nebula is as follows:—"Pretty bright; considerably small; round; very gradually a very little brighter in the middle; mottled as if with stars; almost planetary." The spectrum of the nebula has not yet been recorded.

(2) Dunér describes the spectrum of this star as a very fine example of the Group II. type. He states that all the bands 2–8 are wide and dark, especially 2 and 3, and that the whole spectrum is well developed. No mention is made of the presence or absence of absorption lines, but there is little doubt that some will be found if looked for, the predominance of the bands 2 and 3 probably indicating that the star belongs to a later species, and is therefore approaching Group III., in which line absorption is predominant. Observations of the green and blue carbon flutings are also suggested (see p. 305).

(3) This star has hitherto been described as having a spectrum of the solar type. The usual observations, as to whether the temperature of the star is increasing (Group III.) or decreasing (Group VI.) are required.

(4) Gothard classes Procyon with stars of Group IV., but the Henry Draper Memorial photograph of the spectrum seems to indicate that it would be more properly described as an early stage of Group V., differing from the solar spectrum in having the hydrogen lines more developed and the metallic lines slightly thinner. Further observations of the visible spectrum are suggested.

(5) According to Dunér the spectrum of this star belongs to Group VI., and shows the usual three absorption bands of carbon. Band 6, which appears to be the most variable, is stated in this case to be very dark, and the question is, Are there any other variations in the spectrum accompanying the condition in which band 6 is dark? It seems probable that the number and intensities of the secondary bands will be found to vary with band 6, and these should, therefore, receive special attention.

(6) This variable has a spectrum of the Group II. type, but Dunér does not give a complete description, as he probably did

not observe it at maximum. A further examination is therefore required. Bright lines should also be carefully looked for, in order to determine whether the appearance of bright lines at the maxima of stars of Group II. is general. The period is given by Gore as 256 days, and the range as from 7·5–8·5 at maximum to < 12·2 at minimum. The maximum will occur on February 24.

A. FOWLER.

SPECTRUM OF THE ZODIACAL LIGHT.—In this month's *Observatory*, Mr. Maxwell Hall gives the results of a series of observations of the zodiacal light made at Jamaica. The observations are divided into three groups, according to the angular distance from the sun of the part of the zodiacal light observed. With respect to the first group, made at a distance of 50° from the sun, it is noted that the spectrum was seen as a faint white continuous band, commencing suddenly at λ 561, and extending as far as G, where it died out very gradually. The limit was well determined by comparison with the carbon flutings at $\lambda\lambda$ 470, 517, and 564. The result of the second group of observations, made at a distance of 22° from the sun, showed that the spectrum commenced at λ 561, but not so suddenly; its feeble maximum was transferred to about λ 517; from thence it was tolerably uniform to about λ 497, and then it gradually diminished and faded away at G.

The observations made at a distance of 15° from the sun gave λ 562 for the limit of the red end of the spectrum, and G as before for the violet end. But the spectrum did not commence at all suddenly: the stronger maximum was still at λ 517: it was fairly uniform from thence to λ 497, and then faded away.

Observations of twilight are needed to determine whether, as it grows more and more faint, the maximum appears to shift towards the red end of the spectrum or not; if not, the change in intensity of portions of the spectrum of the zodiacal light as observations are made at varying distances from the sun are peculiar to it, and need further investigation.

SOLAR AND STELLAR MOTIONS.—Prof. J. R. Eastman, in his address as retiring President of the Philosophical Society of Washington, delivered December 7, 1889, gave an exhaustive account of the investigations that have been made to determine the co-ordinates of the solar apex and the annual value of the motion of the solar system. His investigations into the relation between stellar magnitudes, distances, and motions, show that, in opposition to the assumption generally accepted, which asserts that the largest stars are nearest the solar system, there is an almost uniformly *increasing* proper motion as the stars grow fainter. Forty-six stars, that is, practically all those whose parallaxes have been well determined, have been tabulated and arranged in five nearly equal groups according to the magnitude of their proper motion. The following table gives the mean results found for each of the groups:—

	Number of Stars in Group.	Mean Magnitude.	Mean Proper Motion.	Mean Parallax.
			"	"
1st Group	9	5·57	4·93	0·32
2nd ,,	9	5·59	2·33	0·20
3rd ,,	9	3·37	1·04	0·20
4th ,,	9	2·36	0·38	0·16
5th ,,	10	2·84	0·06	0·13

The mean magnitude of the first two groups is 5·58, and the mean proper motion is 3"·63. Of the last three groups the mean magnitude is 2·86, and the mean proper motion is 0"·49.

If the 46 stars investigated be arranged according to the magnitude of their parallaxes, it is found that 18 of them have a parallax greater than 0"·2. The mean magnitude of these stars is 5·56, and the mean parallax is 0"·34. Of the remaining 28 stars the mean magnitude is 2·89, and the mean parallax is 0"·11. From this it would appear that, if any law can be formulated from the observed data, it must be that the fainter rather than the brighter stars are nearest the solar system.

DUN ECHT OBSERVATORY.—The Earl of Crawford, in a circular issued on the 29th ult., expresses his thanks for the hearty co-operation he has met with at all hands in his endeavours to advance the science of astronomy. Although some little time will elapse before all the instruments can be removed from Dun Echt to the Royal Observatory at Edinburgh, the former observatory must be looked upon as closed, and the generous donor trusts that the astronomical friends who have for years continued to enrich the library at Dun Echt Observatory with donations of books and pamphlets will extend their liberality to the new home of the collection at Edinburgh. The important astronomical work done by the Earl of Crawford personally, and at his observatory, has contributed, in no slight degree, to the progress of astronomy, and the very generous gift to the nation of the entire contents of the observatory at Dun Echt is worthy of the man, and appreciated by all friends of the science throughout the world.

MELBOURNE OBSERVATORY.—We have received from Mr. Ellery the volume containing the results of transit circle observations made from the beginning of 1881 to the end of August 1884. The separate results for R.A. and N.P.D. have been taken directly from the transit books, and also the observer's estimates of the magnitude. The places and magnitudes of the stars given in the annual catalogues have been derived from these separate results by taking their arithmetical mean.

GEOGRAPHICAL NOTES.

AT the meeting of the Royal Geographical Society on Monday, Mr. Douglas W. Freshfield read a most interesting paper on "Search and Travel in the Caucasus: an account of the discovery of the fate of the party lost in 1888." He began by acknowledging his obligations to M. de Stael, the Russian Ambassador to the Court of St. James's, the officials at Vladikavkaz, and more particularly to MM. Jukoff and Bogdanoff, of the Russian Survey, for the facilities and assistance given to him and his companions in carrying out the object of his journey. The topographical information accumulated by the surveyors had been placed at his disposal with the greatest readiness, and part of the result might be seen in the great map (6 inches to the mile) of the central group hung on the wall. The heights of the principal peaks were now ascertained. There were eight higher than Mont Blanc, and fifteen of over 15,000 feet. The four highest are Elbruz, Koshtantau, Shkara, and Dychtau. Ushba is 15,600 feet. Mr. Freshfield briefly described the new carriage pass, the Mamison, 9400 feet, from Vladikavkaz to Kutais. Its scenery is finer than that of the Dariel, and the road has been well engineered, but it will shortly fall into ruin unless a service is organized for its maintenance. He referred to the remarkable old Ossete sanctuary of Rekom, at the foot of the Ceja Glacier, and to the tombs found at Chegem, and exhibited a collection of metal and other objects discovered mostly at Styr Degir. In many villages small settlements of "Mountain Jews" were found. There were over 20,000 of this race in the Caucasus, and a work on them has lately been published at Moscow. The author, M. Mirimisoff, states that their beliefs and superstitions are singular, and show Persian influence, but they have had for centuries no connection with the rest of their race, from which they were probably separated at a very early date. The party had crossed five high glacier passes before reaching Suanetia. Here Mr. Freshfield and Captain Powell were the guests of Prince Atar Dadish Kilia, the representative of the family who once ruled Lower Suanetia. He now spends a few months in the summer at his house at Ereri, dispensing hospitality in feudal fashion among his retainers. The population assembles every Sunday for games on the green, and the women sing ballads recounting incidents in local history or tales of love and revenge. The Leila peaks (13,400 feet) south of Suanetia were ascended for the first time. They are pre-eminent in forests and flowers. One of the glaciers falls over a cliff in avalanches into a glen which is a bed of wild roses and yellow lilies, growing often with fourteen blooms on one stalk. From Suanetia to Sukhum Kaleh the travellers forced a way with mules through an almost trackless forest, and down the deserted valley of the Kodor, the region that was once Abchasia. Strange tales are told of the forest, even by Russian officials, who declared that a wild race, without villages, arms, or clothes, haunted its recesses. No one was met, however, but a few hunters and shepherds. But considerable difficulty was met with in forcing a way through the tangle of fallen timber and finding a passage over the torrents, and the native guides employed deserted the travellers before they reached Lata, the first Russian station on the Kodor. Mr. Freshfield proceeded to relate in detail the incidents of the search undertaken by Mr. C. Dent and himself, with the aid of Mr. H. Woolley and Captain Powell, for traces of the fate of the mountaineers, Mr. W. F. Donkin, Mr. H. Fox, and two Meiringen guides, lost in August 1888. It was known, from a note in a diary left by Mr. Fox in a lower

camp with his heavy luggage, that the lost party had set out from the Dumala Valley in the Bezingi District, with the hope of climbing Dychtau, 16,880 feet, from the south-east. Karaoul, at the head of the Cherek Valley, was made, therefore, the head-quarters of the search party. They bivouacked under a rock beside the Tutuin Glacier, at a height of 9400 feet. Next morning (July 29) they started at dawn, and forced, not without difficulty, a passage through the monstrous *séracs* of the Tutuin Glacier. Above them they found a long snowy corridor leading to the base of Dychtau, and to the foot of a gap in its east spur, which they believed Mr. Donkin and his companions had crossed from the Dumala glen on the further side. Nothing was found at the foot of the steep rock wall, 1400 feet high, which protected the pass. The searchers therefore climbed the rocks leading to it, and when 1000 feet above the snow and some 400 below the ridge, the traces sought were met with. The leader at the rope's end suddenly stopped short and gasped, "See, here is the sleeping-place." Before our eyes rose a low wall of loose stones built in a semicircle convex to the lower precipice. A crag partially overhung it; any object dropped over the wall fell 1000 feet on to the snow plain below. The space, some 6 feet square, inside the wall, was filled with uneven snow or ice, from which portions of knapsacks and sleeping bags protruded. A black stew-pan, half full of water, in which a metal cup floated, lay against the rock; a loaded revolver was hung beside it. It cost more than three hours' hard work to dig out all the objects from the frozen stuff in which they were embedded. Only three could work at once in the narrow space, and Mr. Freshfield and Mr. Woolley went on to the ridge, where they found a small stoneman, but no written record. Some manuscript notes and maps of Mr. Fox's were found in the bivouac, but nothing written after leaving the lower camp. The whole of the cliff and cliff's foot were carefully searched with a strong telescope. Mr. Woolley and his guides twice passed along the cliff's foot on his ascent of Dychtau, and he made certain that the party had not climbed the peak—that the accident therefore had happened on the ascent. After the lecture, Mr. Freshfield showed in the lantern a series of views of the Caucasus, from photographs by Mr. Hermann Woolley and Signor V. Sella. A complete set of Signor Sella's views, embracing eight panoramas and 90 views, was shown in an adjoining room. The panorama from Elbruz shows the whole chain of the Caucasus above a sea of clouds, and is probably the finest mountain photograph yet exhibited.

THE last issue of the *Izvestia* of the Russian Geographical Society is more than usually interesting, as it contains detailed letters received from the members of the three Russian expeditions now engaged in the exploration of Central Asia. The letter of M. Roborovsky, dated August 16, and written in the highlands to the south of Yarkend, contains a most vivid description of the journey from the town Prjevalsk to Yarkend, across the passes of Barskaun and Bedel. M. Roborovsky knows Central Asia well, as he was Prjevalsky's travelling companion during three of his great journeys; and his descriptions of the country—its orography, climate, and flora—are full of most valuable information. Another letter is from M. Bogdanovitch, the geologist of the expedition, who joined it at Yarkend, after having crossed the Kashgarian Mountains on another route and explored the Mustagh-ata glaciers. That part of the Pamir border-ridge had already been explored by Stoliczka, but M. Bogdanovitch adds much new information. It appears—as might have been expected from the orography of the region—that there is no trace of mountains running north and south on the eastern edge of the great Pamir plateau. The Kashgar Mountains are an upheaval of gneisses, metamorphic slates, and Tertiary deposits, running from north-west to south-east. The limestones which Stoliczka supposed to be Triassic, proved to be Devonian. The most characteristic fossils of the Upper Devonian (*Atrypa reticularis*, *A. latilinguis*, *A. aspera*, *Spirifer Verneuli*, and several others) were found together with the corals (*Lithodendron*), Stromatoporæ and *Cerioporæ* described by Stoliczka. The Tertiary sandstones are broken through (as is often the case in Siberia) by dolerites of volcanic origin, at the very border of the plateau, on its slope turned towards Kashgaria. Another series of letters, the last of which is dated September 23, from the sources of the Aksu, is from Colonel Grombchevsky. The late spring delayed the advance of the expedition, which spent the first part of June in crossing the Alai Mountains. The great Alai Valley of the Pamir could be reached only on June 19, but the Trans-Alai Mountains were buried in snow; no passage was possible, and the explorer was compelled to march to the lower tracts of Karategin. He thence proceeded to Kala-i-khum, a little town situated on the Pendj, at a height of 4500 feet, and enjoying a relatively mild climate. From Kala-i-khum M. Grombchevsky succeeded in reaching the Vantcha river; but having met there the Afghan troops which were taking possession of the khanates of Shugnan and Rothan, he could not move further south, nor explore the western parts of the Pamir; so he proposed to continue the exploration of the eastern parts of the Roof of the World. Finally, the two brothers, Grum-Grzimailo, who are exploring the Eastern Tian-Shan from Kuldja to Urumtsi, give short news of their progress, and remark that our maps of Eastern Tian-Shan are quite incorrect—a circumstance which might have been guessed from the general orographical structure of Central Asia. The collections of vertebrates and insects which have been gathered by the two explorers are exceedingly rich.

A PERMANENT Marocco museum is to be established at the head-quarters of the Society of Commercial Geography at Berlin.

SMOKELESS EXPLOSIVES.[1]

II.

SO far as smokelessness is concerned, no material can surpass *gun-cotton* pure and simple; but, even if its rate of combustion in a firearm could be controlled with certainty and uniformity, although only used in very small charges, such as are required for military rifles, its application as a safe and reliable propulsive agent for military and naval use is attended by so many difficulties, that the non-success of the numerous attempts, made in the first twenty-five years of its existence, to apply it in this direction, is not surprising.

Soon after its discovery by Schönbein and Böttger in 1846, endeavours were made to apply gun-cotton wool, rammed into cases, as a charge for small arms, but with disastrous results. Subsequently von Lenk, who made the first practical approach to the regulation of the explosive power of gun-cotton, produced small-arm cartridges by superposing layers of gun-cotton threads, these being closely plaited round a core of wood. Von Lenk's system of regulating the rapidity of burning of gun-cotton, so as to suit it either for gradual or violent action, consists, in fact, in converting coarse or fine, loosely or tightly twisted, threads or rovings of finely carded cotton into the most explosive form of gun-cotton, and of arranging these threads or yarns in different ways so as to modify the mechanical condition, *i.e.* the compactness and extent and distribution of enclosed air-spaces, of the mass of gun-cotton composed of them. Thus, small-arm cartridges were composed, as already stated, of compact layers of tightly-plaited, fine gun-cotton thread; cannon cartridges were made up of coarse, loose gun-cotton yarn wound very compactly upon a core; charges for shells consisted of very loose cylindrical hollow plaits (like lamp wicks), along which fire flashed almost instantaneously; and mining charges were made in the form of a very tightly twisted rope with a hollow core. While the two latter forms of gun-cotton always burned with almost instantaneous rapidity in open air, and with highly destructive effects if they were strongly confined, the tightly wound or plaited masses burned slowly in air, and would frequently exert their explosive force so gradually when confined in a firearm as to produce good ballistic results without appreciably destructive effect upon the arm. Occasionally, however, in consequence of some slight unforeseen variation in the compactness of the material, or in the amount and disposition of the air-spaces in the mass, very violent action would be produced, showing that this system of regulating the explosive force of gun-cotton was quite unreliable.

Misled by the apparently promising nature of the earliest results which von Lenk obtained, the Austrian Government embarked, in 1862, upon a somewhat extensive application of von Lenk's gun-cotton to small arms, and provided several batteries of field guns for the use of this material. The abandonment of these measures for applying a smokeless explosive to military purposes soon followed upon the attainment of unsatisfactory results, and was hastened by the occurrence of a very destructive

[1] Friday Evening Discourse delivered by Sir Frederick Abel, F.R.S., at the Royal Institution of Great Britain, on January 31, 1890. Continued from p. 330.

explosion at gun-cotton stores at Simmering, near Vienna, in 1862.

It was at about this time that the attention of the English Government, and through them of the lecturer, was directed to the subject of gun-cotton, the Austrian Government having communicated details regarding improvements in its manufacture accomplished by von Lenk, and results obtained in the extended experiments which had been carried out on its application to the various purposes above indicated, according to the system devised by that officer. One of the results of the lecturer's researches, subsequently carried on at Woolwich and Waltham Abbey, was his elaboration of the system of manufacture and employment of gun-cotton which has been in extensive use at the Government works with little if any modification for over eighteen years, and has been copied from us by France, Germany, and other countries. By reducing the partially purified gun-cotton fibre to pulp, as in the ordinary process of making paper, then completing its purification when in that condition, and afterwards converting the finely-divided explosive into highly compressed homogeneous masses of any desired form and size, very important improvements were effected in its stability, its uniformity of composition and action, and its adaptability to practical uses, a great advance being made in the exercise of control over the rapidity of combustion or explosion of the material.

No success had attended the experiments instituted in England with wound cannon cartridges of gun-cotton threads made according to von Lenk's plan; on the other hand, a number of results which at first sight appeared very promising were obtained at Woolwich in 1867–68 with bronze field-guns and cartridges built up of compressed gun-cotton masses arranged in different ways (with varied air-spaces, &c.) with the object of regulating the rapidity of explosion of the charge. But although the attainment of high velocities with comparatively small charges of the material, unaccompanied by any indications of injury to the gun, was frequent, it became evident that the fulfilment of the conditions essential to safety to the arm were exceedingly difficult to attain with certainty, and appeared indeed to be altogether beyond absolute control, even in so small a gun as the twelve-pounder. Military authorities not being, in those days, alive to the advantages which might accrue from the employment of an entirely smokeless explosive in artillery, the lecturer received no encouragement to persevere with experiments in this direction, and the same was the case with respect to the possible use of a smokeless explosive in military small arms, with which, however, far more promising results had at that time been obtained at Woolwich.

Abel's system of preparing gun-cotton was no sooner elaborated than its application to the production of smokeless cartridges for sporting purposes was achieved with considerable success by Messrs. Prentice, of Stowmarket. The first gun-cotton cartridge, which found considerable favour with sportsmen, consisted of a roll of felt-like paper composed of gun-cotton and ordinary cotton, and produced from a mixture of the pulped materials. Afterwards a cylindrical pellet of slightly compressed gun-cotton pulp was used, the rapidity of explosion of which was retarded, while it was at the same time protected from absorption of moisture, by impregnation with a small proportion of india-rubber. Neither of these cartridges afforded promise of sufficient uniformity of action to fulfil military requirements, but after a series of experiments which the lecturer made with compressed gun-cotton arranged in various ways, very promising results were attained, especially with the Martini-Henry rifle and a charge of pellet-form, the rapidity of explosion of which was regulated by simple means.

A sporting powder which was nearly smokeless had, in the meantime, been produced by Colonel Schultze, of the Prussian Artillery, from wood cut up into very small cube-like fragments, converted into a mild form of nitro-cellulose after a preliminary purifying treatment, and impregnated with a small portion of an oxidizing agent. Subsequently the manufacture of the Schultze powder was considerably modified; it was converted into the granular form, and rendered considerably more uniform in character and less hygroscopic, and it then bore considerable resemblance to the E.C. powder, a granulated nitro-cotton powder, produced, in the first instance, at Stowmarket, and consisting of a less highly nitrated cotton than gun-cotton (trinitrocellulose), incorporated in the pulped condition with a somewhat considerable proportion of the nitrates of potassium and barium, and converted into grains through the agency of a solvent and a binding material. Both of these powders produced some smoke when fired, though the amount was small in comparison with that from black powder. They did not compete with the latter in regard to accuracy of shooting, when used in arms of precision, but they are interesting as being the forerunners of a variety of so-called smokeless powders, of which gun-cotton or nitro-cotton is the basis, and of which those of Johnson and Borland, and of the Smokeless Powder Company, are the most prominent in this country.

In past years, both camphor and liquid solvents, such as acetic ether and acetone for gun-cotton, and mixtures of ether and alcohol for nitro-cotton, have been applied to the hardening of the surfaces of compressed masses or granules of those materials, by von Förster and others, with a view to render them non-porous, and in the E.C. powder manufacture the latter solvent was thus applied to harden the powder-granules. In the Johnson-Borland powder camphor is applied to the same purpose; in smokeless powders of French and German manufacture acetic ether and acetone have been used, and the solvent has been applied not merely to harden the granules or tablets of the explosive, but also to convert the latter into a homogeneous horn-like material.

Much mystery has surrounded the nature and origin of the first smokeless powder adopted, apparently with undue haste, by the French Government, for use with the Lebel magazine rifle. A few particles of the Vieille powder, or *Poudre B*, were seen by the lecturer about two years ago, and very small specimens appear to have fallen into the hands of the German Government about that time. They were in the form of small yellowish-brown tablets of about 0·07 inch to 0·1 inch square, of the thickness of stout notepaper, and had evidently been produced by cutting up thin sheets of the material. They appeared to contain, as an important ingredient, picric acid (the basis of "mélinite") a substance extensively used as a dye, and obtained by the action of nitric acid, at a low temperature, upon carbolic acid and cresylic acid, constituents of coal tar. Originally produced by the action of nitric acid upon indigo, and afterwards by similar treatment of Botany Bay gum, it was first known as carbazotic acid, and is one of the earliest of known explosives of organic origin. When sufficiently heated, or when set light to, it burns with a yellow smoky flame, and even very large quantities of it have been known to burn away somewhat fiercely, but without exploding. Under certain conditions, however, and especially if subjected to the action of a powerful detonator, it explodes with very great violence and highly destructive effects, as pointed out by Sprengel in 1873, and recent experiments at Woolwich have shown that it does this even, as in the case of gun-cotton, when it contains as much as 15 per cent. of water. It is no longer a secret that picric acid at any rate forms the basis of the much-vaunted and mysterious explosive for shells for which the French Government were said to have paid a very large sum of money, and the destructive effects of which have been described as nothing less than marvellous. M. Turpin patented, in 1875, the use of picric acid alone as an explosive for shells and for other engines of destruction, and whether or not his claims to be the inventor of mélinite are valid, there appears no doubt that his patent in France was the starting-point of the development and adoption of that explosive.

The attention thus directed in France to the properties of picric acid appears to have given rise to experiments resulting in its employment as an ingredient of the first smokeless powder (*Poudre B*) adopted for the French magazine rifle.

The idea of employing picric acid preparations as explosive agents for propulsive purposes originated with Designolle about twenty years ago, but no useful results attended the experiments with the particular mixtures proposed by him. It is certain that the recent adaptation of that substance in France was of a different character, and that, promising as were the results of the new smokeless powder, of which it formed an ingredient, and of which a counterpart was made the subject of experiments at Woolwich about three years ago, its deficiency in the all-essential quality of stability must have been at any rate one cause of its abandonment in favour of another form of smokeless powder, which there is reason to believe is of more simple character.

In Germany, the subject of smokeless powder for small arms and artillery was being steadily pursued in secret, while the sensational reports concerning *Poudre B* were spread about in France, and a small-arm powder, giving excellent results in regard to ballistic properties and uniformity, was elaborated at

the Rottweil powder-works, and appears to have been adopted into the German service for a time, but its first great promise of success seems to have failed of fulfilment through defects in stability.

Reference has already been made to the conversion of gun-cotton (trinitrocellulose), and to mixtures of it with less explosive forms of nitrated cotton (or nitrated cellulose of other description), by the action of solvents into horn-like materials. These are in the first instance obtained in the form of gelatinous masses, which, prior to the complete evaporation or removal of the solvent, can be pressed or squirted into wires, rods, or tubes, or rolled or spread into sheets; when they have become hardened, they may be cut up into tablets or into strips or pieces of size suitable for conversion into charges or cartridges. Numerous patents have been secured for the treatment of gun-cotton, nitro-cotton, or mixtures of these with other substances, by the methods indicated; but in this direction the German makers of the powder just now referred to seem to have secured priority. Experiments were made about a year and a half ago with powder produced in this way at Woolwich, and the Wetteren Powder Company in Belgium has also manufactured so-called paper powders, or horn-like preparations, of the same kind, which were brought forward as counterparts of the French small-arm and artillery smokeless powder.

Mr. Alfred Nobel, to whom the mining world is so largely indebted for the invention of dynamite, and of other very efficient blasting agents of which nitro-glycerine is the basis, was the first to apply the latter explosive agent, in conjunction with one of the lower products of nitration of cellulose, to the production of a smokeless powder. The powder bears great resemblance to one of the most interesting of known violent explosives, also invented by Mr. Nobel, and called by him blasting gelatine, in consequence of its peculiar gelatinous character. When the nitro-cotton is impregnated and allowed to digest with nitro-glycerine, it loses its fibrous nature and becomes gelatinized while assimilating the nitro-glycerine, the two substances furnishing a product which has almost the character of a compound. By macerating the nitro-cotton with from 7 to 10 per cent. of nitro-glycerine, and maintaining the mixture warm, the whole soon becomes converted into a plastic material from which it is very difficult to separate a portion of either of its components. This preparation, and certain modifications of it, have acquired high importance as blasting agents more powerful than dynamite, and possessed of the valuable property that their prolonged immersion in water does not separate from them any appreciable proportion of nitro-glycerine.

In the earlier days of the attempted application of blasting gelatine to military uses, in Austria, when endeavours were there made to render the material less susceptible of accidental explosion on active service (as by the penetration of bullets or shell fragments into transport waggons containing supplies of the explosive), this result was achieved by Colonel Hess by incorporating with the components a small proportion of camphor, a substance which had then, for some time past, played an important part in the technical application of nitro-cotton to the production of the remarkable substitute for ivory, horn, &c., known as xylenite. By incorporating with nitro-glycerine a much larger proportion of nitro-cotton than used in the production of blasting gelatine, and by employing camphor as an agent for promoting the union of the two explosives, as well as, apparently, for deadening the violence, or reducing the rapidity of explosion of the product, Mr. Nobel has obtained a material of almost horn-like character, which can be pressed into pellets or rolled into sheets while in the plastic condition, and which compares favourably with the gun-cotton preparations of somewhat similar physical characters just referred to, as regards ballistic properties, stability, and uniformity, besides being almost absolutely smokeless. The retention in its composition of some proportion of the volatile substance camphor, which may gradually be reduced in amount by evaporation, renders this explosive liable to undergo some modification in its ballistic properties in course of time; it is believed that this point has been dealt with by Mr. Nobel, and accounts from Italy speak favourably of the results of trials of his powder in small arms, while Mr. Krupp is reported to be carrying on experiments with it in guns of several calibres.

The Government Committee on Explosives, in endeavouring to remedy the above defect of Nobel's original powder, were led by their researches to the preparation of other varieties of nitro-glycerine powder, which, when applied in the form of wires or rods, made up into sheaves or bundles, have given, in the service small-bore rifle, excellent ballistic results. The most promising of them, which fulfils, besides, the conditions of smokelessness and of stability, so far as can be guaranteed by the application of special tests of exposure to elevated temperatures, &c., is now being submitted to searching experiments with the view of so applying it in the arm as to overcome certain difficulties attending the employment, in a very small-bore rifle, of an explosive developing much greater energy than the black-powder charge, which therefore gives very considerably higher velocities even with much smaller charges, and consequently heats the arm much more. Thus, the service black-powder charge furnishes, with the small-bore rifle, an average (and variable) velocity of 1800 f.s., together with pressures ranging from 18 to 20 tons per square inch; on the other hand, with considerably less of the powder referred to, there is no difficulty in securing a very uniform velocity of about 2200 f.s. with pressures not exceeding 17 tons, while velocities as high as 2500 f.s. are obtainable with pressures not greater than the maximum allowed with the black-powder charge.

It is obvious, from what has already been said respecting the causes of the erosive action of powder in guns, that comparatively considerable erosive effects would be expected to be produced by powders of high energy as compared with black powder. Moreover, the freedom of the products of explosion from any solid substances, and consequently the absence of any fouling or deposition of residue in the arm, causes the heated surfaces of the projectile and of the interior of the barrel to remain clean, and in a condition, therefore, very favourable to close adherence together. If to these circumstances be added the fact that the behaviour of the smokeless powder has to be adapted to suit an arm, a cartridge, and a projectile originally designed for use with black powder, it will be understood that the devising of an explosive which shall be practically smokeless, sufficiently stable, and susceptible of perfectly safe use in the arm under all service conditions, easy of manufacture, and not too costly, is, after all, but a small part of the difficult problem of adapting a smokeless powder successfully to the new military rifle—a problem which, however, appears to be on the near approach to satisfactory solution.

The experience already acquired in guns ranging in calibre from 1·85 inches to 6 inches, with the smokeless powder devised for use in our service, has been very promising, and indicates that the difficulties attending its adaptation to guns designed for black powder are likely to prove considerably less than in the case of the small arm. But here, again, the circumstances that much smaller charges are required to furnish the same ballistics as the service black-powder charges, and that the comparatively gradual and sustained action of the new powder gives rise to lower pressures in the chamber of the gun, and higher pressures along the chase, demonstrate that the full utilization of the ballistic advantages, and the increase in the power of guns of a given calibre and weight with the new form of powder, are only attainable by some modifications in the designs of the guns—such as a reduction in size of the charge-chamber, and some additions to the strength, and perhaps, in some cases, of the length, of the chase.

When, however, the smokeless powder has been adapted with success in all respects to artillery, from small machine-guns to guns of comparatively heavy calibre, and when its ballistic advantages have been fully utilized in guns of suitable design, it will remain to be determined how far such a powder—undeniably of much more sensitive constitution than black powder, or any of its modifications—will withstand, unchanged and unharmed, the various vicissitudes of climate, and the service storage-conditions in ships and on land in all parts of the world—a condition essential to its adaptability to naval and military use, and especially to the service of our Empire; and whether sufficient confidence can be placed in its stability for long periods under these extremely varied conditions to warrant the necessary freedom from apprehension of possible danger, emanating from within the material itself, to allow of its being substituted for black powder wherever its use may present advantages.

Possible it might be, that the storage, with perfect safety, of such a powder in ships, forts, or magazines might demand the adoption of precautionary measures tending to place comparatively narrow limits upon the extent of its practicable service applications; even then, however, an imperative need for the introduction of special arrangements to secure safety and immunity from deterioration may be of small importance as

compared with the great advantages which the provision of a thoroughly efficient smokeless powder may secure to the possessor of it, especially in naval warfare.

That the opinions respecting the importance of such advantages are founded upon a sound basis, one can hardly doubt, after the views expressed by several of the highest military and naval authorities, although opinions as to their extent may differ very considerably even among such authorities.

The accounts furnished from time to time from official and private sources of the effects observed, at some considerable distance, by witnesses of practice with the smokeless powders successively adopted in France, have doubtless been regarded by military authorities as warranting the belief that the employment of such powders must effect a great revolution in the conduct of campaigns. Not only have the absence of smoke and flame been dwelt upon as important factors in such a revolution, but the recorders of the achievements of smokeless powder—whose descriptions have doubtless been to some extent influenced by the vivid pictures already presented to them of what they *should* anticipate—have even been led to make such explicit assertions as to the *noiselessness* of these powders, that high military authorities have actually been thereby misled to portray, by vivid word-painting, the contrast between the battles of the future and the past;—to imagine the terrific din caused by the discharge of several hundred field-guns and the roar of musketry in the great battles of the past, giving place to noise so slight that distant troops will no longer receive indications where their comrades are engaged, while sentries and advanced posts will no longer be able to warn the main body of the approach of an enemy by the discharge of their rifles, and that battles might possibly be raging within a few miles of columns on the march without the fact becoming at once apparent to them.

It is somewhat difficult to conceive that, in these comparatively enlightened days—an acquaintance with the first principles of physical science having for many years past constituted a preliminary condition of admission to the training establishments of the future warrior—the physical impossibility of such fairy tales as appear to be considered necessary in France for the delusion of the ordinary public, would not at once have been obvious. Yet, even in professional publications in Germany, where we are led to expect that the judgment of experts would be comparatively unlikely to be led astray through lack of scientific knowledge, we have, during the earlier part of last year, read, in articles upon the influence of smokeless powder upon the art of war (based evidently upon the reports received from France), such passages as these:—"The art of war gains in no way as far as simplicity is concerned; on the contrary, it appears to us that the absence of so important a mechanical means of help as *noise* and smoke were to the commander, requires increased skill and circumspection in addition to the qualities demanded by a general. . . ." "The course of a fight will certainly be mysterious, on account of the *relative stillness* with which it will be carried on."

In an amusing article, in imitation of the account of the Battle of Dorking, which appeared in the *Deutsche Heeres Zeitung* of April last, the consternation is described with which a battalion receives the information from a wounded fugitive from the outposts that the enemy's bullets have been playing havoc among them, without any visible or audible indications as to the quarter of attack. Later in the year, and especially since the manœuvres before the German and Austrian Emperors, when the employment of the new smokeless powder was the event of the day, the absurdity of the assertions as to the noiselessness of the new powders became a theme for strong observations in the German service papers; the assumed existence of a noiseless powder was ridiculed as a thing equally impossible with a recoil-less powder; the violence of the report, or explosion, produced upon the discharge of a firearm being in direct relation to the volume and tension of the gaseous matter projected into the surrounding air.

The circumstance that blank ammunition was alone used in the smokeless powder exhibition at the German manœuvres, may have served to lend some support to the assertions as to *comparatively* little noise made by the powder—the report of blank cartridges being slight, on account of the small and lightly confined charges used. It is said that the sound of practice with blank ammunition at the German manœuvres, was scarcely recognized at a distance of 100 metres. In a recently published pamphlet on the results of employment of the latest German smokeless powder in the manœuvres, it is stated, on the other hand, that the difference between the violence of the report of the new powder and of black powder is scarcely perceptible: that it is sharper and more ringing, but not of such long duration. This description accords exactly with our own experience of the reports produced by different varieties of smokeless powder, and of the lecturer's earlier experience with gun-cotton charges fired from rifles and field guns. The noise produced by the latter was decidedly more ringing and distressing to the ear in close proximity to the gun, but also of decidedly less volume than the report of a black-powder charge, when heard at a considerable distance from the gun.

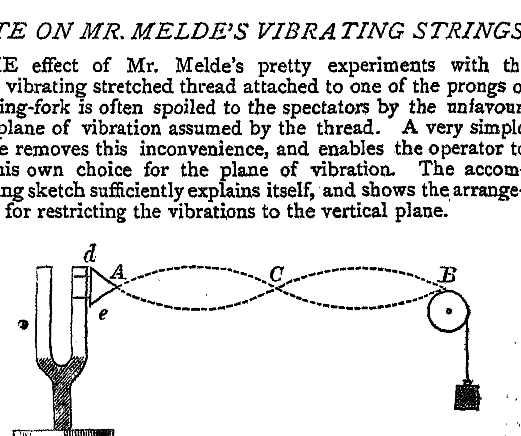
As regards smokelessness, the present German service powder is not actually smokeless, but produces a thin, almost transparent, bluish cloud, which is immediately dissipated. Independent rifle-firing was not rendered visible by the smoke produced at a distance of 300 metres, and at shorter ranges the smoke presented the appearance of a puff from a cigar. The most rapid salvo-firing during the operations near Spandau did not have the effect of obscuring those firing from distant observers.

That, in future warfare, if smokeless or nearly smokeless powders have maintained their position as safe and reliable propelling agents for small arms and field artillery, belligerents of both sides will be alike users of them, there can be no doubt. The consequent absence of the screening effect of smoke—which, on the one hand, removes an important protection and the means of making rapid advances or sudden changes of position in comparative safety, and, on the other hand, secures to both sides the power of ensuring to the fullest extent accuracy of shooting, and of making deadly attack by individual fire through the medium of cover, with comparative immunity from detection—can scarcely fail to change more or less radically many of the existing conditions under which engagements are fought.

As regards the naval service, it is especially and, at present at any rate, exclusively for the new machine and quick-firing guns that a smokeless powder is wanted; for such service the advantages which would be secured by the provision of a reliable powder of this kind can scarcely be over-estimated, and their realization within no distant period may, it is believed, be anticipated with confidence.

NOTE ON MR. MELDE'S VIBRATING STRINGS.

THE effect of Mr. Melde's pretty experiments with the vibrating stretched thread attached to one of the prongs of a tuning-fork is often spoiled to the spectators by the unfavourable plane of vibration assumed by the thread. A very simple device removes this inconvenience, and enables the operator to suit his own choice for the plane of vibration. The accompanying sketch sufficiently explains itself, and shows the arrangement for restricting the vibrations to the vertical plane.



Instead of attaching the end of the thread to the prong of the tuning-fork, it is tied to the middle of a short thread *d*A*e*, and the ends *d* and *e* of this are attached to the prong in a vertical line. It is clear that if the distance of A from the line *de* is an appreciable part of the quarter wave-length of the vibration, and AB is an integral multiple of the half wave-length, vibration is possible only in the vertical plane. For in the horizontal plane this rate of vibration is impossible, A being not a fixed point of the thread for vibration in this plane, and the length from the prong to the pulley being not an integral multiple of the half wave-length of vibration. And in any other plane the vibration, if possible, would be compounded of two, viz. of the vertical which is possible and of the horizontal which is impossible.

The most convenient form of fixture for the short thread *d*A*e*, is a light steel wire with an eye at each end, lashed to the prong

with two turns of fine thread. The plane of vibration can then be easily adjusted to suit the spectators by sluing the wire in its lashing.

Note.—The triangular thread *dAe* should be of the same quality as the vibrating length. If it is much heavier length for length the arms of the triangle may become half wave-lengths of the vibration for the tension employed, and then they lose their control over the plane of vibration.

The arrangement has its own worth, independently of the aid it lends to visible effect, as an illustration of the suppression of all half wave-lengths which are not true sub-multiples of the vibrating length of the cord. When the fork is moved from its position in the figure to bring up the line *de* to the position of A, the vertical vibrations are suppressed, and only the horizontal vibrations are possible.

W. SIDGREAVES.

EIGHTH CONGRESS OF RUSSIAN NATURALISTS.

THE eighth Congress of Russian Naturalists and Physicians was opened on January 9 at St. Petersburg, and was a great success. It was attended by no fewer than 2000 members, half of whom came from the provinces, and at the three general public sittings (corresponding to the sittings of the British Association devoted to the delivery of the Presidential addresses), as well as the meetings of the Sections, the public were well represented. At the first general sitting, Prof. Mendeleeff delivered a most interesting address on the methods of natural science as applied to the study of prices. His parallels between the prices of goods and the specific weights and specific volumes of chemical bodies were very suggestive. The next address, by Prof. Sklifasovsky, was on the wants of Russian medical education. At the second general sitting, Prof. Stoletoff spoke of ether and electricity. Prof. Famintzyn's address on the psychical life of the simplest representatives of living beings, partly based upon his own recent researches into the intelligence of Infusoria, was full of facts as to the means used by various micro-organisms in attack and defence. Prof. Wagner dealt with the physiological and psychological views upon hypnotism, and Prof. Gustavson spoke of the micro-biological bases of agronomy.

The work of the Sections was very varied, and will be fully reported in the Diary of the Congress, the publication of which began during the sitting of the Congress, and will be continued till a full account has been produced.

The Sections of Geography and Anthropology, Hygiene, and partly of Agronomy, were most largely attended, and many interesting communications were made in them. At the combined sittings several important questions were raised as to the geography of Russia, its meteorology, and the bearings of a scientific study of climate and soil upon agriculture.

The following communications relative to geography and anthropology were especially worthy of note. Captain Makaroff reported the results of his careful measurements as to the differences of level of various seas of Europe. Taking the average level of the Atlantic Ocean opposite Lisbon for zero, he found that the level of the western parts of the Mediterranean is 434 millimetres below zero, its eastern part, − 507 millimetres; the Ægean Sea, − 563 millimetres; the Marmora Sea, from − 360 to − 291 millimetres; while the Black Sea is + 246 millimetres—that is, higher than the Lisbon zero; the western part of the Baltic, + 259 millimetres; its eastern part, + 254 millimetres; and the Gulf of Finland, + 415 millimetres. Dr. Blum's anthropological measurements amidst twelve different tribes of the Caucasus show that there are no pure races in Caucasia, all of them being mixtures between Semitic and Indo-European races. Like conclusions were arrived at by M. Kharuzin as regards the Bashkires, who proved to be a mixed race, presenting features both of the Mongolian and the Caucasian races.

Prof. Klossovsky's researches into the variations of level and temperature in the coast region of the Black Sea are most valuable, as they are based on accurate measurements made since 1879 at 16 different places. They fully disclose the importance of atmospheric pressure upon the level of the Black Sea, and it is worthy of note that the passage of a cyclone over Odessa resulted in a rise of the level of the sea by fully 5 feet over the average, followed by a sinking of the level by fully 7 feet, in accordance with the variations of atmospheric pressure.

Dr. Orzanski's extensive anthropological researches amidst the population of Russian prisons, and his numerous measurements, show no difference between the supposed "criminal's skull" and the average Russian skull. Numerous photographs were exhibited to illustrate this conclusion, so different from those arrived at by Dr. Lombroso.

Two new periodicals—one of them devoted to Russian natural science, and the other to meteorology—were founded while the Congress was at work. The meeting came to an end on January 20.

The Congress hoped to obtain from the Government permission to appoint a permanent Board, and thus to lay the foundation of a Russian Association for the Advancement of Science.

TECHNICAL EDUCATION IN ELEMENTARY SCHOOLS.

THE Committee of the National Association for the Promotion of Technical and Secondary Education have submitted to the Education Department the following suggestions for the modification of the Code as regards elementary technical education:—

A.—*Drawing.*

(1) Drawing to be introduced in infant schools, at least for boys.

(2) Drawing to be made compulsory in boys' schools.

(3) The Minute requiring cookery to be taught in girls' schools as a condition of receiving grant for drawing, to be repealed.

B.—*Object Lessons.*

(4) No school to be recognized as efficient which does not provide in the three lower standards a graduated scheme of object lessons in continuation of Kindergarten instruction in the infant school.

C.—*Science.*

(5) In order to encourage science as a class subject, the clause requiring English as one of the class subjects to be cancelled, and the teaching of science as a class subject to be further encouraged in the upper standards by an additional grant.

(6) Scholars of any public elementary school to be allowed to attend science classes held at any place approved by the inspector, and such attendance to count as school attendance.

(7) Examinations in science to be conducted orally, and not on paper, especially in the first five standards. If the inspection is satisfactory, an attendance grant of 4*s*. to be made for scientific specific subjects.

(8) Managers to be encouraged to submit alternative courses of instruction in specific subjects under Art. 16 (Code 1888). Such subjects to receive a grant on the same principle as the subjects enumerated in Art. 15.

[Art. 16. "Any other subject *other than those mentioned in Art.* 15, may, if sanctioned by the Department, be taken as a specific subject, provided that a graduated scheme of teaching it be submitted to and approved by the inspector."

But Art. 109 (*g*) which lays down the condition for grants, says, "The specific subjects which may be taken *are those enumerated in Art.* 15."]

(9) Grants to be made towards apparatus for science teaching and school museums.

D.—*Manual Instruction.*

(10) Manual instruction to be introduced in boys' schools, corresponding to needlework for girls.

(11) Instruction in the use of *simple tools* to be introduced in the higher standards as a specific subject, and grants to be paid thereon.

(12) Provision to be made for the introduction of *elementary modelling* in connection with the teaching of drawing, and a grant to be made in connection therewith.

(13) Instruction in *laundry work* to be encouraged in girls' schools, so far as practicable, as a part of domestic economy.

E.—*Evening Schools.*

(14) The clause providing that "No scholar may be presented for examination in the additional subjects alone" to be cancelled, to enable scholars to earn grants though not receiving instruction in the standard subjects.

(15) The number of "additional subjects" which may be taken to be increased from two to four.

F.—*Training Colleges.*

(16) Day Training Colleges and a third year of training to be recognized. The Universities and local University Colleges to be utilized for the training of teachers, where suitable arrangements can be made.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—The following appointments of Electors to Professorships have been made. Each Board consists of eight members, and it is provided by the Statutes that at least two members shall not be resident in the University or officially connected with it. In certain cases more than two such members have been voluntarily chosen by the Senate.

Arabic: Prof. Bensly; *Music:* Sir George Grove; *Chemistry:* Dr. E. Frankland, F.R.S.; *Plumian of Astronomy;* Mr. W. D. Niven; *Anatomy:* Dr. Huxley, F.R.S.; *Botany:* Prof. D. Oliver, F.R.S.; *Woodwardian of Geology:* Dr. A. Geikie, F.R.S.; *Jacksonian of Natural Philosophy:* Dr. Hugo Müller, F.R.S.; *Mineralogy:* Sir W. Warington Smyth, F.R.S.; *Political Economy:* Mr. R. H. Inglis Palgrave, F.R.S.; *Zoology and Comparative Anatomy:* Dr. Huxley, F.R.S.; *Sanskrit:* Prof. Aufrecht and Mr. R. A. Neil; *Cavendish of Physics:* Sir William Thomson, F.R.S.; *Mechanism:* Mr. W. Airy; *Downing of Law:* Mr. Justice Denman; *Downing of Medicine:* Dr. Richard Quain, F.R.S.; *Physiology:* Prof. Burdon Sanderson, F.R.S.; *Pathology:* Dr. J. F. Payne; *Surgery:* Sir James Paget, F.R.S; *Chinese:* Dr. Peile.

Prof. Robertson Smith being unable on account of the state of his health to lecture this term, Mr. A. A. Bevan, B.A., of Trinity College, has been appointed his deputy.

The Syndicate appointed to consider the probable expense of maintaining and working the great telescope offered to the University by Mr. Newall, report that a capital sum of £2225, and an annual expenditure of £400 will probably be required. They report further that the Sheepshanks Special Fund, founded in 1863 for the benefit of the observatory, will probably be able to furnish a capital sum of £1000, and an annual grant of £100, towards the expenses of the Newall telescope. The remainder, or £1225 at once, and £300 a year, will have to be provided from other sources; but whence is by no means apparent.

SCIENTIFIC SERIALS.

Revue d'Anthropologie, troisième série, tome iv., sixième fasc. (Paris, 1889).—Researches on the cephalic index of the Corsican population, by Dr. A. Fallot (of Marseilles). In an earlier number of this review, the author drew attention to the very appreciable alteration which the cephalic index had undergone in recent times among the inhabitants of Marseilles. Thus in one group of living subjects, born at the beginning of the century, he found that 21 per cent. exhibited an index of 84, while in another group, consisting of men of middle age, this number occurred only in the ratio of 7 per cent. This remarkable difference led the author to continue his determinations of the cephalic index among different communities. With this object in view, he last year visited Corsica, and in the present article we have the results of his craniometric determinations in this island, where from its peculiar geographical position and geognostic features, the inhabitants have preserved a permanence of type, and a homogeneity of ethnic characteristics, probably unequalled in any other European nation. Indeed so inconsiderable have been the changes effected in recent times in the Corsican population, that the observations made by Volney, in 1793, on the country and the people, apply almost equally well to their present condition. At the same time so little addition has been made since that period to our previously imperfect knowledge of Corsica, that Dr. Fallot's observations supply a valuable contribution to ethnological inquiry. All his determinations tend to demonstrate the great uniformity of cranial type and characters in the people. Thus while 54 per cent. of the population present a cephalic index varying from 75 to 78, not more than 13 per cent. gave an index above 80, while in only one out of 200 cases the index amounted to 86, and hence he assumes the mean index to be 76·5. He found that this uniformity was the greatest in the interior of the island, and more especially in the *département* of Corte; while at Bastia, in the extreme north, the cranial characteristics exhibited more variety, and afforded evidence of an admixture with foreign elements, a subbrachycephalic type supplanting the more general Corsican character of dolichocephalism. In the preponderance of this latter type Dr. Fallot thinks we have incontrovertible evidence against the opinion of Lauer, that the Corsicans are of Ligurian descent, and he believes that they may be more correctly characterized as an offshoot from the old Iberian races. The author gives numerous useful tables, and his brief summary of the history of the island is clear and instructive. From his observations on the geological conformation of the island we learn how numerous spurs, thrown off from the central high mountain range, have enclosed and isolated the several valleys, cutting off villages and settlements from their neighbours, and thus exerted so strong an influence upon the character and habits of the inhabitants, that the physical features of the island may be said to supply the key to its history. From the author's observations it may be assumed that in the mountain districts of the interior the genuine Corsican cranial type has been best preserved.—On infibulation, and other mutilations practised among the littoral tribes of the Red Sea, and the Gulf of Aden, by Dr. Jousseaume. The author describes at length the methods by which these processes are effected, and considers that whatever may have been their original motive they are in no way at present connected with religious observances, but are simply carried on from generation to generation as survivals of ancient barbarous customs.—On modern crania in Montpellier, by M. de Lapouge. In 1888 the author obtained 150 tolerably perfect skulls, which had been recovered from the soil of a cemetery at Montpellier used for interments from the seventeenth century until it was closed in 1830. An examination of the author's elaborate series of comparative craniometric measurements shows that the mean for the cephalic index of these skulls, viz. 78·3, is the lowest as yet observed in France, while their general cranial characters have less affinity with a French, than a North African type.—Prehistoric Scandinavia, by M. I. Undset. This is a sequel to a paper published in this review in 1887, the author now bringing his survey of the progress of northern palæontological science up to the present time.

THE *American Meteorological Journal* for December contains:—An article by W. M. Davis and C. E. Curry, on Ferrel's convectional theory of tornadoes; his theory, which is remarkably simple, is based on the occurrence of an ascensional movement in the tornado-whirl. The authors state that this fact seems too well established to admit of a doubt, although Faye and others in Europe, and Hazen in the United States, have questioned it. The paper contains graphical illustrations of the instability caused by convection.—Tornado chart of the State of Indiana, by Lieutenant J. P. Finley, compiled from statistics for seventy-one years ending 1888. The average yearly frequency is 4·5 storms. The month of greatest frequency is May.—Theory of storms, based on Redfield's laws, by H. Faye, continued from the November number, and dealing with the mechanics of whirls in flowing water, and with the upper currents of the atmosphere; the conclusion being that cyclones are whirls, originating in the upper regions of the air.—A continuation of the article on the meteorology at the Paris Exhibition, by A. L. Rotch, describing the meteorological instruments in the foreign sections.—The conclusion of Dr. F. Waldo's interesting discussion of wind velocities in the United States, with charts of "isanemonals" for January, July, and the year. The fact that the curves can be drawn with general symmetry shows that there is some uniformity in the exposure of the anemometers for *like* regions. The author points out that the effect of the Rocky Mountains seems to make itself felt on the winds to a distance of 200 or 300 miles to the eastward.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, December 19, 1889.—"Some Observations on the Amount of Luminous and Non-Luminous Radiation emitted by a Gas-Flame." By Sir John Conroy, Bart.

These experiments show—

(1) that 3 millimetres of glass and 10 centimetres of water transmit a small portion of the non-luminous radiation of an Argand gas-burner, but that, when the thickness of the water is increased to 15 centimetres, the transmitted radiation consists exclusively, or almost exclusively, of those kinds of radiation which affect the eye as light.

(2) That, with the form of apparatus employed (a thermopile and galvanometer), there is no measurable difference between the diathermancy of pure water and of a solution of alum.

(3) That the radiation from an Argand gas-burner consists of about 1·75 per cent. luminous and 98·25 per cent. non-luminous radiation.

January 30.—"On outlying Nerve-cells in the Mammalian Spinal Cord." By Ch. S. Sherrington, M.A., M.B., &c. Communicated by Prof. M. Foster, Sec. R.S.

Gaskell has shown that in the cord of the alligator scattered nerve-cells are to be seen at the periphery of the lateral column. Although nerve-cells appear to be absent from that position in the spinal cord of Mammalia as represented by the rabbit, cat, dog, calf, monkey, and man, yet there are in these animals isolated nerve-cells present in the white matter of the cord, not only in the deeper portions of the lateral column, but in the anterior and posterior columns as well.

In the anterior columns occasional nerve-cells, of the multipolar kind, lie among those fibre-bundles which pass between the deeper mesial border of the anterior horn and the anterior commissure at the base of the anterior fissure. They, in the instances observed, are smaller than the large cells characteristic of the anterior horn, and lie with two of the processes directed parallel with the horizontal transverse fibres among which they are placed.

In the lateral column, of the spinal cord of man and the other animals named above, it is common to find outlying members of the group of small cells of the lateral horn, Clarke's tractus intermedio-lateralis, situated in the white matter, distinctly beyond the limits of the grey. Some outlying cells here are placed at a great distance from the grey. They are generally placed upon, or at least in close connection with, the fine connective-tissue septa which pass across the white matter. It is probable that the cells are connected with the medullated nerve-fibres running along these septa.

In the part of the lateral column adjacent to the lateral reticular formation numerous nerve-cells are to be found among the interlacing bands of nerve-fibres. These are often fusiform, but in many cases multipolar; they are for the most part small, but occasional large individuals can be found; the latter would appear always to be multipolar. Where the lateral column comes into contact with the lateral limb of the substantia gelatinosa of the caput cornu posterioris ganglion-cells can frequently be seen in it. The larger axis of these cells is parallel to the outline of the caput cornu.

In the posterior columns outlying nerve-cells are also to be found, especially in the human cord. They are best seen in the upper lumbar and lower dorsal regions. They are large, measuring in some instances 70 μ across. In appearance they closely resemble the cells of Clarke's column. They are nearly always of broadly ovate shape. They appear always to lie on or in close relation to those horizontal bundles of nerve-fibres which curve in a ventro-lateral direction from the depth of the extero-posterior column into the grey matter in the neighbourhood of the posterior vesicular group. The longer axis of the cell is placed parallel to the nerve-fibres it lies upon or among. Where a process from the bipolar cell-body can be followed, it disappears in a direction which is that of the surrounding nerve-fibres.

With regard to the cells existing among fibres passing to the white commissure of the cord, it is legitimate to consider their presence as evidence in favour of the view that some of the cells of the median portion of the ventral grey horn are directly connected with medullated fibres passing to or from the opposite half of the cord by way of the anterior commissure.

The cells in the lateral column outside the lateral horn may be taken to point to the connection of the intermedio-lateral group of Clarke with the nerve-fibres which radiate in bundles from the grey matter of that region into the lateral column. Concerning some of the outlying cells in the more dorsal portion of the lateral column, the same inferences may be drawn; and some of them would seem to be connected with fibres of the posterior roots that curve round the lateral aspect of the caput cornu posterioris. Of the outlying cells in the posterior column, if they are outlying members of Clarke's group, the relations which they suggest for that group are—

i. That the group is connected *directly* with certain of the median fibres of the posterior spinal roots—namely, those which after an upward course in Burdach's column plunge into the grey matter of the base of the posterior horn.

ii. That some at least of the cells of that group are interpolated, more or less immediately, into the course of medullated nerve-fibres of large calibre.

The question naturally arises, May not these cells in the posterior column of the Mammalian cord represent the bipolar cells discovered by Freud, in the cord of *Petromyzon planeri*, to be in direct communication with fibres of the posterior roots? If so, may Clarke's column be considered a portion of the ganglion of the posterior spinal nerve-root which has been retained in the interior of the spinal cord in the thoracic and certain other regions?

Royal Meteorological Society, January 15.—Annual Meeting.—Dr. W. Marcet, F.R.S., President, in the chair.—The Council, in their Report, congratulated the Fellows on the generally prosperous state of the Society; the past year's work, though not in any respect exceptional, having been thoroughly successful. The total number of Fellows is 550, being an increase of 25 on the previous year; the finances are improving, and the library is overflowing.—Mr. Baldwin Latham was elected President for the ensuing year.—The retiring President, Dr. Marcet, then delivered an address on "Atmospheric Dust," which he divided into organic or combustible, and mineral or incombustible. The dust scattered everywhere in the atmosphere, and which is lighted up in a sunbeam, or a ray from an electric lamp, is of an organic nature. It is seen to consist of countless motes, rising, falling, or gyrating, although it is impossible to follow any of them with the eye for longer than the fraction of a second. It is difficult to say how much of the dust present in the air may become a source of disease, and how much is innocuous. Many of the motes belong to the class of micro-organisms which are frequently the means of spreading infectious diseases. Many trades, owing to their dusty nature, are very unhealthy. Dust, when mixed with air, is inflammable and liable to explode. After giving several instances of explosions due to fine dust in flour mills and coal mines, Dr. Marcet referred to inorganic or mineral dust, and gave an account of dust storms and dust pillars in India. He then proceeded to describe volcanic dust, which consists mainly of powdered vitrified substances, produced by the action of intense heat. The so-called ashes or scoriæ shot out in a volcanic eruption are mostly powdered pumice, but they also originate from stones and fragments of rocks, which striking against each other, are reduced into powder or dust. Volcanic dust has a whitish-gray colour, and is sometimes nearly quite white. Dr. Marcet concluded with an account of the great eruption of Krakatão in August 1883. The address was illustrated by a number of lantern slides.

EDINBURGH.

Royal Society, January 20.—Sir W. Thomson, President, in the chair.—Prof. Tait communicated an obituary notice of Dr. Andrew Graham, R.N., by Mr. John Romanes, W.S.—The President gave a paper on electrostatic stress. A complete dynamical illustration of electro-dynamic action may be had in an elastic solid, homogeneous in so far as rigidity is concerned, permeated with pores of unalterable size containing liquid. These pores may be in part in communication with each other, and in part closed by elastic partitions. These cases correspond to conductors and non-conductors respectively. Electrostatic stress depends on the curvature and extension of the partitions. The law of capacity in the model is identical with that in conductors.—Prof. C. Michie Smith described the great eruption at Bandaisan, Japan, photographs being shown.—Prof. Tait read a paper, by Prof. Heddle, on a curious set of fog-bows.—Dr. Berry Haycraft gave an account of some experiments which extend our knowledge of volitionary movement and explain the production of the muscle and heart sounds.

PARIS.

Academy of Sciences, February 3.—M. Hermite in the chair.—On the nuclei of the great Comet II. of 1882, by M. F. Tisserand. From the presence of five bright points disposed in a straight line, it is evident that the matter was not uniformly

distributed in the head of this comet. There exist several centres of condensation with apparent diameters of 1″ or 2″, their mutual distances changing from time to time, but their position remaining constant in the same straight line, which revolves progressively round the principal nucleus. These conditions are specially favourable for the development of secondary nuclei, which the author regards as so many minor comets submitted to the attraction of the sun alone, moving in very elongated elliptical orbits with a common perihelion and different long axes, disposed, however, according to the same straight line. Hence the comet contained within itself the germs of disrupture, its elements in this respect resembling those of the 1843 and 1880 comets.—On the roots of an algebraic equation, by Prof. A. Cayley. Resuming the theory of the roots of the equation $f(u) = 0$, instead of the surface $c - z = P^2 + Q^2$, the author now studies the surface $(c - z)^2 = P^2 + Q^2$, taking into consideration the positive values only of z that are not greater than c. He hopes to apply this theory to the case of a cubic equation, where the calculations, however, are much more difficult.—Determination of regulated harmonic surfaces, by M. L. Raffy. Very few surfaces are known whose linear element is reducible to the harmonic form (Liouville's form). To find others, the author employs two distinct processes. The first consists in taking the analytical form of the co-ordinates of the surface in function of two parameters, and determining the unknown functions, so that the linear element may be harmonic; the second, in seeking for harmonic surfaces amongst those which may be generated by taking their linear element alone.—Solar observations for the last six months of 1889, by M. Tacchini. Excluding the month of August, the observations here tabulated for the spots and faculæ show that the period of calm has continued to the end of the year, and the observations already made for January 1890 show that this period still continues. The same result is shown in the case of the protuberances, so that we appear to have entered the period of absolute minimum.—On the propagation of sound, by MM. Violle and Vautier. These experiments, made with a cylindrical tube, lead to the inference that, whatever be the nature of the initial impulse, the sound-wave tends towards a simple, determined form, and this form once acquired, the various parts of the wave are propagated with a uniform velocity which must be regarded as the normal velocity of the sound. The velocity in the open air is greater than in a tube, where the influence of the walls causes a retardation in inverse ratio to the diameter, and exceeding 0·46 m. in a tube with diameter of 1 meter. The normal velocity of sound in a dry atmosphere at zero is 331·10 m., with probable error less than 0·10 m.—On the state of the magnetic field in conductors of three dimensions, by M. P. Joubin. The results of these researches, which agree with experience, show that the magnetic field produced by a current exists in the medium traversed by the electric flux as well as in the exterior medium.—On the mechanical actions of variable currents, by M. J. Borgman. In reproducing, with the limited resources of a laboratory, the interesting experiments exhibited by Prof. E. Thomson at last year's Exhibition, the author has obtained some fresh results, which are here described.—Results of the actinometric observations made at Kiev in 1888–89, by M. R. Savelief. These observations lead to the general conclusion that 63·5 per cent. of the annual solar heat reaching the earth is absorbed by the terrestrial atmosphere, only 36·5 arriving on the surface of the ground; in October the proportion is 41, in January and February 28 per cent. The maximum received on a fine day in the beginning of July is 610, and in December 87 calories on a given space.—On the compounds of the metals of the alkalies with ammonia, by M. Joannis. In continuation of his previous communication (*Comptes rendus*, cix. p. 900) the author describes some further experiments, which are totally at variance with the theory advanced by M. Bakhuis Roozeboom (*Comptes rendus*, cx. p. 134) to explain the phenomena already observed by M. Joannis.—On the combinations of ammonia and phosphuretted hydrogen with dichloride and dibromide of silicon, by M. Besson. With ammonia a solid, white, amorphous substance, of the formula Si_2Br_4, $7NH_3$, is obtained, in all respects resembling the corresponding compound of the chloride. Phosphuretted hydrogen has no action on silicon dichloride at the ordinary temperature, but is absorbed at low temperatures. At $-60°$ C. the composition is approximately Si_2Cl_4, $2PH_3$.—On the part played by certain foreign substances in iron and steel, by M. F. Osmond. The author here gives results for boron, nickel, copper, silicium, arsenic, and tungsten, reserving for a future paper full treatment of the subject.—On lussatite, a new crystallized variety of silica, by M. Er. Mallard. To the substance here described as nearly pure silica, the author gives the name of lussatite, from the deposits of bitumen at Lussat, near Pont-du-Château, where its properties may best be studied.—On the oxides of manganese, by M. Alex. Gorgeu. In this paper, the author studies the psilomelanes and wads, reserving for a future note the manganites, properly so called: hausmannite, acerdese, and braunite.—Papers were read by M. Paul Marchal, on the structure of the excreting organ in the prawn; by M. P. A. Dangeard, on the junction of stem and root in the gymnosperms; by M. Stanislas Meunier, on a new method of artificially producing ferriferous platinum with magnetic poles; and by M. Alexis de Tillo, on the hypsometric chart of European Russia.—M. Gilbert was nominated Corresponding Member of the Section for Mechanics in place of the late M. Broch.

BERLIN.

Physiological Society, January 17.—Prof. du Bois-Reymond, President, in the chair.—Dr. Weyl gave an account of experiments which he had made in conjunction with Dr. Kitasato on the biology of anaërobic Bacteria. Koch had only imperfectly overcome the difficulty in the way of a pure culture of these Bacteria, viz. the exclusion of atmospheric oxygen, by covering the plates on which they were being grown with films of mica. Livonius was more successful by means of a deep layer of Agar-Agar, and by replacing the air by an atmosphere of hydrogen. The speaker had endeavoured to arrive at the same result by mixing the material on which the cultivation was carried on with some substance which has an affinity for oxygen, and obtained good results with dioxyphenols and aldehydes, but more particularly with formate of soda. The members of the first class of substances, of which a large number were tried, had for the most part to be abandoned, for they exerted a toxic action on the Bacteria when they were employed in quantities sufficient to insure the complete absorption of oxygen. Very fine pure cultures of the anaërobic Bacteria of "quarter-evil" (*Rauschbrand*), of tetanus, and of malignant œdema, were obtained on Agar-Agar by the use of eikonogen and of formate of soda, and were exhibited to the meeting. By means of these pure cultures it was possible to demonstrate that the anaërobic Bacteria exert a powerful reducing influence; this was shown on preparations in which the culture-material was deeply coloured with indigo-blue, the latter being then reduced by the organisms to indigo-white. These simple methods of cultivation facilitate greatly the further investigation of these Bacteria.—Prof. Liebreich spoke on the function of the bladder in fishes. During his investigations of the inert layer on the upper surface of fluids, he had allowed a float whose specific gravity was slightly less than that of the fluid to ascend through the fluid, and observed that it came to rest a short distance below the surface and remained there. During these experiments the slight changes of temperature which are unavoidable in large masses of fluid produced irregularities which led him to study the phenomena exhibited by a "Cartesian diver." These are not correctly described in either the older original works on the subject or in the more recent textbooks of physics. The equilibrium of the diver is unstable for any given pressure exerted upon the elastic membrane which covers the upper end of the vessel in which he is contained. This the speaker proved, not only by developing the formulæ which hold good for a system composed partly of solids and partly of air when immersed in a liquid, but also by means of a series of striking experiments. When the attention is directed to the experiment, it may readily be noticed that it is impossible to keep the diver in a condition of rest at any given level by exerting a *uniform* pressure with the finger on the elastic membrane, but that, in order to produce this result the pressure must be continuously varied. If the pressure is applied by a screw instead of the finger, the diver does not remain at rest. When the air is compressed until the specific gravity of the diver is slightly greater than that of the liquid, he sinks to the bottom and remains there, however great the air-pressure may be. If now he is drawn to the top of the liquid by means of a magnet attracting a small slip of iron attached to the diver, he similarly remains at rest at the surface. If, again, he is now drawn slightly down, he rises towards the surface again, when left to himself, until he reaches a level above which he no longer rises but now sinks to the bottom. This layer of fluid—such that when drawn above it he rises and when drawn down below it he sinks—may be called his "hydrosphere," or, in other words, it is a layer of liquid within the limits of which his specific gravity is unity. A fish possessed

of a swim-bladder is in exactly the same condition as the diver, for it also is in unstable equilibrium in the water. The fish can only remain at rest in the water by continually readjusting its "hydrosphere" by means of slight contractions of the bladder, and thus balancing itself in a position of rest. When the fish rises or sinks, or moves horizontally, the alterations of the swim-bladder and the changes in specific gravity which are the result of this, play an important part, inasmuch as they strike a continual balance between the forces tending to raise and depress the fish's body. The laws according to which the swim-bladder plays its part in a fish are in general the same as those which hold good for the Cartesian diver, and these laws are now considerably cleared up by the speaker's researches.

DIARY OF SOCIETIES.

LONDON.

THURSDAY, FEBRUARY 13.

ROYAL SOCIETY, at 4.30.—The Liquation of Gold and Platinum Alloys: E. Matthey.—On the Unit of Length of a Standard Scale by Sir George Shuckburgh: General Sir J. T. Walker, R.E., F.R.S.
MATHEMATICAL SOCIETY, at 8.—Concerning Semi-invariants: S. Roberts, F.R.S.—Ether-Squirts: Prof. K. Pearson.—On Class-Invariants: Prof. G. B. Mathews.
INSTITUTION OF ELECTRICAL ENGINEERS, at 8.—The Theory of Armature Reaction in Dynamos and Motors: Jas. Swinburne.
ROYAL INSTITUTION, at 3.—The Three Stages of Shakspeare's Art: Rev. Canon Ainger.

FRIDAY, FEBRUARY 14.

ROYAL ASTRONOMICAL SOCIETY, at 3.—Anniversary Meeting.
AMATEUR SCIENTIFIC SOCIETY, at 7.30.—Annual General Meeting.—Election of Council, &c.—The Old Red Sandstone of North-East Scotland: J. W. Evans.
ROYAL INSTITUTION, at 9.—Problems in the Physics of an Electric Lamp; Prof. J. A. Fleming.

SATURDAY, FEBRUARY 15.

ROYAL INSTITUTION, at 3.—Electricity and Magnetism: Right Hon. Lord Rayleigh, F.R.S.

SUNDAY, FEBRUARY 16.

SUNDAY LECTURE SOCIETY, at 4.—Norway; its Scenery and its People (with Oxyhydrogen Lantern Illustrations): H. L. Brækstad.

MONDAY, FEBRUARY 17.

SOCIETY OF ARTS, at 8.—Stereotyping: Thomas Bolas.
ARISTOTELIAN SOCIETY, at 8.—The Distinction between Society and the State: J. S. Mann.
VICTORIA INSTITUTE, at 8.—Iceland (concluding paper): Rev. Dr. Walker.

TUESDAY, FEBRUARY 18.

SOCIETY OF ARTS, at 8.—Ocean Penny Postage and Cheap Telegraph Communication between England and all Parts of the Empire and America: J. Henniker Heaton, M.P.
ZOOLOGICAL SOCIETY, at 8.30.—First Report on Additions to the Lizard Collection in the British Museum (Natural History): G. A. Boulenger.—On a Guinea-fowl from Zambesi, allied to Numida cristata: P. L. Sclater, F.R.S.—Notes on the Genus Cyon: Dr. Mivart, F.R.S.
ROYAL STATISTICAL SOCIETY, at 7.45.
INSTITUTION OF CIVIL ENGINEERS, at 8.—The Shanghai Water-Works: J. W. Hart.—The Tytam Water-Works, Hong-Kong: Jas. Orange.—The Construction of the Yokohama Water-Works: J. H. T. Turner.
ROYAL INSTITUTION, at 3.—The Post-Darwinian Period: Prof. G. J. Romanes, F.R.S.

WEDNESDAY, FEBRUARY 19.

SOCIETY OF ARTS, at 8.—The Organization of Secondary and Technical Education in London: Prof. Silvanus P. Thompson.
ROYAL METEOROLOGICAL SOCIETY, at 7.—Observations on the Motion of Dust, as illustrative of the Circulation of the Atmosphere, and of the Development of certain Cloud Forms: Hon. Ralph Abercromby.—Cloud Nomenclature (illustrated by Lantern Slides): Captain D. Wilson-Barker.—An Optical Feature of the Lightning Flash (illustrated by Lantern Slides: Eric S. Bruce.
UNIVERSITY COLLEGE CHEMICAL AND PHYSICAL SOCIETY, at 5.—The Chemical History of a Crystalline Schist: E. Greenly.

THURSDAY, FEBRUARY 20.

ROYAL SOCIETY, at 4.30.
LINNEAN SOCIETY, at 8.—On the Fruit and Seed of Juglandia; on the Shape of the Oak-leaf; and on the Leaves of Viburnum; Sir John Lubbock, Bart., P.C., M.P., F.R.S.
CHEMICAL SOCIETY, at 8.—The Behaviour of the most Stable Oxides at High Temperatures: G. H. Bailey and W. B. Hopkins.—The Influence of Different Oxides on the Decomposition of Potassium Chlorate: G. J. Fowler and J. Grant.
ZOOLOGICAL SOCIETY, at 4.
INSTITUTION OF ELECTRICAL ENGINEERS, at 8.
ROYAL INSTITUTION, at 3.—The Three Stages of Shakspeare's Art: Rev. Canon Ainger.

FRIDAY, FEBRUARY 21.

GEOLOGICAL SOCIETY, at 3.—Annual General Meeting.
PHYSICAL SOCIETY, at 5.—On a Carbon Deposit in a Blake Telephone Transmitter: F. B. Hawes.—The Geometrical Construction of Direct Reading Scales for Reflecting Instruments: A. P. Trotter.—A Parallel Motion Suitable for Recording-Instruments: A. P. Trotter.—On Bertrand's Refractometer: Prof. S. P. Thompson.
INSTITUTION OF CIVIL ENGINEERS, at 7.30.—Some Types of American Locomotives, and their Construction: C. N. Goodall.
ROYAL INSTITUTION, at 9.—Magnetic Phenomena: Shelford Bidwell, F.R.S.

SATURDAY, FEBRUARY 22.

ROYAL BOTANIC SOCIETY, at 3.45.
ROYAL INSTITUTION, at 3.—Electricity and Magnetism: Right Hon. Lord Rayleigh, F.R.S.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

A Dictionary of Applied Chemistry, vol. 1: Prof. T. E. Thorpe (Longmans).—Prodromus Faunæ Mediterraneæ, vol. 2, Part 1: J. V. Carus (Stuttgart, E. Koch).—Reports from the Laboratory of the Royal College of Physicians, Edinburgh, vol. 2 (Pentland).—Catalogue of the Fossil Reptilia and Amphibia in the British Museum (Natural History), Part 3: R. Lydekker (London).—Elements of Logic: E. E. C. Jones (Edinburgh, Clark).—A Catalogue of British Fossil Vertebrata: A. S. Woodward and C. D. Sherborn (Dulau).—The Elements of Astronomy: Prof. C. A. Young (Arnold).—American Spiders and their Spinning Work, vol. 1: Dr. H. C. McCook (Author, Philadelphia).—The Flowering Plant: J. R. A. Davis (Griffin).—The *Electrician* Electrical Trades' Directory and Handbook for 1890 (*Electrician* Office).—The Photographers' Diary and Desk Book, 1890 (*Camera* Office).—Untersuchungen über die Bewegungsverhältnisse in dem Dreifachen Sternsysteme Scorpii: B. Schorr (München, Straub).—A Modern University: Hy. Dyer (Perth, Cowan).—On a University Faculty of Engineering: Hy. Dyer (Glasgow, Munro).—Types of Metamorphosis in the Development of the Crustacea: I. C. Thompson (Liverpool).—Magnetism and Earth Structure: Dr. E. Naumann (Trübner).—Journal of the Chemical Society, February (Gurney and Jackson).—Brain, No. 48 (Macmillan).—Journal of the Institute of Actuaries, January (Layton).—Monograph of the British Cicadæ, Part 1: G. B. Buckton (Macmillan).—Quarterly Journal of the Geological Society, No. 181 (Longmans).—Bulletin of the U.S. Geological Survey, No. 54 (Washington).

CONTENTS.

THURSDAY, FEBRUARY 20, 1890.

THE PHYSICS AND CHEMISTRY OF THE "CHALLENGER" EXPEDITION.

Report on the Scientific Results of the Exploring Voyage of H.M.S. "*Challenger*," 1873–76. Physics and Chemistry, Vol. II. (Published by Order of Her Majesty's Government, 1889.)

THE second volume of the Report on the Physics and Chemistry of the *Challenger* Expedition has been published, and contains matter of very great interest.

The first paper is on the compressibility of water, by Prof. Tait. He has used Amagat's "manomètre à pistons libres."

"The principle on which the instrument works is the same as that of the Manomètre Desgoffes—a sort of inverse of that of the well-known Bramah Press. In the British instrument, pistons of very different sectional area are subjected to the same pressure (that of one mass of liquid), and the total thrust on each is, of course, proportional to its section. In the French instrument, the pistons are subjected to *equal* total thrusts, being exposed respectively to fluid pressures which are inversely proportional to their sections. The British instrument is employed for the purpose of overcoming great resistances by means of moderate forces; the French, for that of measuring great pressures in terms of small and easily measurable pressures."

By means of the instrument from his description of which the above is an extract (p. 21), Prof. Tait has determined the compressibilities of cistern water, sea water, and solutions of common salt up to pressures of 450 atmospheres, and for a range of temperature extending from 0° to 15° C. The results may be briefly summed up as follows.

The average compressibility of fresh water at 0° C. and at low pressures is 520×10^{-7} per atmosphere. The compressibility is a minimum at 60° C. Both the compressibility and the temperature at which the minimum occurs are lowered by pressure. The average compressibility for a pressure of 456·9 atmospheres is 478×10^{-7} per atmosphere, and the temperature of minimum compressibility is about 30° C. The average compressibility of sea water is about 0·92 of that of fresh water. The point of minimum compressibility is about 56° C. at atmospheric pressure.

At 0° C. the average compressibility of water per atmosphere may be expressed by the formula $0{\cdot}00186/(36 + p)$, where p is the pressure in tons per square inch. The compressibility of solutions of NaCl, containing s parts of salt to 100 of water, is given by the formula

$$0{\cdot}00186/(36 + p + s).$$

The depth of a sea about six miles deep is reduced by 620 feet by compression. If the ocean were incompressible, the level of the surface would be 116 feet higher than it is at present, and about two million square miles of land would be submerged. Finally, the maximum density-point of water is lowered by about 3° C. by an additional pressure of 150 atmospheres, and the temperature of maximum density coincides with the freezing-point at $-2°{\cdot}4$ C. under a pressure of 2·14 tons per square inch.

It will be seen from this brief recapitulation of his results that Prof. Tait has carried through a very difficult research with success, and has made substantial additions to our knowledge. It may therefore appear ungracious to criticize points which do not touch the essence of the investigation, but it is impossible to read the Report without feeling that, in some respects, it falls short of the standard of classical perfection which ought to be attained in papers published at the national expense to illustrate a great national research.

In the first place, the C.G.S. system is entirely ignored. As the compressibilities are measured *per atmosphere*, this is, so far, not of importance; but in the formulæ quoted above, which express the compressibility per atmosphere, terms occur in which the pressures are measured in tons per square inch. The units are thus mixed, and though the requisite data for conversion into atmospheres are supplied, there is no doubt that foreigners will have some difficulty in interpreting the results.

Again, though we cannot but admire the scrupulous honesty with which he tells the tale, some annoyance may justly be felt that a paper should go forth to the world in a publication intended to mark the highest level to which British science has attained, marred by the confession that the author—who deservedly holds a place in the very foremost ranks of British physicists—had never heard of Van der Waals' work on the continuity of the liquid and gaseous states till the end of the year 1888.

Van der Waals' investigation was published in Dutch in 1873. In spite of the disadvantage due to the language in which it was written, its importance was at once recognized. Clerk-Maxwell gave a long account of it in NATURE in 1874 (vol. x. p. 477). He returned to the subject in a lecture delivered before the Chemical Society on February 18, 1875, and reported in full in NATURE (vol. xi. p. 357). After indicating what he considered to be the weak points of Van der Waals' theory, he added that nevertheless "his attack on this difficult question is so able and so brave, that it cannot fail to give a notable impulse to molecular science. It has certainly directed the attention of more than one inquirer to the study of the Low-Dutch language in which it is written." Maxwell again referred to Van der Waals in his articles on "Atom" and "Capillary Action," published in the "Encyclopædia Britannica" in 1875 and 1876. So important was the theory considered, that, although it was then four years old, twelve pages were devoted to it in the first number of the "Beiblätter" to *Poggendorff's Annalen* (1877). O. E. Meyer discussed it in his "Kinetische Theorie der Gase" in the same year. It is described in modern German text-books, such as Rühlmann's "Handbuch der Mechanischen Wärmetheorie," and Winkelmann's edition of Graham-Otto's "Lehrbuch der Chemie," both published in 1885. It was translated in full into German by Dr. Roth in 1881, and an English translation by Prof. Threlfall, of the University of Sydney, is about to be published by the Physical Society of London.

In spite of all this, the author of the Report we are discussing informs us, in an addendum dated August 8, 1888, that only a few days before he had been told by a visitor to his laboratory "that one of Van der Waals' papers (he did not know which, but thought it was a recent one)

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contains an elaborate study of the molecular pressure in fluids"; and a few lines further down he refers to "Van der Waals' memoir 'On the Continuity of the Gaseous and Liquid States,' which I have just rapidly perused in a German translation."

In view of the fact that Prof. Tait published a book on "Heat" in 1884, these statements are so astonishing that his interview with the visitor from whom he heard of Van der Waals can only be described, in the words of Mr. Montague Tigg when he discovered that Martin Chuzzlewit was in the next box in the pawn-shop, as "one of the most tremendous meetings in Ancient or Modern History."

Other indications of a lack of acquaintance with what has been done by others are not wanting. Taking $p(v - a)$ = constant, as the equation to the isothermal of a gas, and assuming that it applies approximately to a liquid, the author concludes "that water [at 0° C.] can be compressed to somewhat less than three-fourths of its original bulk, but not further." He adds that "the whole of this speculation is of the roughest character," but makes no reference to the converging lines of evidence which indicate that liquids could be compressed to from 0·2 to 0·3 of their bulk at ordinary temperatures and pressures. The numbers which lead to this conclusion are frequently in good accord, whether they are deduced from direct observation on the specific inductive capacities or the refractive indices of the liquids themselves, or from those of their vapours, or from the molecular volumes of the elements of which they are composed. The latter, however, as calculated in the few cases he discussed from Van der Waals' theory, are larger, except in the case of hydrogen, than the corresponding numbers obtained from optical or electrical measurements. Van der Waals did not deal with water-vapour, but if we use the molecular volumes for H_2 and air obtained by means of O. Meyer's modification of his theory, and take the molecular volumes of air and O_2 as identical (an assumption which will certainly make the result too large), we obtain the following values:—

Volume of the Matter in the Unit Volume of Water under Standard Conditions.

Deduced from observations on the refractive index of liquid water (L. Lorentz)	0·2061.
Deduced from observations on the refractive index of water-vapour (L. Lorentz)	0·2068.
Deduced from the molecular volumes of H_2 and O_2 obtained from refractive index or specific inductive capacity	0·23.
Deduced from the molecular volumes of H_2 and air given by Van der Waals' theory	0·33.

Prof. Tait's value is 0·717. It is certainly unfortunate that a number so widely divergent from the results of a whole literature of optical, electrical, and thermal researches should be published in a *Challenger* Report without any reference to the discrepancy. It is still more unfortunate that in discussing the theory on which this result is based the opinion should be registered that "the quantity a [in the formula $p(v - a)$ = constant] obviously denotes the ultimate volume" (p. 48). This was published sixteen years after Van der Waals had given reasons for believing that a (or, as he calls it, b) is four times the ultimate volume, and twelve years after O. Meyer had argued that the multiplier ought to be increased to $4\sqrt{2}$. The best theories on the subject are no doubt tentative, their agreement with facts is imperfect, but it is established beyond the possibility of doubt that the constant in question need not have the meaning which is here said to be obvious.

Two papers in which the compressibilities of solutions of NaCl are discussed had appeared in *Wiedemann's Annalen* some little time before the conclusion of Prof. Tait's work. Röntgen and Schneider (*Wied. Ann.*, xxix. 165, 1886) determined the relative compressibilities of water and of a number of different salt-solutions, and Schumann (*Wied. Ann.*, xxxi. 14, May 1887) gave absolute measures. Both researches were carried on at low pressures only, but they are interesting in their relation to Prof. Tait's conclusions, inasmuch as his compressibilities at low pressures are obtained (as he fully explains) by an extrapolation, and it is therefore desirable to compare them with the values given by direct observation.

In the following table the compressibilities obtained by Schumann for solutions containing given percentages of NaCl (*i.e.* parts of salt to 100 of solution) are compared with the values deduced from Prof. Tait's formula:—

Percentage.	Compressibility per atmosphere $\times 10^6$.	
	Schumann.	Tait.
0	50·3	52·0
5	45·5	45·1
10	39·7	39·5
15	34·8	34·6
20	30·6	30·5
25	25·8	26·8

It is to be observed that the number 50·3 is assumed by Schumann from Grassi, and that it was employed in experiments made with water, for determining the effect of pressure on the internal volume of the piezometers. If it had been replaced by Prof. Tait's value, the close agreement between the results for mean percentages would be destroyed. Schumann also obtains maxima of compressibility for low percentages of certain salts, though he seems very doubtful about the validity of these results. We have no intention of entering into a detailed discussion of his work which certainly appears to require confirmation, but there is no doubt that nobody could have made a critical comparison between his own experiments and those of Schumann so well as Prof. Tait, when he had the whole subject at his fingers' ends. It is thus a real loss to science when a man of his great ability ignores an investigation published nearly a year before the date of his own paper.

The form of the formula given by Prof. Tait for the compressibility of salt-solutions is closely analogous to that deduced from theory by Prof. J. J. Thomson in his "Applications of Dynamics to Physics and Chemistry" (p. 184). He shows that if k' is the compressibility of water, and P is the internal pressure due to the solution of a salt, the compressibility of the solution is $k'/(1 + Pk')$. If then we put $k' = 0·00186/(36 + p)$, Prof. Tait's formula for a salt-solution becomes $k'/\left\{1 + k'\frac{s}{0·00186}\right\}$, which, since P is proportional to very similar to J. J. Thomson's

expression, and would be identical with it if $P = s/0{\cdot}00186$ atmospheres. In that case the internal pressure due to the salt in a solution containing 20 parts of salt to 100 of water would be about the same as the internal pressure in pure water as given by Van der Waals. If, however, we attempt to apply van 't Hoff's theory of the pressure due to dissolved substances, we find, as in the examples quoted in the "Applications" (*loc. cit.*), that the observed values of Pk' are many times greater than those given by calculation.

The second Report, by Mr. Buchan, on "Atmospheric Circulation," of which we shall give some account in a future number, is rather a treatise on meteorology than a simple discussion of the *Challenger* observations. All the data, other than those derived from the expedition (which have been previously published), are set forth, and a vast collection of meteorological facts from all parts of the world is utilized.

It would be impossible to attempt to discuss Mr. Buchan's conclusions in detail, but one may be selected as an example. Twenty-six thunderstorms occurred at sea during the voyage, and of these only four took place between 8 a.m. and 10 p.m. Nineteen occurred when the ship was near the land, and these were pretty evenly distributed throughout the twenty-four hours. Over land thunderstorms are most frequent during the day. At sea thunderstorms are nocturnal, and occur chiefly during the morning minimum of pressure.

"Over the land the maximum of thunderstorms occurs during the hours of the day when temperature is the highest, but over the open sea during those hours when temperature is lowest. The great majority of thunderstorms over the land thus occur during the part of the day when the ascensional movement of the air from the heated surface of the ground takes place" (p. 32).

These facts furnish Mr. Buchan with an interesting suggestion as to the cause of these differences:—

"As regards thunderstorms over the land surfaces of the globe, the disturbance of atmospheric equilibrium, resulting in ascending and descending currents, is brought about mainly by the superheating of the surface and thence of the lowermost strata of the air. But as regards the open sea, this mode of disturbing the atmospheric equilibrium cannot take place, inasmuch as the influence of solar radiation is only to raise the temperature of the surface of the sea not more than a degree. Hence it is probable that the disturbance of the equilibrium of the atmosphere, in the case of thunderstorms over the open sea, is brought about by the cooling of the higher strata of the atmosphere by terrestrial radiation" (p. 34).

There can be little doubt that Mr. Murray is right in thinking that Mr. Buchan's Report will be a standard work of reference for many years to come.

The third Report, by Commander Creak, is on the Magnetical Results of the voyage. As the author has himself described the main results of his investigations in the pages of NATURE, it is unnecessary to do more than refer to its most salient features. We have two, and only two criticisms to make. Commander Creak has employed the British unit of force, and his paper will therefore be used with less comfort and ease by most magneticians than if he had employed the C.G.S. system. Perhaps, however, as an Admiralty official he felt bound to adhere to the traditions of his office. Again, we think that he has been rather too modest in the amount of space he has claimed. Like Mr. Buchan, he has used information from many sources which are not, or at all events are not stated to be, generally accessible. These he has employed in determining the rates of secular change during the last 40 years all over the globe. It would have been interesting if means could have been devised for showing not merely the results of this investigation but the data on which they are based. Again, the map in which the direction of motion—eastward or westward—of the north pole of the needle is graphically shown for the period considered would have been more valuable if the magnitudes of the mean annual motion at different places had been added. This has, in fact, been done in a recent German work on the same subject.

But if we are inclined to wish that Commander Creak had claimed a larger share of space and given more details, in what he has done he has gone beyond any previous writer. His work is of the highest importance as introducing a novel view of the causes of secular magnetic change, and in connecting it with certain definite localities.

Mr. Buchan has furnished us with new meteorological maps. Commander Creak has prepared new magnetic maps, which enable us to institute a comparison between the magnetic state of the globe in 1880 and its condition when Sabine portrayed it for an epoch some 40 years earlier. The positions of the magnetic poles and foci of maximum intensity do not appear to have altered. The secular change is associated, not with these, but with four points, towards two of which the north pole of the needle is veering, and from two of which it is apparently being repelled. The points of increasing attraction on the north-seeking pole are to the south of Cape Horn and in the south of China; the foci of diminishing attraction are in the Gulf of Guinea and near the north magnetic pole in Canada. The existence of this last focus is more or less hypothetical, but in the case of the other three the various magnetic elements concur in indicating the same neighbourhood as the centre of change. Thus not only is the secular variation of the declination of opposite signs to the east and west of these points, but the increase of the downward attraction on the north pole of the needle is a maximum near Cape Horn and in China, and a minimum (*i.e.* a maximum decrease) in the Bight of Benin.

Again the annual change of horizontal force is very small near Cape Horn, but it is decreasing in South America, and the rate of decrease is a maximum at a point between Valparaiso and Monte Video. These are precisely the kind of results which would follow from the gradual production of a subsidiary centre of relative attraction on the north-seeking pole of the magnet near Cape Horn. The real existence of the Gulf of Guinea centre is similarly confirmed. Commander Creak cautiously abstains from theorizing on these remarkable facts, but there can be no doubt that he is right in thinking that they must lead us to look for the chief causes of secular variation within the globe rather than in solar or extra-terrestrial influences. His paper will be a point of new departure in the science of terrestrial magnetism.

It will be seen from what has been said that the three Reports which have been discussed are written with a wider scope than the mere discussion of the observations

made during the voyage of the *Challenger*. Prof. Tait's paper has indeed little connection with the work of the Expedition. Mr. Buchan and Commander Creak have worked up an immense amount of matter derived from other sources.

The records of the *Challenger* have not only added facts of great importance to our stock of knowledge; but have been, as it were, nuclei round which a host of other observations have crystallized into orderly arrangement. Each one of the authors has made a step forward. Prof. Tait has extended the range of pressure over which compressibilities have been measured. Mr. Buchan has attacked the diurnal climatology of the ocean. Commander Creak has given a new turn to our ideas on the secular change of terrestrial magnetism. It is only to be regretted that the exclusive use of British systems of measurement, and the other blemishes to which we have felt compelled to refer, give a certain insular appearance and character to a work of world-wide interest.

The Report on the Rock-Specimens collected on Oceanic Islands, by Prof. A. Renard, consists of 180 pages, well illustrated by woodcuts and seven maps, and constitutes a very important part of the petrology of the *Challenger* Expedition. The account of the rocks of St. Paul's from the pen of Prof. Renard has already appeared in Vol. II. (Narrative), Appendix B, of the *Challenger* Reports, and we are glad to learn from the preface to the volume now before us that the "Report on Deep-Sea Deposits" which has been so long looked for by geologists, is to be issued next month.

Mr. Murray is to be congratulated on having secured the services of so able a mineralogist and petrographer as Prof. Renard to describe the rocks brought home by the Expedition. Most of these descriptions have already appeared in the *Bulletin of the Musée Royal d'Histoire Naturelle de Belgique*; but English geologists will be glad to see them collected together and published in their own language, and in a convenient form for reference.

Prof. Renard explains in his opening remarks the grounds for publishing this account of the rock-specimens collected on the oceanic islands by the officers of the *Challenger* Expedition:—

"Mr. Murray had discovered that loose volcanic materials played a very large part in the formation of the deposits of the deep sea, and it was considered desirable to institute a comparison between these and the products of the same origin in volcanic islands situated in or on the borders of the great ocean basins."

It is at the same time admitted, by the editor of the volume, that Prof. Renard's lithological and mineralogical descriptions must be regarded rather as contributions to the geology of the islands visited, than as supplying full and descriptive discussions of the subject.

"The necessities of the voyage, bad weather, or the difficulties of the exploration, prevented, in many cases, the naturalists from passing more than an hour or two on shore; they were thus unable to give any detailed account of the stratigraphical relations, and the collections of hand-specimens were sometimes limited to those rocks situated near the coast."

In the case of Tenerife, of which we have such full descriptions in the writings of Von Fritsch and Reiss, and of Sauer; in that of the Cape de Verde Islands, the rocks of which have been carefully studied by Dölter; and of Fernando Noronha, which has been surveyed and its rocks admirably described by Profs. Branner and Williams, it is obvious that the description of the specimens placed in the hands of Prof. Renard can only be regarded as supplementary to the fuller and more comprehensive accounts of the geology of the islands which we already possess. But in the case of some of the smaller islands, like Tristan da Cunha, Marion Island, and Heard Island, the notes in the present Report constitute almost the only materials which exist for judging of their geological constitution and structure. In the case of the Island of St. Thomas, in the West Indies; of Kandavu, in Fiji; of the volcano of Goonong Api, in the Banda Islands; of the volcano of Ternate, and of several islands in the Philippine Group, Prof. Renard has taken the opportunity afforded to him by the receipt of interesting specimens casually collected, to discuss points of considerable mineralogical and geological interest.

Quite apart from their connection with certain localities, these very careful notes of Prof. Renard on peculiarities exhibited by rock-forming minerals are of much value to geologists; and so also are the series of analyses of these rock-specimens, made, evidently with great care, by Dr. Klement.

So many of the islands visited by the *Challenger* were previously touched at by the *Beagle*, on board of which Charles Darwin was acting as naturalist, that it is impossible to avoid comparing the work before us with that author's classical memoir, "Geological Observations on the Volcanic Islands," which was published in 1844 and re-issued in 1876. In spite of the improvements of our petrographical methods during the half-century, which has witnessed the application of the microscope to the study of rocks, it is very interesting to see how often observations made by Darwin, aided by that great pioneer in crystallographic research, Prof. W. H. Miller of Cambridge, are confirmed by the painstaking labours of Prof. Renard. There is, perhaps, some danger, at the present day, that the facilities afforded for the microscopic study of rocks, by the aid of transparent sections, should lead geologists and mineralogists to despise, or to regard as of small value, the observations made without such aid. To those who entertain such an idea, it will be instructive to see how Darwin and Miller by the aid of pocket-lens, knife-blade, and magnet, were often able to form an appreciation of the mineralogical constitution of rocks, which has been very largely confirmed by the application of the more refined methods of the present day.

The discussion of great geological problems, which, as treated by Darwin in 1844, contributed so largely to the interest excited by his book, have of course not come within the scope of the work undertaken by Prof. Renard. The particular varieties of volcanic rocks in Ascension, which Darwin found to illustrate in so striking a manner the origin of foliation in the crystalline schists, do not seem to have been among those collected by the officers of the *Challenger*. But as an important contribution to micropetrography, the work of Prof. Renard is of the highest value, as might indeed have been anticipated from the well-proved skill and acumen of the author in this interesting branch of scientific research.

THE HUMAN FOOT.

The Human Foot: its Form and Structure, Functions and Clothing. By Thos. S. Ellis. (London: J. and A. Churchill, 1889.)

THIS book is an endeavour on the part of a practical surgeon to explain the mechanical construction of the human foot, and from this basis to show the principles on which boots and shoes ought to be constructed. Although written in a popular form, and intended for the instruction of the public, it is treated in a scientific spirit by one who is competent, on the ground of anatomical knowledge, to discuss the subject. Mr. Ellis was led to give special attention to the mechanism of the foot owing to one of his feet having been accidentally injured; and his recovery from lameness was due to the independent study which he was obliged to give to the structure of the foot in relation to its functions.

The earlier pages of the book are occupied by a short but clearly-written description of the form of the foot, and of so much of its anatomy as is needed to explain its mechanism. In the course of this description the author points out that the two feet are to be considered together, not as if they were two independent pedestals, or plinths, supporting the lower limbs and body, but as the two halves of one pedestal or plinth, the divisions of which are separated from each other. He recognizes the inner margin of the foot in its front or expanded part as forming a straight line, whilst the outer margin forms a bold curve, and acts as a sort of buttress to the main structure of the foot. The inner margin also is elevated to form the arch of the instep. He refers to Prof. Meyer's well-known line continued backwards from the mid-line of the great toe through a central point of the heel which follows the line of the long flexor of the great toe, and states that this line corresponds with the highest part of the ridge on the dorsum or upper surface of the foot, which indicates the course of the long extensor of the great toe.

The importance of the great toe in the construction of the foot is dwelt upon by Mr. Ellis. He shows that, when the foot is used as the basis from which the body is to be propelled forwards in the act of progression, the great toe leaves its fellows and passes towards the mesial plane between the two feet, but that it is not bent in so doing. On the other hand, the smaller toes, whilst being pressed against the ground, become bent, and the phalangeal joints are lifted upwards.

The relative length of the great and second toes is also discussed. As is well known, in many of the statues of ancient art the second toe is modelled somewhat longer than the great toe, but as a rule in nature itself the great toe is the longer. Exceptions, however, occasionally occur. The writer of this notice has now before him the casts of two well-formed feet, from a man and a woman, in both of which the second toe projects beyond the great toe. He has also in his possession casts of the feet of several of the aborigines of Australia, taken under the superintendence of Prof. Anderson Stuart, of the University of Sydney, in which interesting variations in the relative length of these toes may be seen. In a man and one woman the great toe is longer than the second; in another woman the second toe in the right foot is longer than the first, but in the left foot the opposite is the case. In an Australian boy, aged 4, in the right foot the great toe is slightly the longer, but in the left foot the second toe has the advantage. In none of these Australians had the feet ever worn shoes, so that the variation in the length of these toes is natural, and not produced by artificial means. It would appear, therefore—as was shown several years ago by Prof. Ecker, of Freiburg, and by a writer in NATURE, to be the case in the hand with the ring and index finger—that variations in relative length may occur, not only in different individuals, but in opposite limbs in the same person.

The author then discusses the movements at the joints of the foot and the action of the muscles; more especially when the heel is raised and the foot rests on tip-toe as in the movements of progression. He regards the long flexor of the hallux as exercising a bow-string or tie-rod influence, bracing up the arch and diminishing the distance between the heel and the great toe. Hence the exercise of dancing is one of the most important means of promoting and maintaining the strength of the foot. As regards the act of walking, Mr. Ellis contends that what he calls the "four-square position," in which the inner borders of the great toes are retained almost parallel to each other, is that which is most conducive to steady and continuous progression, for the joints and muscles of the foot obtain through it momentary rest in the intervals between the steps. He condemns the military position, with the toes turned outwards, both in standing and walking, as much more fatiguing, by keeping the muscles and joints in a constant strain. The condition of "flat-foot" ought never to arise if the tie-rod action of the long flexor muscles of the toes be sufficiently exercised by frequent springing of the foot to tip-toe, such as takes place in the act of dancing.

The author applies the anatomical principles which he has expounded to the construction of stockings and shoes. He holds that quite as much mischief is done to the feet by wearing ill-made socks as badly-shaped shoes. He considers that a stocking with a separate stall for the great toe is always desirable, but that a straight inside line is imperative. To obtain a properly fitting boot it is necessary, in addition to the measures of length and girth, to have the contour lines of the foot, and to obtain these the author has devised a foot-stand or pedistat, a description and figure of which are given in the book. From these measures a last can be made which conforms to the shape of the foot throughout as it stands on a level surface.

We recommend the perusal of this book to all who are interested in the mechanism of the foot, and in obtaining for it well-fitting socks and shoes; and we do so with the more confidence as the author had obviously passed through a painful experience before he had satisfied himself of the principles which ought to be attended to in the construction of its clothing.

OUR BOOK SHELF.

Das australische Florenelement in Europa. Von Dr. Constantin Freiherr von Ettingshausen. Pp. 10, Tab. I. (Graz: Leuschner and Lubensky, 1890.)

THIS is a defence of the identification of fossil plants from the Tertiary beds of Europe, chiefly from Austria and

Hungary, with existing Australian genera. Baron Ettingshausen himself is largely responsible for these identifications, which have been questioned "by certain critics insufficiently acquainted with the subject." He claims that he was supported in his views by such eminent palæontologists as Franz Unger and Oswald Heer. It is now some years since Unger published his sensational "Neuholland in Europa." In this little work almost every one of a set of Eocene fossil plants is identified with some essentially Australian genus, and often, we should add, on the very slenderest of material. The late Mr. G. Bentham, who, as is well known, handled and described every Australian plant of which specimens had been collected up to his time, disputed the correctness of the identifications, and endeavoured to prove that the remains might well be those of genera still found in the northern hemisphere; yet Baron Ettingshausen gives us to understand that Mr. Bentham confirmed his determination of a European fossil leaf as belonging to the genus *Dryandra*.

Quite recently the Marquis de Saporta has attacked Baron Ettingshausen's position, and the present pamphlet may be regarded as a reply. The author concludes with the statement that, to prevent misunderstanding, he wishes it to be known that any objections or criticisms will meet with no response from him, because he is convinced of the accuracy of his "facts," and his time is too valuable to enter upon superfluous discussion. Without discussing his "facts" one by one, and without actually denying their accuracy, we may say that the illustrations given are by no means convincing, as most botanists who have worked many years in herbaria on plants from all parts of the world, we believe, will agree. Few persons probably have paid so much attention to the venation and forms of leaves as Baron Ettingshausen, yet we find none of his determinations absolutely beyond doubt. So far as we are aware, not a single fruit of *Eucalyptus* or of the assumed *Proteaceæ* has been discovered in the European Tertiary formations. As to his leaves of *Eucalyptus*, they might be matched in the genus *Eugenia*, and we see no reason why any of the others are necessarily remains of species of Australian genera. W. B. H.

Is the Copernican System of Astronomy True? By W. S. Cassedy. (Standard Publishing Co., Kittanning, Pa., 1888.)

AN astronomer nowadays would find it a hard task to bring forth any facts which would throw doubt upon the truth of the Copernican theory, but it appears that there are still people amongst us who are bold enough to attack the strongholds of astronomy. Such attempts are always hopeless failures, and the one under notice is no exception. It is, indeed, doubtful whether the author knows what is meant by the Copernican system, for he goes so far as to suggest that the known diameter of the earth's orbit (assuming that it exists) should be used as a base-line for determining the distance of the sun! He also states that he has "found by experiment" that similar right-angled triangles have sides proportionate in length, though it is only fair to say that he is aware of the existence of the first book of Euclid, if not of the sixth.

We have already said enough to show that the book need not be considered seriously; but we cannot refrain from stating that the author, by sighting the sun along straight-edges at the equinoxes, has found that "the distance of the sun from the surface of the earth, at 40° N., is one million miles (p. 49)." This result is about as near the mark as could be expected from the method employed.

Naturalistic Photography. By P. H. Emerson, B.A., M.B. (London: Sampson Low, Marston, Searle, and Rivington, 1890.)

THE quick call for a second edition of this work indicates the approval with which it has been received, and we may safely say there is not a better or more instructive book on the art principles of photography than the one before us. Dr. Emerson is a photographer of the first rank, his artistic compositions are everywhere admired, and the energetic manner with which many of the old and cherished ideas of the ordinary photographer are attacked and others established makes it very manifest that he only writes what he knows to be true. The literary style of the book is excellent, and the exposition has the merit of being strikingly original; it should, therefore, be studied by every photographer, both amateur and professional, who desires to excel in his art.

LETTERS TO THE EDITOR.

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Acquired Characters and Congenital Variation.

BEYOND this letter I cannot pursue my interpolated adversary, Mr. Dyer.

The syllogisms which he attributes to me are entirely his own. I willingly admit, therefore, that they are as ingeniously bad as they can well be.

I will now state shortly what my position was, and is:—

(1) The assumed antithesis between "acquired characters" and "congenital variation" has arisen out of the cult of Darwin as opposed to Lamarck.

(2) The theory of Lamarck fails, in my opinion, as much as the theory of Darwin, to give any adequate or satisfying explanation either of the genesis, or of the development, of organic forms.

(3) But the theory of Lamarck is more philosophical than the theory of Darwin, in so far as it seeks for, and specifies, a definite natural cause for the phenomena of variation.

(4) The theory of Darwin is essentially unphilosophical in so far as it ascribes these phenomena to pure accident, or fortuity.

(5) That Darwin himself, at one time, if not always, admitted this idea of fortuity to be a mere provisional resort under the difficulties of ignorance.

(6) That the later worshippers of Darwin depart, in this respect, from their master, and making the weakest part of his system the special object of their worship, have set up Fortuity as their idol.

(7) That it is under the influence of this superstition that they now seek to deny altogether that acquired characters can become congenital.

(8) That this denial is against the most familiar experience of Nature, and especially of artificial selection, which is the antetype and foundation of the whole theory of evolution.

(9) That in all domestic animals, and especially in dogs, we have constant proof that many acquired characters may become congenital.

(10) That it is no answer to this argument to demand proof that the babies of a blacksmith are ever born with the abnormal arm-muscle of their papa.

(11) That in order to avoid and evade the force of innumerable facts proving that many acquired characters may, and do, become hereditary, fortuitists have invented a new verbal definition of what they mean by "acquired."

(12) That this definition is full of ambiguities and assumptions, concealed under plausible words, but the object of which is to limit the meaning of "acquired characters" to gross, visible, palpable changes affecting single individuals, and which the analogies of Nature do not lead us to expect or to suppose can be repeated in a single generation, even if a tendency to their development is really implanted in the race.

(13) That, still farther to render impossible the proof they demand, our fortuitists affix to their definition of the word "acquired," conditions which beg the whole question in dispute. Not only must the new characters be gross, palpable, visible—cases of "hypertrophy," of "extension," or of "thickening,"—but also they must be "obviously due to the direct physical action of the environment on the body of the individual." This is a condition which is irrational. It excludes

all those fine, invisible "molecular" changes, through which Nature habitually works, and it ascribes to mere outward and mechanical agencies, effects which, alone, we have no reason to suppose they ever can produce.

On the question of "prophetic germs," Mr. Dyer challenged me to produce a single case of organs useless now, but in course of preparation for future use. I replied by referring him to this phenomenon as universal throughout Nature in the life-history of every individual organism; and I also referred him to the well-known idea of Darwinian embryology which establishes a close analogy between the laws governing the development of the embryo, and the whole past development of organic life.

Mr. Dyer replies that I ought to have explained this sooner—when challenged to do so by Prof. Ray Lankester—an observation which has nothing to do with the merits of the question. The truth is, I wished to close my dispute with that distinguished Professor, as I now desire to close it with Mr. Dyer, and I was satisfied with an indirect admission that, as regards every individual organism, my assertion could not be contradicted. What this involves, I left, and now leave again, as unexhausted as it is indeed inexhaustible.

In conclusion, I must observe upon the use Mr. Dyer makes of the phrase "*a priori* argument," which he apparently uses not only for all deductive argument, but for all analytical reasoning. When he says he "has not an *a priori* mind," he really means that he is indisposed to all analysis. This is a very common attitude even with many able and distinguished men—especially when they are devoted to a system, and are the disciples of some prophet, whose words and phrases they gulp and swallow whole. It is an attitude which has its use; but it is not one to boast of. Mr. Dyer's declaration that "the questions at issue with regard to evolution are now, I believe, thoroughly understood by biologists" is the most astonishing utterance I have ever heard or read coming from a scientific man. Discussion with him is useless. He and his friends know all about it. How life began, and how it grew from more to more—the whole secret of creation—"an open scroll, before them lies." I am happy to think that I am not the only searcher—by many thousands—whose pens Mr. Dyer must intervene to stop. There is a great army of us who are conscious above all things of the ignorance of man. ARGYLL.

Kinellan, Murrayfield, N.B.

IN the number for January 16 (p. 247) Mr. Thiselton Dyer observes that "there are many readers of NATURE who, while taking a general interest in the problems raised by Darwinism, have not followed all that has been written about it." For the benefit of such persons he gives an interesting explanation of Darwin's views on several important points.

I have not read *all* that has been written, but all, I think, that has ever appeared in the pages of NATURE, and with the result that I am more and more convinced of the inadequacy of the Darwinian theory to account for the origin of species. Natural selection is a *vera causa*, but of very limited operation. The theory of sexual selection but partly removes one serious difficulty not of the first magnitude.

I find Darwinians—not Darwin—very ready to insinuate or assert that an unwillingness to adopt their views, on the part of persons who believe in a supernatural revelation, arises from theological prejudice, which hinders them from listening to the voice of reason. I think there is some prejudice on both sides. For myself, fully believing in a Supreme Designer, I am perfectly and most fearlessly willing that "the attempt at mechanical explanation" should be carried as far as possible, well knowing that "a final universal cause" cannot possibly be disproved or reasonably denied. And Darwinism is committed to no such denial.

We have our choice between two alternatives. Life on our globe had a beginning; and its cause was certainly not mechanical or natural,—for reasons not theological, but strictly scientific, in the technical sense of the word. For, as the laws of Nature operate uniformly, if life had ever commenced spontaneously, it must of natural necessity do so again and again, since it would be most absurd to suppose that only during some previous state of the earth's surface did matter exist in such a condition as to be capable of conversion into living things. If life had ever arisen mechanically, it would require a miracle to *prevent* repetitions of the process.

We have, then, to take our choice between supposing with Darwinians that the life-producing power acted once for all, and supposing that it has acted repeatedly and continuously, in more ways than one. I see no theological, and, let me say, no Scriptural, objection to either. Let it be believed willingly, if good reasons can be given, that all life began with a single germ which could not only produce its like—which is wonderful enough—but which even contained in itself such amazing potentialities that it could become, and has become, the parent of every form of life, sentient or non-sentient, that has ever appeared on our globe.

To me this seems scientifically improbable. For why should the power, whether acting intelligently, or, if anyone prefers it, without intelligence, create one germ only? Why not millions? And if of one kind, why not of many? And if single organisms, why not organisms connected with one another, even in highly complex structures? And why act once only? Why not start non-sentient life at one time, sentient at another? For do not sentient things need a separate germ? I take leave to think so. But be this as it may, they are as much in advance of the non-sentient, however much alike those germs we know of may appear to be, as the non-sentient are of inanimate matter.

The other alternative supposition is that the life-producing power, instead of acting once only, and then subsiding into its primæval torpor, continues to act. That, as it once acted upon inanimate matter, not robbing it of anything, but rather, while availing itself of its properties, conferring upon it new powers, so it has acted since upon living things, ever producing out of the old new and higher forms of life; availing itself of all existing faculties of living things, but while allowing them to achieve all that they can, still moulding fresh forms, and conferring higher faculties. To suppose this, is only to suppose that the action of the life-producing power, since life began, has been analogous to what we know was its action in producing life. It is hardly to be supposed that the production of one marvellous germ has exhausted all its energy.

Yet, if the Darwinian theory can enable us to dispense with the aid of this power, let it do so. Let reason prevail.

Darwinians offer, as an adequate explanation of the formation of new species from the older, that this development comes about simply through natural selection—through the survival of the fittest of favourable variations.

"The origin of any species," says Mr. Thiselton Dyer, "lies firstly in the occurrence, and secondly in the selection and preservation, of a particular variation." But surely a particular variation alone—that is, such as can be brought about, as we know from experience, in a single generation—does not sufficiently differentiate one species from another. Short-horned cattle, for instance, are not a new species, nor would they deserve to be so termed if it should eventually happen that all other varieties of horned cattle became extinct. In the great majority of cases, at all events, there must be *more than one* particular variation, before we can recognize a specific difference. Species have become what they are by the *combination*, in one organism, of many particular variations, each well suited to the rest. No particular variation could make of another ruminant a giraffe. What we want, and what seems to be wanting in the Darwinian theory, is a satisfactory hypothesis to explain the *concurrence* of many particular variations, by the co-existence of which in one structure the new species is constituted. Variations, or "fluctuations," as Mr. Thiselton Dyer has happily termed them, will not account for this. Between some species there may be merely slight and single differences; but Nature can show us much more than this. We often find a complicated apparatus formed by the concurrence in one individual of many particulars of structure combining to produce an effect wholly peculiar.

Take the following instance, or rather group of instances. There are venomous serpents, of many species and in many lands, which differ most widely from the non-venomous kinds, from which, or from the ancestors of which, they are generally believed to have been derived. In these we find, to begin with, teeth which have undergone strange modifications. They are needle-like in shape. They are not fixed in the jaw. They occupy a very prominent position. They have minute perforations, terminating near, but not precisely at, the point. They have muscles by which they may be recurved, so that their points may be directed towards the throat. They have hollows in which to lie. They have muscles by which, on occasions, they may be projected beyond the mouth. Besides all this poison-secreting glands, and poison-bags, and channels of com-

munication with the perforations in the teeth. Further still, a special instinct leading the snake to make use of this wonderful weapon of offence, and suitable nerves to regulate its complicated action.

Now, unless all these numerous variations—and they might fairly be multiplied by subdivision—had in the first instance appeared simultaneously in one individual, and unless all had been duly connected, the whole apparatus would have been useless, and there would have been nothing of which natural selection could avail itself. Useful intermediate forms there can be none. A rifle is a more formidable weapon than a lance or dart, but of what use would be a thing half-way between the two? The venom-discharging apparatus has in it no part which could possibly be dispensed with.

To give one more instance. The tongue of the woodpecker is moved forwards in a singular way; not simply, as usual, by a muscle and sinew in front of the base of the tongue, but by a sinew terminating in a loop, through which passes another sinew from behind the tongue which, doubling through the loop, is attached to the base of the tongue. By this means, when the muscle is contracted, the tongue is drawn forward with a double velocity, which is to this bird specially useful. Now, it is impossible for any ingenuity to devise an action intermediate between this and the usual simple pull in respect of utility or complexity. But there is much more here than "a particular variation." The first woodpecker that possessed this structure must have had it in complete order, for otherwise the tongue would not move at all. In that woodpecker it must have commenced to exist in a rudimentary form before birth, in a germ possessing novel powers.

And here I must ask, How is it that anyone questions the Duke of Argyll's statement that "all organs do actually pass through rudimentary stages in which actual use is impossible"? Is it not precisely this which is implied in the Darwinian statement that "from the variable constitution of the ovum probably arises the varying structure of the organism developed from it"? What was afterwards developed was at first rudimentary, and useless. This is equally true of the whole organism—say of the serpent, or of the bird—and of the entirely novel and complicated apparatus found in them.

To call the apparatus in either serpent or bird "a particular variation" would be to give up the whole case for Darwinism. A wonderful *combination of many* particular variations has to be accounted for; and, so far as I can see, Darwinism utterly fails to account for it. There are thousands of cases presenting the same difficulty.

There are simpler cases of specific change, in which the concurrence, the simultaneous appearance, of many slight and particular variations is not indispensable, but only their succession in due order in the course of many generations. Here, there is some room for the theory. Thus perhaps, possibly, we might get a giraffe. But I prefer a theory which, if true at all, accounts as readily for the most complicated apparatus as for the simplest forms of living things. R. COURTENAY.

Hotel Faraglioni, Capri, January 31.

PROBABLY many readers of the recent discussion on the transmission of acquired characters will regret that a more definite conclusion has not been arrived at. This is probably due to the fact that the premises now in our possession do not admit of a definite answer yet being given. Those who assume that there is no evidence in favour of the transmission of acquired characters are mostly, I presume, supporters of "the continuity of the germ-plasm" theory of Weismann. Almost everyone admits that individuals may and do acquire certain characters due to change in environment, use, disuse, &c.; but while many maintain that these characters are transmitted to offspring, others deny that such is the case, or think that the evidence is insufficient. In supporting "the continuity of the germ-plasm" theory it is impossible to suppose that the germ-plasm is continued from one generation to another like a portion of entailed property. For each individual gives off thousands of ova or spermatozoa as the case may be, only a very few of which go to produce new individuals; therefore there is a dissipation of "germ-plasm,"—that is to say, in the germinal cells of mammals of to-day there cannot be any of the identical "germ-plasm" which existed in their remote invertebrate ancestors ages ago. For all this dissipation there must be some constructive process, otherwise the germ-plasm would come to an end.

From whence is derived this constructive material? Clearly from the exterior, for a fertilized ovum obtains material from without to admit of growth and elaboration. The constructive material, then, which the "germ-plasm" obtains—to admit of its liberal dissemination each generation—is derived from the external world, *viâ* the organism with which it is incorporated, or indeed of which it forms a part. Seeing, then, that the organism—from which this germinal matter is derived—can acquire characters—that is, undergo certain definite changes in response to altered conditions—then it seems reasonable to suppose that that part of it which ultimately finds its way to the germ-cells, is also modified during its transmission, and will therefore have more or less effect upon the forthcoming generation. But how much variation is due to the above cause, and how much to the almost infinitely various possible combinations of the two unlike germinal elements, it is impossible to say.

J. COWPER.

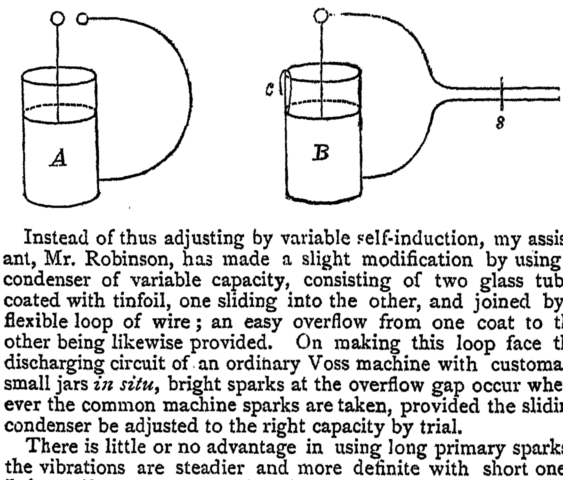
Easy Lecture Experiment in Electric Resonance.

AN experiment, exhibited by me in its early stages at the Royal Institution a year ago, and since shown here in various forms, on the overflow of one Leyden jar by the impulses accumulated from a similar jar discharging in its neighbourhood, is so simple an illustration of electric resonance, and so easily repeated by anyone, that I write to describe it.

Two similar Leyden jars are joined up to similar fairly large loops of wire, one of the circuits having a spark-gap with knobs included, the other being completely metallic, but of an adjustable length.

The jar of this latter circuit has also a strip of tinfoil pasted over its lip so as to provide an overflow path complete with the exception of an air-chink, *c*. It is important that this overflow path be practically devoid of self-induction. A jar already perforated could be well utilized for the purpose.

Then if the two circuits face each other at a reasonable distance, and if the slider, *s*, is properly adjusted, every discharge of A causes B to overflow. A slight shift of the slider puts them out of tune.



Instead of thus adjusting by variable self-induction, my assistant, Mr. Robinson, has made a slight modification by using a condenser of variable capacity, consisting of two glass tubes coated with tinfoil, one sliding into the other, and joined by a flexible loop of wire; an easy overflow from one coat to the other being likewise provided. On making this loop face the discharging circuit of an ordinary Voss machine with customary small jars *in situ*, bright sparks at the overflow gap occur whenever the common machine sparks are taken, provided the sliding condenser be adjusted to the right capacity by trial.

There is little or no advantage in using long primary sparks; the vibrations are steadier and more definite with short ones. It is needless to point out that the 2 jars constitute respectively a Hertz oscillator and receiver, but fair precision of timing is more needed with these large capacities than with mere spheres or discs, because the radiation lasts longer and there are more impulses to accumulate. Hence actual resonance as distinguished from the effect of a violent solitary wave is better marked. Moreover, the sparks are bright enough to be easily seen by a large audience. OLIVER J. LODGE.

University College, Liverpool.

African Monkeys in the West Indies.

WITH reference to the note in NATURE of February 13 (p. 349), on the occurrence of an Old-World monkey in Barbados, I may point out that the same West African monkey (*Cercopithecus callitrichus*) has also been introduced and is now found wild in St. Kitts (cf. Sclater, P.Z.S., 1866, p. 79). It likewise

occurs in Nevis, whence the Zoological Society received living specimens (presented by Mr. Graham Briggs) in 1870.

The only West Indian island in which Quadrumana of the *American* type occurs is Trinidad, which was, doubtless, formerly part of the mainland of South America.

3 Hanover Square, W., February 17. P. L. SCLATER.

Galls.

I HAD not intended to take any further part in this correspondence; but the interesting suggestion which has now been made upon the subject by Mr. T. D. A. Cockerell (NATURE, Feb. 13, p. 344) induces me to withdraw the sentences that he quotes from my previous letters, to the effect that it seems impossible to imagine any way in which galls can be attributed to natural selection acting on the plants *directly*. In my own consideration of the matter this seemed "obvious," and therefore my motive in taking up the difficulty as presented by Mr. Mivart was that of "asking whether anybody else had a better explanation to offer" than the one which my letter suggested—viz. "that natural selection may operate on the plants *indirectly through the insects*," by always selecting those insects the character of whose secretions is such as will best cause the plants to grow the particular kind of morphological abnormality which the larvæ require. Mr. Cockerell, however, has now furnished what seems to me an extremely plausible hypothesis, showing that there is a way in which it is quite conceivable that the growth of galls may be an actual benefit to the plants, and therefore that natural selection may act directly on the plants themselves in evolving these sometimes highly specialized structures for the use of their parasites. Mr. Cockerell informs me in a private communication that he has been verifying this hypothesis by observations in detail; but whether or not he will be able to establish it, I think at any rate he has done good service in thus suggesting another possibility.

On the other hand, I cannot see that Mr. Ainslie Hollis has helped us at all (NATURE, January 23, p. 272). For he merely enunciates the truism that trees which were not endowed with sufficient "developmental vigour" adequately to resist the attacks of gall-making insects "would doubtless have long ago succumbed in a struggle for existence." And this truism he appears to suppose furnishes an explanation of how "natural selection, operating in the ordinary manner," has produced galls for the exclusive benefit of the insects. But it is obvious that the more detrimental the growth of galls has proved to trees, the less reason there must have been for natural selection, "operating in the ordinary manner," to have developed these often highly specialized structures for the benefit of parasites.

London, February 13. GEORGE J. ROMANES.

The Supposed Earthquakes at Chelmsford on January 7.

NATURE for January 16 (p. 256) reprints from the *Essex County Chronicle* a short account of two supposed earthquake-shocks felt at and near Chelmsford on January 7, at 12.30 and 1.25 p.m. Being engaged in the study of British earthquakes, I made inquiries in the district referred to, and the result of these is to show that the shocks were almost certainly due to the firing of unusually heavy guns at Woolwich. It may be worth while to state the evidence for this conclusion somewhat fully, as it will be difficult to obtain it in after years.

(1) I applied to the authorities at Woolwich and Shoeburyness as to the nature of the firing on January 7. At the latter place, the only practice was from 9-inch and 10-inch guns, the maximum charge used was 70 pounds of powder, and therefore not capable of producing the shocks felt at Chelmsford. At Woolwich, however, the 110-ton gun, "*the heaviest in H.M. service*," was fired at the times mentioned.

(2) *Form of the Disturbed Area.*—The only accounts I have as yet received are from the following places: Great Warley (near Romford), Brentwood, Epping, Ingatestone, on the road between Ongar and Fyfield, Roxwell, Chelmsford, Chignall St. James, and Chipping Hill (Witham); which are respectively at about 6, 12½, 14, 16, 16, 21, 24, 24, and 32 miles distance from Woolwich. Referring to a map of Essex, it will be seen that these places all lie close to a line drawn from Woolwich in a north-easterly direction; with the exception of Epping, the direction of which is about north by east from Woolwich. According to the *Times* weather report of January 8, southerly and south-westerly breezes prevailed very generally throughout the kingdom on the previous day.

(3) *Nature of the Shock.*—In four cases, the shock was in the first instance attributed to the firing of heavy guns. If there was any vibration of the earth, it must have been very slight, and the following descriptions seem to leave little doubt that the rattling of windows noticed was due to an air-wave.

Great Warley—The shock "broke a pane of glass 4 feet × 2 feet on my job."

Brentwood—"The shocks commenced as a low rumble, increasing till the doors shook and rattled, as though the rumbling was followed by a bang or explosion."

Between Ongar and Fyfield (the observer driving)—"The ground felt as if it were sinking," and there was "a rumbling noise something like guns in the distance."

Roxwell—The sound "exactly resembled the report of the big guns at Shoebury, but was far louder than we usually hear them."

Chelmsford (the observer walking)—There was "a noise as of a very heavy weight being rolled across the floor of the room of the house to the south of him, which he was passing."

Chignall St. James—"The shock was extremely slight, but there was a most pronounced concussion in the air which made a sound on the windows as if a person had thumped the centre of the window frame with the soft part of his hand. There was no tremulous motion felt."

Witham—The observer "heard a strange rumbling sound which seemed to slightly deafen him, but he felt no vibration of the earth."

That the disturbances recorded had only one origin is, I think, evident, (1) from the decrease in intensity (roughly speaking) as the distance from Woolwich increases, and (2) from there being no considerable gap between the places of observation. Records from the immediate neighbourhood of Woolwich could hardly be expected, as there they would naturally be attributed to their proper source.

I am indebted to the editor of the *Essex County Chronicle* for inserting a letter asking for observations on the shocks, and to several gentlemen for the courtesy and kindness with which they replied to this letter and to other inquiries that I made in the surrounding district. CHARLES DAVISON.

38 Charlotte Road, Birmingham, February 13.

Shining Night-Clouds.

IN July last, on a fine night, about 8 p.m. (two hours after sunset), I noticed a fleecy cloud lit up by a yellowish light, directly over the back of a range of hills due west from this place. As it did not move, it struck my attention, and I observed that what little wind there was carried the few floating clouds north-east to south-west. I continued to watch the cloud, which covered say 4° or 5°, until 11 p.m., and concluded that as in that direction lay the Puracé volcano, about 40 miles away, the light and cloud probably came from it. But I made inquiries by telegraph, and found that no eruption had taken place in the Puracé, which has been quiet now for many years. I regret, seeing now that the subject is interesting, that I did not observe more carefully. I may add that in the direction of the cloud no prairie or forest fire could have occurred to account for it. ROBERT B. WHITE.

Agrado (lat. 2° 20′ N.), Department of Tolima, U.S. of Colombia, S.A., December 22, 1889.

A Greenish Meteor.

TO-NIGHT (Jan. 30), at 8.15 p.m., I saw a meteor which, notwithstanding a bright moon, shone out exceedingly brightly, exceeding any star. It appeared to travel south, for about 10°, vanishing about 15° above the horizon. Its colour differed from that of any meteor I have seen before, being pale green or greenish. T. D. A. COCKERELL.

West Cliff, Custer Co., Colorado, January 30.

THE MOLECULAR STABILITY OF METALS, PARTICULARLY OF IRON AND STEEL.

(1) ALLOW me to add some words relative to the very timely lecture on the hardening and tempering of steel, recently published by Prof. Roberts-Austen

(NATURE, xli. pp. 11, 42). I desire, in the first place, to point out the bearing of the singular minimum of the viscosity of hot iron (*loc. cit.*, p. 34) on the interpretation given of Maxwell's theory of viscosity (*Phil. Mag.* (5), xxvi. pp. 183, 397, 1888; xxvii. p. 155, 1889). When iron passes through Barrett's temperature of recalescence, its molecular condition is for an instant almost chaotic. This has now been abundantly proved (cf. John Hopkinson, Phil. Trans., London, clxxx. p. 443, 1889, where the literature may be found; cf. Osmond, below). The number of unstable configurations, or, more clearly, the number of configurations made unstable because they are built up of disintegrating molecules, is therefore at a maximum. It follows that the viscosity of the metal must pass through a minimum. Physically considered, the case is entirely analogous to that of a glass-hard steel rod suddenly exposed to 300°. If all the molecules passed from Osmond's β state to his α state together, the iron or steel would necessarily be liquid. This extreme possibility is, however, at variance with the well-known principles of chemical kinetics. The ratio of stable to unstable configurations cannot at any instant be zero. Hence the minimum viscosity in question, however relatively low, may yet be large in value as compared with the liquid state.

(2) My second point has reference to the function of carbon in steel. It is not to be understood that we ignore the importance of the changes of carburation produced by tempering steel. To explain the varied physical phenomena which accompany temper, it is sufficient to recognize some *special* instability in the tempered metal. This is given by the carbide configuration, and the physical explanations in question may be made without specifying its nature further. Hence the permissibility of the purely physical considerations.

On the other hand, it is indeed surprising that, on the part of engineers and chemists, the important subject of temper has been but inadequately dealt with, as Prof. Austen justly remarks. Sir Frederick Bramwell (cf. NATURE, xxxviii. p. 440), in his inaugural address at Bath, in 1888, dwelt at some length on the subject of temper. The question is again touched upon by Mr. Anderson at the Newcastle meeting of the British Association. Neither of these gentlemen, however, really shows forth the gist of the matter. Indeed, even in Ostwald's massive "Lehrbuch der Allgemeinen Chemie" (Leipzig, W. Engelmann, 1887), full of examples as it is, bearing on all points of chemical physics, the frequent and exceptionally important case of tempered steel is altogether absent. And yet the chemical interpretation to be given to the phenomena of temper seems to be closely at hand. Dr. Strouhal and I (*Wied. Ann*, xi. p. 390, 1880; Bulletin U.S. Geol. Survey, No. 14, chap. ii., 1885) showed that, by the process of hardening, the electrical resistance of steel may be increased by more than three times its value for the soft metal. If the hard rod is now softened, the resistance again decreases by an amount depending on the temperature to which the hard metal is exposed and on the time of such exposure, in a way which, throughout the whole research, is beautifully sharp and characteristic. Eventually, the relatively low resistance of soft steel is again reached. Now suppose the carbon molecule of steel to be dissolved in the metal, forming an alloy of Matthiessen's Class II. Seeing that the quantity of carbon contained is not large, the electrical resistance of hard steel is at once an expression of its chemical composition, structurally unknown though it be. Hence in the electrical diagram of the phenomena of temper constructed by Dr. Strouhal and myself, the time variations of resistance of hard steel at any given temperature may be interpreted as a case of Wilhelmy's (*Pogg. Ann.*, lxxxi., pp. 413, 499, 1850) rate of chemical reaction (*Reactionsgeschwindigkeit*), and expressed in accordance with his well-known exponential law. This indeed is the character of the observed time curves. Hence also the full diagram of the phenomena of temper, considered both in their variation with time and with temperature, is available for the elucidation of most points relative to the effect of temperature on rate of chemical reaction.[1]

(3) A further remark may be made relative to Osmond's (*Annales des Mines*, July–August, 1888, pp. 6–7; *Mém. de l'Artillerie de la Marine*, Paris, 1888, p. 4) iron of the α and the β type. The assertion that mere strain partly changes α into β iron is in conformity with the viscous behaviour of the metal. For it appears that the effect of any mechanical strain as well as of temper, is marked decrease of the viscosity of the metal. Osmond's theory, however, appears to explain too much. Since most metals can be similarly hardened by straining, it would follow that there should be α and β varieties in all these cases, even though a molecular change corresponding to Gore's phenomenon in iron has only in a few instances been observed (iron, nickel, platinum-iridium alloy). I believe, however, that there is reason to be urged even in favour of this extreme view.[2] The ion theory of metallic conductivity is fast gaining ground.

J. J. Thomson states it in his well-known book ("Applications of Dynamics," p. 296). Giese (*Wied. Ann.*, xxxvii. p. 576, 1889) has outlined an ion theory of electric conduction, uniformly applicable to metals, electrolytes, and gases. It seems to me, if a preliminary hypothesis be made relative to the evolution of a magnetic field out of an electric field; if advantage be taken of the spiral distribution of points which frequently results from the symmetrical interpenetration of two congruent Bravais systems;[3] if, finally, in metals, the function performed by a bodily transfer of ions can also be performed by an exchange of the charges of charged atoms (Giese, indirectly Helmholtz), that the possibility of an ion theory of magnetism may be suspected. Quite apart from the influence of a field, the conditions of exceptionally close approach favourable to the transfer of charges from atom to atom, are given by the distribution of the heat agitation in the metal.

(4) I will close this note by some remarks on the change of the character of diffusion when occurring in solids. Studying the coloured oxide coats on iron, Dr. Strouhal and I (Bull. U.S.G.S., No. 27, p. 51, 1886) pointed out that the outer surface of the film is oxidized as highly as possible in air; and that the inner surface of the film, continually in contact with iron, is reduced as far as possible. This distribution of the degree of oxidation along the normal to the layer, is equivalent to a force in virtue of which oxide is moved through the layer, from its external surface to its internal surface. The formation of an oxide coat is thus a case of diffusion. Conformably with this view, the film, during its formation, behaves like an electrolyte, as was pointed out by Franz, Gaugain, and Jenkin, and more recently by Bidwell and by S. P. Thompson.

We then adverted to the crucial difference between diffusion in solids and diffusion in liquids, inasmuch as in the former case (solids) diffusion demonstrably *ceases* after a certain small thickness is permeated. The limit thickness of the film is reached asymptotically, through infinite time. It has a definite value for each temperature, increasing as temperature increases. In the light of other evidence since gained, this explanation is substantiated. The formation of the

[1] An ulterior consideration presents itself here relative to an extension of the theory of Arrhenius (*Wied. Ann.*, iv. p. 391, 1878) to metallic conductivity. Arrhenius and Ostwald find in the maximum of electrolytic conductivity a measure of rate of reaction. I must pass over this question here, since it is without immediate bearing on the text.

[2] I have spent much time in endeavouring to throw light on this question, and will indicate the results later. My methods were (1) to find the effect of mechanical strain on the carburation of steel; (2) to find the effect of strain on the rate of solution; (3) to find the hydro-electric effect of stretching.

[3] A good account of the relations of the Bravais and the Sohncke systems is given by H. A. Miers, in NATURE, xxxix. p. 277.

oxide coat is a case of solid diffusion, and as such it bears the same relation to the diffusion of liquids, that the viscosity of solids bears to the viscosity of liquids. The two phases (solid, liquid) of each phenomenon are to be correlated in ways essentially alike. The available stress, as compared with the available instability at a given temperature, determines the time character of the result.

CARL BARUS.
Physical Laboratory, U.S. Geological Survey,
Washington, D.C.

CHRISTOFORUS HENRICUS DIEDERICUS BUYS BALLOT.

BUYS BALLOT was born on October 10, 1817, at Kloetinge in Zealand; was a student in arts and the natural sciences at the University of Utrecht, where he first became Lector of Physics and Chemistry in 1844, and then successively Professor of Mathematics in 1847, and of Experimental Physics in 1870, which latter chair he ceased to hold in November 1887 on completing his fortieth year as Professor. He was appointed Director of the Royal Meteorological Institute of the Netherlands in 1854, and held this position with great ability and distinction till his death on Monday, the 3rd of the present month.

His first contribution to science was a paper on a chemical subject in 1842, this being a science of which he was Lector at the time; but soon thereafter he turned his attention to meteorology, which he emphatically made the business of his life. The following are among the earlier of his papers on the subject, and they are, it will be seen, very significant of his future work:—"On the Influence of the Rotation of the Sun on the Temperature of our Atmosphere," in 1846; "On the Importance in Meteorology of Deviations from the Mean States of the Atmosphere," in 1850; "Results of the Observations of 1849 and 1850 in different places in Holland," in 1851; and "On Synchronous Representations of Weather Phenomena," in 1854.

In these early times of meteorology, when instruments and modes of observing still greatly needed the guiding hand of science towards the founding of international meteorology, Dr. Buys Ballot was wisely led to attempt the construction of no general isobaric and isothermal maps in investigating storms and other weather phenomena, but contented himself in investigating weather disturbances by representing them over the surface of Europe by means of deviations from the means, or averages, of the places represented. In this mode of working he made several of his more important contributions to meteorology, and out of it developed the system of storm warnings he issued for Holland. In this connection his barometric and thermometric means for a very large number of places over Europe will long be a standard work. Of these contributions, unquestionably the most important is that known as BUYS BALLOT'S LAW OF THE WINDS, which states the relation between the direction of the wind and the distribution of atmospheric pressure at the time the wind is blowing. This relation was further developed by Dr. Buchan in 1869, in his paper on the mean pressure of the atmosphere and prevailing winds of the globe, in which it was shown that the prevailing winds of all climates are simply the result of the distribution of pressure.

One of the most exhaustive discussions of the influence of the moon on weather was made by Dr. Ballot. The discussion covered a period of about a century, and he showed that the longer the period the closer do the cases for or against any such influence approach equality. Subsequent to Maury, Dr. Ballot was one of the earlier and most energetic and successful workers in maritime meteorology, and his meteorological charts of the routes of Dutch ships over the great oceans is a standard work. Dr. Ballot also took an active and efficient part in the Meteorological Conferences and Congresses held at intervals from 1872 to 1888, which have brought about a greater uniformity in meteorological observations and discussions than previously existed. He was chosen, by the first Congress, President of the Permanent Committee. Among his last works was the proposal of a method of developing and representing the variability of the weather and climates by the values of the deviations of the daily observations from the averages, irrespective of sign.

The great merits of his indefatigable services to science, but more particularly to meteorology, were recognized by his being made LL.D. of Edinburgh University, Knight of the Order of the Netherland Lion, Commander of the Order of Franz Joseph of Austria, and of St. James of the Sword of Portugal, and Knight of second class of the Prussian Order of the Crown. But above all, his ever readiness in every degree to oblige, the genial sunshine of his face, and his lovableness, make his death to be felt by many of us as a sharp personal bereavement.

NOTES.

ON Tuesday evening the Cambridge University Natural Science Club and the Master of Downing (Dr. Alex. Hill) gave a *conversazione* at Downing Lodge, at which 260 guests, including many distinguished residents and non-residents, were present. The several scientific professors were very liberal in lending the treasures from their museums, and as this is the first entertainment of the kind which has been given in Cambridge, many objects of great historic interest, such as Babbage's calculating machine, Cavendish's apparatus, &c., were exhibited. Artificial silk was spun, quartz filaments drawn, smokeless gunpowder and other scientific novelties shown. One of the most interesting exhibits was a series of Egyptian heads unwrapped from their mummy cloths, and artfully "restored" by Prof. Macalister. A very attractive feature of the entertainment was an address by Dr. Lauder Brunton, who had much that was interesting to say about his recent experiences in India. Mr. Gardiner illustrated the dispersion of seeds by the aid of the limelight and boxes of seeds of various kinds suspended from the ceiling.

THE annual general meeting of the Geological Society of London will be held to-morrow (Friday) at 3 o'clock, and the Fellows and their friends will dine together at the Criterion Restaurant at 7.30 p.m.

BEFORE the next ordinary meeting of the Royal Microscopical Society, it will have moved its quarters from the rooms hitherto occcupied by it in King's College, which are now required for the purposes of the College, to 20 Hanover Square. The ordinary meetings will in future be held on the third instead of the second Wednesday in the month, and the annual meeting in January instead of February. The Quekett Microscopical Club has also transferred its place of meeting to 20 Hanover Square since the commencement of the year.

WE regret to have to record the death of Sir Robert Kane, F.R.S. He died after a short illness on Sunday, the 16th inst., at his residence in Dublin.

THE fine buildings of the University of Toronto were almost wholly destroyed by fire last Friday. The flames were unfortunately fanned by a strong wind, and the fire spread so rapidly that hardly anything could be saved. A small number of specimens in the museum, and some of the scientific apparatus, were brought out by students, but they were mostly broken while

being removed. The Canadians are justly proud of the University of Toronto, and will no doubt provide for it even more splendid buildings than those which are now in ruins.

SIGNOR SELLA'S views of the Caucasus have been on exhibition in the Royal Geographical Society's map-room since Friday last, and will continue to be exhibited till the close of the month.

WE print elsewhere Prof. David P. Todd's record of work done by the U.S. Scientific Expedition to West Africa, 1889, of which he was director. This is one of several bulletins printed on board the U.S.S. *Pensacola*.

IN the engineering notes from North-West India, of *Engineering* of the 14th inst., we find a most interesting account of the testing of the Chenab Bridge, near Mooltan. This bridge consists partly of seventeen spans of 200 feet, which are of mild steel throughout. These trusses are of the Whipple-Murphy type, with raking heel posts; the ties are at an angle of 45°, and consequently the depth is a tenth of the span. In previous girders of this type, made in iron, the deflection under full loads was usually less than 0·0004 of the span, while here $1\frac{1}{2}$ inch, equal to 0·0006, obtains throughout, and in each case the observed permanent set is less than $\frac{1}{8}$ inch in the whole thirty-four girders in the viaduct. *Engineering* observes that "there is thus no question of bad workmanship either in the pieces sent out from home or in the erection at site, and it is very clear that steel structures, especially when so light as these spans, which only weigh, with corrugated floor and all bearing and expansion gear, 220 tons each, are necessarily more sensitive than those of iron."

THE new number of the *Internationales Archiv für Ethnographie* (Band ii. Heft vi.) opens with a valuable paper, by Prof. G. Schlegel, of Leyden, on Siamese and Chinese-Siamese coins. This contribution is illustrated by a coloured plate. Of the other papers, the most important is an account of the Nanga of the Fiji Islands, by Mr. Adolph B. Joske, Fiji. These remarkable stone inclosures, now ruined, were first brought to the notice of anthropologists by the Rev. Lorimer Fison, of the Australasian Wesleyan Mission. Three of them have been visited by Mr. Joske, and he is thus enabled to give the plan of an inclosure drawn from his own measurements. His paper has been edited by Baron Anatole von Hügel, who adds instructive notes. In another paper, Prof. Giglioli gives an interesting account of a remarkable stone axe and stone chisel in use among the Chamacocos of South-East Bolivia.

WE are glad to observe that in the Ceylon estimates for the current year provision is made for an increased vote of Rs. 10,000 for archæological purposes. Sir Arthur Gordon, in explaining the vote, said, "It is proposed to make some systematic examination of the interesting remains at Sigiri, and to commence on a modest scale, before the rapidly disappearing monuments of the past have altogether perished, a species of archæological survey resembling that carried on in India. Such an examination should be completed in about three years, and the vote is proposed to cover the salary and travelling expenses, for 1890, of the officer selected for the purpose."

A LARGE and rich collection of specimens of amber, illustrating all the varieties found in the amber district of North Germany, has lately been sent to the New York School of Mines by one of its earliest graduates, Mr. H. A. Demelli, now a resident of Berlin. At a recent meeting of the New York Academy of Sciences, this collection was examined with great interest by the members, and Dr. Newberry, the President, read an instructive paper on amber. After the reading of the paper, Dr. N. L. Britton spoke of the occasional occurrence of amber in New Jersey, in connection with the lignites so abundant in the Cretaceous and Eocene beds; and Mr. George F. Kunz exhibited several specimens of American amber, one of which —from Mexico—excited much admiration. Mr. Kunz said that during the last fifteen or twenty years travellers had occasionally brought specimens of a very remarkable amber from some locality in Southern Mexico. The only thing known about this amber is that it is taken to the coast by natives, who report that it occurs in the interior so plentifully, and in such large pieces, that they use it for making fires. It is of a rich, deep golden yellow, and, when viewed in different positions, it exhibits a remarkably green fluorescence, like that of certain petroleums. It is perfectly transparent, and, according to Mr. Kunz, even more beautiful than the famous so-called opalescent or green amber found at Catania, Sicily.

A FRESH illustration of the way in which foreign plants may become "weeds" under new and favourable conditions is afforded by *Melilotus alba* in the Western States of America. It was introduced a few years ago as a garden-plant, and has spread so rapidly in the rich bottom-lands along the Missouri River that, according to *Garden and Forest*, it is fast driving out the sunflower and other native weeds. It is commonly called "Bokhara clover."

AT the meeting of the Scientific Committee of the Royal Horticultural Society, on February 11, Dr. Oliver and Prof. Scott presented an interim report on the investigations undertaken by them respecting the effects of London fogs on plants under glass. Specimens of orchids affected by fog had been received from Messrs. Veitch and Son, Chelsea; and of tomato plants from the superintendent of the Royal Horticultural Society's gardens at Chiswick. On the suggestion of the chairman, it was decided that the chemical constituents of London fog should be investigated, and that the exciting causes of the injury to plants should be traced. In order that the work might be carried out under advantageous circumstances, it was resolved that application should be made to the Government Grant Committee of the Royal Society for pecuniary aid.

AT the same meeting of the Royal Horticultural Society's Scientific Committee, Mr. McLachlan drew attention to a disease in sugar-cane at St. Vincent, where in some localities about 25 per cent. of the crop would be lost this year. According to Mr. Herbert Smith, who had examined the canes, a beetle of the family Scolytidæ, and the larva of a moth, were concerned. It is probable that the beetles enter the canes only by the exit holes of the moths, and that the moth is a widely spread species, already known to attack sugar-cane in other countries.

IN the January number of the *American Naturalist* Mr. R. E. C. Stearns begins what promises to be an interesting series of papers on the effects of musical sounds on animals. His first paper deals with "dogs and music." From his friend, Prof. George Davidson, of California, he has received the following instance:—"A small black-and-tan named 'Bessie,' belonging to Mr. A. B. Corson, of North Fifth Street, Philadelphia, will, on hearing 'Shall we meet beyond the river?' sung, throw her head back and set up a most dismal howl, while the tears will run down her cheeks. If the tune is played solemnly on an organ and no word spoken, the same thing will occur; but if any of the words are spoken, with not the slightest musical intonation, she will run to the speaker, and beg and plead in her own way, and do everything but speak, to have it stopped."

THE *Annalen der Hydrographie und Maritimen Meteorologie* for December, published by the German Admiralty, contains an interesting discussion by Dr. W. J. van Bebber, on the dependence of the force of the winds upon the surface over which they blow. It is generally admitted that the winds at sea are, under

similar circumstances, stronger than on land; but actual comparisons, such as the author has undertaken, are not frequently made. He has chosen two stations on the coast—viz. Cherbourg and Hurst Castle—having a different position with regard to the sea, but at which the observations are made under nearly similar conditions. The results of careful comparisons under eight points of the compass, for a period of several years, plainly show that in all months the northerly and north-easterly winds at Cherbourg are considerably stronger than at Hurst Castle, and that the southerly winds at Cherbourg fall considerably short in strength of those at Hurst Castle. The tables show that the strong winds coming from the sea are on an average one degree of Beaufort's scale (1–12) heavier than those coming from the land, while, with lighter or local winds, the difference often amounts to two degrees of the above scale. Information of this kind should be of use to fishermen and others when putting to sea.

M. PLANTAMOUR gives, in a recent number of the *Archives des Sciences*, the results of his eleventh year's observations of periodic movements of the ground, as shown by spirit-levels. It appears that, while in general the east side sinks with lowering of temperature, and rises with a rise, these movements do not always follow with the same rapidity. A sudden change of temperature produces at once a rise or sinking of the east side; but the maxima of the ground-positions rarely coincide with the maximum or minimum of temperature. This eleventh year is exceptional in that the extremes of temperature are but one or two days in advance of those of the movements, whereas in previous years the retardation has been a fortnight to four months behind minimum temperature, and a fortnight to three months behind maximum. In two years (1881 and 1885) the maximum of rise was even four days before the maximum of temperature. Thus, while temperature seems to be the chief cause of the oscillations, some other opposing cause must be at work. M. Plantamour compared the eleven years' mean effects with the variations in solar intensity, but failed to detect any relation.

CARL HESS, the German naturalist, has proved by minute microscopical investigation that the eye of the mole is perfectly capable of seeing, and that it is not short-sighted, as another naturalist (Kadyi) would have us believe. Hess maintains that, in spite of its minute dimensions,—1 millimetre by 0·9 millimetre—the eye of this little creature possesses all the necessary properties for seeing that the most highly-developed eye does; that it is, indeed, as well suited for seeing as the eye of any other mammal, and that in the matter of refraction it does not differ from the normal eye. In order to bear out the theory of short-sightedness, the physiological reason was adduced that in its subterranean runs the mole is accustomed to see things at close distances, and that its eye had become gradually suited to near objects. But to this Hess objects that the mole when under ground most probably makes no use of his eyes at all, as it would be impossible to see anything owing to the absence of light, but that when he comes to the surface, and especially when he is swimming, he does use his eyes. In order to accomplish this, he only has to alter the erect position of the hairs which surround and cover his eyes, and which prevent the entry of dirt when he is under ground, and at the same time to protrude his eyes forward.

IT seems rather strange that, while skins and eggs of the Great Auk are so highly valued, the public rarely hear of Pallas's Cormorant, the extinction of which in the North Pacific corresponds to that of the Great Auk in the North Atlantic. Only four specimens of Pallas's Cormorant are known to exist in museums; no one possesses its eggs; and no bones were found or preserved until Mr. Leonhard Stejneger, of the Smithsonian Institution, was so fortunate some years ago as to rescue a few of them. Yet this bird was the largest and handsomest of its tribe. So says Mr. Stejneger in an interesting paper—just issued by the Smithsonian Institution—in which he records how the bones referred to were found by him in 1882 near the north-western extremity of Behring Island. In an appendix to this paper Mr. Stejneger's "find" is fully and exactly described by Mr. Frederic A. Lucas.

WE have received the first two numbers of the *Scottish Journal of Natural History*. This monthly periodical is intended to be mainly a chronicle of the work done by the different Natural History Societies in Scotland; but short papers on subjects connected with Natural History will also be given, and we notice that articles have been promised by well known men of science, including Profs. James Geikie, G. J. Romanes, and many others. At present very few of the Scottish Natural History Societies print Transactions; so there is ample room for the new venture, and we wish it all success. Communications are to be addressed to the Editors, care of the publisher, Mr. W. B. Robinson, 194 Sauchiehall Street, and 105 New City Road, Glasgow.

THE first part of the Memoirs and Proceedings of the Manchester Literary and Philosophical Society for the current session has been issued. It contains a paper by Mr. Charles Bailey, on the discovery near Ribblehead of *Arenaria gothica*, a plant new to Britain, the typical form of which has so far been recorded only for two Swedish localities. The Ribblehead specimens are stated to be more robust than those from Sweden. The issue also includes a paper by Mr. Charles H. Lees on the law of cooling and its bearing on the theory of heat in bars; and the first part of Mr. Faraday's "Selections from the (unpublished) Correspondence of Colonel John Leigh Philips, of Mayfield, Manchester" (1761–1814). The latter includes letters from Dr. Henry Clarke (the mathematician), James Sowerby, and a number of other persons of local eminence during the latter half of the last century.

PROF. WEISMANN requests us to state that in his article on Heredity, printed in NATURE on February 6, the sentence beginning on p. 319, line 38, should have read—"Sir William Thomson, in endeavouring to make clear the dispersion of rays of light by conceiving of a molecule as consisting of hollow spheres enclosed one within the other and in contact with one another through springs, never believed," &c.

Two gaseous fluorides of carbon, the tetrafluoride, CF_4, and the difluoride, C_2F_4, have been isolated, and form the subject of two simultaneous papers contributed to the current number of the *Comptes rendus*. One of these communications is from M. Moissan, whose energy in this domain of chemistry appears untiring. Unlike chlorine, fluorine directly attacks carbon with varying degrees of energy, according to the form in which the carbon is presented. When a current of pure fluorine is passed over the purest form of lamp-black, which has previously been freed from hydrocarbons by digestion with petroleum and boiling alcohol, combination occurs with such energy that the whole of the finely divided carbon becomes instantly incandescent. The lighter varieties of wood charcoal also take fire spontaneously in fluorine, the gas appearing to be first condensed for a few moments, and then the mass becomes suddenly incandescent and throws off brilliant scintillations. If the density of the charcoal is greater, and there is no loose dust upon its surface, it is necessary to warm it to 50°–100° C. in order to bring about combination and its accompanying incandescence. When once the incandescence is started at any spot it rapidly extends throughout the entire mass. Ferruginous graphite requires to be heated to a temperature just below dull redness, and gas retort carbon to full redness, in order to effect combination, while the diamond may be heated for any length of time over a

Bunsen lamp without any alteration in weight being noticeable. The products of combination are generally gaseous mixtures of CF_4 and probably C_2F_4. When the most readily attacked varieties of carbon are employed, and only in small quantities so as to avoid excess, the gas is almost pure CF_4. Carbon tetrafluoride is a colourless gas, which liquefies under a pressure of five atmospheres at 10° C. It is completely absorbed and decomposed by an alcoholic solution of potash with production of potassium fluoride and carbonate. On decomposing the latter salt with an acid the volume of carbon dioxide liberated is the same as that of the carbon tetrafluoride used. CF_4 is slightly soluble in water, more readily in carbon tetrachloride, alcohol, or benzene. Determinations of its density gave numbers which agreed with the formula CF_4. If excess of carbon is heated to redness in a platinum tube, and fluorine allowed to slowly stream through, another gas is obtained on collecting over water which is not capable of being absorbed by alcoholic potash. This gas liquefies at 10° under a pressure of 19–20 atmospheres. M. Moissan does not seem to have yet determined its composition, but it appears likely to be the C_2F_4 described in the second communication by M. Chabrié. M. Moissan also states that CF_4 may likewise be prepared by passing vapour of carbon tetrachloride over silver fluoride heated to a temperature of 300° C. in a glass or metal tube. M. Chabrié shows that both CF_4 and C_2F_4 may be obtained by heating the corresponding chlorides of carbon with silver fluoride in a sealed tube to 220° C. In an actual experiment 5·1 grams of AgF were heated with 1·55 grams of CCl_4 for two hours, at the end of which time the tube, which itself was but little attacked, was opened, and an almost theoretical yield of CF_4 obtained; the gas was totally absorbed by alcoholic potash in accordance with the equation $CF_4 + 6KOH = K_2CO_3 + 4KF + 3H_2O$. When C_2Cl_4 was used instead of CCl_4, a gas whose density corresponded to the formula C_2F_4 was obtained. The experimental density was 3·43; the calculated value for C_2F_4 is 3·46. The spectra of the two fluorides, according to M. Moissan, exhibit the lines of fluorine very clearly, together with several broad bands, resembling the flutings of carbon.

OUR ASTRONOMICAL COLUMN.

Objects for the Spectroscope.

Sidereal Time at Greenwich at 10 p.m. on February 20 = 8h. 3m. 7s.

Name.	Mag.	Colour.	R A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G.C. 1565	—	—	7 36 25	− 14 29
(2) 27 Cancri	6	Yellowish-red.	8 20 39	+13 1
(3) β Cancri	4	Yellow.	8 10 36	+ 9 32
(4) ζ Canis Min. ...	5	White.	7 46 0	+12 3
(5) 26 Pickering ...	Var.	Reddish-yellow.	7 57 2	− 12 47
(6) S Cygni	Var.	Reddish.	20 3 14	+57 40

Remarks.

(1) "Planetery nebula; pretty bright, pretty small; extremely little elongated." The spectrum has not yet been recorded.

(2) A star of Group II. Dunér states that the bands are very wide and dark in the red, but weaker in the green and blue. He does not, however, state what bands are present. Observations similar to those already suggested for other stars of the group are required.

(3) This is stated to have a fine spectrum of the solar type by Vogel. The usual differential observations are required.

(4) A star of Group IV. (Vogel). Usual observations required.

(5) This star has a very feeble spectrum of the Group VI. type, which has not yet been fully described.

(6) Although Cygnus is not now in the most convenient position for observations, it may still be observed soon after sunset. The variable, S Cygni, has not yet had its spectrum recorded, and the approaching maximum (February 28) may therefore be taken advantage of. Gore states the period as 323 days, and the range as from 8·8–10·1 at maximum to <13 at minimum. If it has a banded spectrum, as may be expected from the colour, the type of spectrum will probably not be difficult to determine, notwithstanding the faintness of the star.

A. Fowler.

Progress of Astronomy in 1886.—An account of the progress of astronomy in the year 1886, by Prof. Winlock, has been issued from the Smithsonian Institution. Although the record is primarily intended to serve as a series of notes for those who have not access to a large astronomical library, the bibliography will be found useful to the professional astronomer as a reference list of technical papers. A considerable amount of useful information is given in this extract from the Smithsonian Report for 1886–87, the section devoted to reports of Observatories being very complete. A subject-index to the review has been effected by inserting the necessary page references to the bibliography.

The Maximum Light-Intensity of the Solar Spectrum.—We have received from Dr. Mengarini his paper on the above subject (*Untersuchungen zur Naturlehre des Menschen und der Thiere*, xiv. Band, 2 Heft). After reviewing the previous work that has been done on the varying intensity of different parts of the spectrum, the author describes the three methods he used in his researches. The observations led him to conclude that the maximum of light-intensity is subject to variability in position from day to day and hour to hour, just as the maxima of thermal and chemical effects of the spectrum, although the sky be clear and the atmosphere steady. Using a prismatic spectrum, it was found that the maximum light-intensity fluctuated between about λ 564 and D, and, generally speaking, was more pronounced in the morning than in the afternoon. Some observations made at Rome in July 1881, on clear or slightly clouded days, showed that the maximum shifted from λ 564·1 to 584·3.

Spectrum of Borelly's Comet, *g* 1889.—Mr. Backhouse, in a letter to the *Observatory*, notes that he observed the spectrum of this comet with a Browning miniature spectroscope on the 15th and 19th ultimo. The three CO bands were very vividly seen, but no other line; on the former date there was a very faint continuous spectrum, but on the latter only a suspicion of such.

Spectra of δ and μ Centauri.—Prof. Pickering, in a communication to *Astronomische Nachrichten*, No. 2951, records that an examination of the photographs of stellar spectra taken by Mr. S. J. Baily at the Harvard Observatory station, near Closica, Peru, shows that the F line due to hydrogen is bright in the spectra of the stars δ and μ Centauri.

On the Star System ξ Scorpii.—Some elaborate researches into the orbits of the components of this system were given by Dr. Schorr in an inaugural dissertation at Munich University last year. All available measures of position-angle and distance have been brought together and compared with those derivable from the new elements found, making the computation of great value.

GEOGRAPHICAL NOTES.

On Tuesday, Dr. Nansen lectured in Christiania on his plan for a North Pole Expedition. He advocates the employment of a ship built with a special view to strength, having its sides constructed at such an angle that, instead of being crushed by the ice, the vessel would be raised by it. The Expedition, he thinks, should advance through the Behring Straits, where the vessel would be carried northward by a favourable current. At the New Siberian Island the vessel would enter the ice-floes. It would then "proceed towards the North Pole, in which direction the current would probably carry it."

The *Colonies and India* gives the last news from Cooktown relating to Sir William Macgregor's explorations in New Guinea. His project was to ascend the Fly River on another voyage of discovery. It seems that Sir William and his party, in a steam launch, dropped anchor in the river on December 14. The

launch stranded, and fifteen canoes, carrying about 150 natives, bore down upon the explorers and commenced a savage attack. The Governor's party opened fire, and the natives promptly beat a retreat. After about half an hour, however, they returned, bringing a pig as a peace offering. Sir William consequently went 180 miles further up the river, and on his return visited the same people again, to find them quite peaceably inclined. The Governor started again on December 26 to explore higher up the Fly River.

THE Survey Department of Burmah has in preparation a new map containing all the latest information derived from the parties sent out by the Department. A preliminary issue omitting all the mountain ranges has recently been published.

SIGNOR G. B. SACCHIERO, Italian Consul at Rangoon, sends to the *Bollettino* of the Italian Geographical Society for December an interesting notice of the savage Chin tribes who occupy the hilly region in the north of Burma about the headwaters of the Irawady. The collective tribal name is variously written Chin, Kyen, Kiyin, Kachin, Kakyen, &c.; but they call themselves Sihù, and according to Signor Sacchiero they evidently belong to the Burmese branch of the Mongol stock. In the districts brought under British rule many have already adopted the Burmese dress, and these can with difficulty be distinguished from the Burmese themselves. But the language is more allied to that of the widespread Karen race, and the Karen alphabet composed by the American missionaries in Lower Burma is well suited for expressing the sounds of the Chin idiom. The Chins themselves have no knowledge of letters; nor have they made any progress beyond the rudest state of social culture. They still go nearly naked, and the women on arriving at the age of puberty are tattooed all over the face with a black pigment, being thus disfigured for life, either to prevent the Burmese or the neighbouring tribes from kidnapping them, or else to distinguish them from the women captured by the Chins from the surrounding peoples. They marry early, the bride requiring the consent, not of her parents, but of an elder brother, and the husband promising not to beat her too much, nor to cut her hair if she behaves well. The family yields obedience to the father alone, who recognizes no authority except that of the village chief, this authority passing in both cases to the youngest son. The men always carry firearms, and make their own gunpowder, using instead of sulphur a seed called *aunglak*, first roasted, and then pounded up with charcoal and saltpetre, three parts of the two first to twenty of the last, and mixing the whole with alcohol, or tobacco juice. Both sexes smoke little Indian hookahs, and their favourite drink is *khaung*, a kind of beer extracted from fermented rice. They live mainly by the chase, and when a boar, stag, or other big game is captured, there are great rejoicings in the village. The quarry is covered from neck to tail in a red cloth, and presented to the "temple," or abode of the *nat* (spirit); then the "friend of the *nat*" (priest) pronounces a blessing on the successful hunter, after which all join in the feast, with much tamtaming, shouting, drinking, and dancing through the village. When they descend to the plains, the Chins are Buddhists, but in their villages spirit-worshippers. Not only every village and every district, but every person has his special *nat*, mostly a malevolent being who requires to be pacified by propitiatory offerings. The vendetta is a universal institution, feuds being inherited from family to family, from tribe to tribe, and thus leading to constant bloodshed. If a man is drowned, his son reeks vengeance on the water where he perished by piercing it with spears or slashing it about with long knives. Many of the Chins have already tendered their submission to the British authorities, and arrangements are now in progress for extending orderly government over the whole territory.

ON SOME NEEDLESS DIFFICULTIES IN THE STUDY OF NATURAL HISTORY.[1]

A LITTLE while ago I read, in the preface to a work on natural history, that the book was "of little value to the scientific reader, but that its various anecdotes, and its minute detail of observation would be found useful and entertaining."

What, then, may the "scientific reader" be expected to desire? He must be, in my opinion, a most unreasonable man, if he does not thankfully welcome anecdotes of the creatures he wishes to study, when these anecdotes are the result of patient and accurate observation. For it is precisely such information, that is conspicuously absent from many scientific memoirs and monographs; the author generally spending his main space and strength in examining the shape and structure of his animals, and in comparing one with another, but giving the most meagre details of their lives and habits.

Which, then, is the more scientific treatment of a group of animals—that which catalogues, classifies, measures, weighs, counts, and dissects, or that which simply observes and relates? Or, to put it in another way, which is the better thing to do—to treat the animal as a dead specimen, or as a living one?

Merely to state the question is to answer it. It is the living animal that is so intensely interesting, and the main use of the indexing, classifying, measuring, and counting is to enable us to recognize it when alive, and to help us to understand its perplexing actions.

But, it may be objected, that because the study of the living animal is the more interesting, it is not necessarily the more scientific; indeed, that the amount of entertainment, which we may get out of the pursuit of natural history, has nothing to do with the question at all; that by science we mean accurate knowledge presented in the most suitable form; that shape, structure, number, weight, comparison are the fundamental notions, with which sciences of every kind have to deal; and that scientific natural history is more properly that which takes cognizance of a creature's size, form, bodily organs, and relation to other creatures, than that which concerns itself with the animal's disposition and habits.

I can fancy that I already hear some of my audience say: "But why set up any antagonism between these two ways of studying a creature? Both are necessary to its thorough comprehension, and our text-books should contain information of both kinds; we should be told how an animal is made, where it ought to be placed among others of the same group, and also how it lives, and what are its ways."

Precisely; that is just what memoirs and text-books ought to do; but what, too often, they do *not*. We read much of the animal's organs; we see plates showing that its bristles have been counted, and its muscular fibres traced to the last thread; we have the structure of its tissues analyzed to their very elements; we have long discussions on its title to rank with this group or that; and sometimes even disquisitions on the probable form and habits of some extremely remote, but quite hypothetical ancestor—some "archirotator" (to take an instance from my own subject) who is made to degrade in this way, or to advance in that, or who is credited with one organ, or deprived of another, just as the ever-varying necessities of a desperate hypothesis require:—but of the living creature itself, of the way it lives, of the craft with which it secures its prey or outwits its enemies, of the home that it constructs, of its charming confidence or its diabolical temper, of its curious courtship, its droll tricks, its games of play, its fun and spite, of its perplexing stupidity coupled with actions of almost human sagacity—of all this, this which is the real natural history of the animal, we, too often, hear little or nothing. And the reason is obvious, for in many cases the writer has no such information to give; and, even when he has, he is compelled by fashion to give so much space to that which is considered to be the more scientific portion of his subject, that he has scant room for the more interesting. Neither ought we to be surprised if a writer is "gravelled for the lack of matter," when he comes to speak of an animal's life; for the study of the lives of a large majority is a difficult one. It requires not only abundant leisure, but superabundant patience, a residence favourably situated for the pursuit, and an equally favourable condition of things at home. The student, too, must be ready to adopt the inconvenient hours of the creatures that he watches, and be indifferent to the criticisms of those that watch *him*. If his enthusiasm will not carry him, without concern, through dark nights, early mornings, vile weather, fatiguing distances, and caustic chaff, the root of the matter is not in him. Besides, he ought to have a natural aptitude for the pursuit, and know how to look for what he wants to see; or if he does not know, to be able to make a shrewd guess: and, above all, when circumstances are not favourable, to have wit enough to invent some means of making them so. And yet when the place, the man, the animals, and the circumstances all seem to promise a rich harvest of observations, how often it happens that some luckless accident, a snapt twig, a

[1] The Presidential Address to the Royal Microscopical Society, at the annual meeting, on February 12, 1890, by Dr. C. T. Hudson, F.R.S.

lost glass, a hovering kestrel, a sudden gust of wind, a roving dog, or a summer shower, robs the unlucky naturalist of his due; nay, it sometimes happens that, startled by some rare sight, or lost in admiration of it, he himself lets the happy moment slip, and is obliged to be contented with a sketch from memory, when he might have had one from life.

But I have not yet got to the bottom of my budget—the heaviest trouble still remains; and that is, that the result of a day's watching will often go into a few lines, or even into a few words; and so it happens, that the writer of the history of a natural group of animals is too frequently driven to fill up his space with minute analysis of structure, discussions on classification, disputes on the use of obscure organs, or descriptions of trifling varieties; which, exalted to the rank of species, fill his pages with wearisome repetitions; for were he, before he writes his book, to endeavour to make himself acquainted with the habits of all the creatures he describes, his own life-time might be spent in the pursuit.

We will now take a different case, and suppose that many years have been spent in the constant and successful study of the animals themselves; and that the time has come, when the naturalist may write his book, with the hope of treating, with due consideration, the most interesting portion of his subject. He is now beset with a new class of difficulties, and finds that publishers and scientific fashion alike, combine to drive him into the old groove: for the former limit his space, by naturally demurring to a constantly increasing number of plates and an ever lengthening text; while the latter insists so strongly on having a complete record of the structure, and points of difference, of every species, however insignificant, that it is hardly possible to do much more than give that record—a mere dry shuck, emptied of nearly all that makes natural history delightful.

And so we come round again to the point that I have already glanced at, viz. "Ought natural history to be delightful?"

Ought it to be delightful! Say, rather, ought it to exist? What title has the greater part of natural history to any existence but that it charms us? It is true that this study may help—does help many—to worthier conceptions of the unseen, to loftier hopes, to higher praise; that it gives us broader and sounder notions of the possible relation of animals, not only to one another, but also to ourselves; that it provides us with the material for fascinating speculations on the embryology of our passions and mental powers; and that it may even serve to suggest theories of the commencement and end of things, of matter, of life, of mind, and of consciousness—grave questions, scarcely to be dealt with successfully by human faculties, but in a condition to be discussed with infinite relish.

When I speak, then, of the pleasure we derive from the study of natural history, I include these graver and higher pleasures in the word.

Here and there, too, no doubt, the knowledge of the powers and habits of animals is materially useful to us; and, indeed, in the case of some of the minuter organisms, may be of terrible importance; but, in that of the large majority of creatures, we might go out of the world unconscious of their existence (as, indeed, very many people do), and yet, unlike the little jackdaw, not be "a penny the worse." For what is a man the better for studying butterflies, unless he is delighted with their beauty, their structure, and their transformations? Why should he learn anything about wasps and ants, unless their ways give him a thrill of pleasure? What can the living plumes of the rock-zoophytes do for us, but 'witch our eyes with their loveliness, or entrance us with the sight of their tiny fleets of medusa-buds, watery ghostlets, flitting away, laden with the fate of future generations?

When, at dusk, we steal into the woods to hear the nightingale, or watch the night-jar, what more do we hope for than to delight our ears with the notes of the one, or our eyes with the flight of the other? When the microscope dazzles us with the sight of a world, whose inhabitants and their doings surpass the wildest flights of nightmare or fairy tale, do we speculate on what possible service this strange creation may render us? Do we give a thought to the ponderous polysyllables that these mites bear in our upper world, or to their formal marshalling into ranks and companies, which are ever being pulled to pieces, to be again re-arranged? No! it is the living creature itself which chains us to the magic tube. For there we see that the dream of worlds peopled with unimagined forms of life—with sentient beings whose ways are a mystery, and whose thoughts we cannot even guess at—is a reality that lies at our very feet; that the air we breathe, the dust that plagues our nostrils, the water we fear to drink, teem with forms more amazing than any with which our fancy has peopled the distant stars; and that the actions of some of the humblest arouse in us the bewildering suspicion, that, even in these invisible specks, there is a faint foreboding of our own dual nature.

If, then, we make some few exceptions, we are entitled to say that the study of natural history depends for its existence on the pleasure that it gives, and the curiosity that it excites and gratifies: and yet, if this be so, see how cruelly we often treat it. Round its fair domain we try to draw a triple rampart of uncouth words, elaborate, yet ever-changing classifications, and exasperatingly minute subdivisions; and we place these difficulties in the path of those whose advantages are the least, those who have neither the vigorous tastes that enable them to clear such obstacles at a bound, nor the homes whose fortunate position enables them to slip round them. For modern town life forces a constantly increasing number of students to take their natural history from books; and too often these are either expensive volumes beyond their reach, or dismal abridgments, which have shrunk, under examination pressure, till they are little else than a stony compound of the newest classification and the oldest woodcuts.

But the happier country lad wanders among fields and hedges, by moor and river, sea-washed cliff and shore, learning zoology as he learnt his native tongue, not in paradigms and rules, but from Mother Nature's own lips. He knows the birds by their flight, and (still rarer accomplishment) by their cries. He has never heard of the *Œdicnemus crepitans*, the *Charadrius pluvialis*, or the *Squatarola cinerea*, but he can find a plover's nest, and has seen the young brown peewits peering at him from behind their protecting clods. He has watched the cunning flycatcher leaving her obvious, and yet invisible young, in a hole in an old wall, while it carried off the pellets that might have betrayed their presence; and has stood so still to see the male redstart, that a field-mouse has curled itself up on his warm foot and gone to sleep. He gathers the delicate buds of the wild rose, happily ignorant of the forty-odd names under which that luckless plant has been smothered; and if, perchance, his last birthday has been made memorable by the gift of a microscope, before long he will be glorying in the transparent beauties of *Asplanchna*, unaware that he ought to crush his living prize, in order to find out which of some half-dozen equally barbarous names he ought to give it.

The faults, indeed, of scientific names are so glaring, and the subject is altogether so hopeless, that I will not waste either your time or my patience by dilating on it. But, while admitting that distinct creatures must have different names, and very reluctantly admitting that it seems almost impossible to alter the present fashion of giving them, I see no reason why these, as well as the technical names of parts and organs, should not be kept as much as possible in the background, and not suffered to bristle so in every page, that we might almost say with Job, "There are thistles growing instead of wheat, and cockle instead of barley."

We laughed at the droll parody in which the word *change* was defined as "a perichoretical synechy of pamparallagmatic and porroteroporeumatical differentiations and integrations," yet it would not be a difficult matter to point out sentences, in recent works on our favourite pursuits, that would suggest a similar travesty. No doubt, new notions must often be clothed in new language, and the severer studies of embryology and development require a minute precision of statement that leads to the invention of a multitude of new terms. Moreover, the idea that the meaning of these terms should be contained in the names themselves is excellent; but I cannot say that the result is happy—I might almost say that it is repulsive; and if we suffer this language to invade the more popular side of natural history, I fear that we shall only write for one another, and that our scientific treatises will run the risk of being looked at only for their plates, and of being then bound up with the Russian and Hungarian memoirs.

The multiplication of species, too, is a crying evil, and the exasperating alterations of their names, in consequence of changing classifications, is another. The former, of course, is mainly due to the difficulty, no doubt a very great one, of determining what shall be a species, and what a variety. How widely experts may differ on this question, Darwin has shown by pointing out that, excluding several polymorphic genera and many trifling varieties, nearly two hundred British species, which are generally considered varieties, have all been ranked by

botanists as species; and that one expert has made no fewer than thirty-seven species of one set of forms, which another arranges in three. Besides, even in the cases where successive naturalists have agreed in separating certain forms, and in considering them true species, it happens now and then, as it did to myself, that a chance discovery throws down the barriers, and unites half-a-dozen species into one.

Under these circumstances one would have expected that the tendency would have been to be chary of making new species, and no doubt this is the practice of the more experienced naturalists; but, among the less experienced, there is a bias in the opposite direction; and all of us, I fear, are liable to this bias when we have found something new; for, even if it is somewhat insignificant, we are inclined to say with Touchstone, "A poor thing, sir, but mine own!" Now, were this fault mended, much would be avoided that tends to make monographs both expensive and dull; for, though the needs of science require a minute record of the varieties of form, which are sometimes of high importance from their bearing on scientific theories, yet the description of them, as varieties, may often be dismissed in a line or two, when nothing further is set forth than their points of difference; whereas, if these forms are raised to the rank of species, they are treated with all the spaced-out dignities of titles, lists of synonyms, specific characters, &c., &c., and so take up a great deal of valuable room, weary the student with repetitions, and divert his attention from the typical forms.

But when everything has been done that seems desirable, when names and classification have been made both simple and stable, and the number of species reduced to a minimum, there will still remain the difficulty that monographs must, from the nature of the case, generally be grave, as well as expensive books of reference, rather than pleasant, readable books, within the reach of the majority. I would suggest then, that, if it be possible, each group of animals should be described not only by an all-embracing monograph, to be kept for reference on the shelves of societies like our own, but by a book that would deal only with a moderate number of typical, or very striking forms; that would describe them fully, illustrate them liberally from life, and give an ample account of their lives and habits.

Such a book should give as little of the classification as possible; it should avoid the use of technical terms, and above all, it should be written with the earnest desire of so interesting the reader in the subject, that he should fling it aside, and rush off to find the animals themselves. By this means we should not only get that active army of out-of-door observers, which science so greatly needs; but, by bringing the account of each group into a reasonable compass, we should enable students of natural history to get a fair knowledge of many subjects, and so greatly widen their ideas and multiply their pleasures.

For why should we be content to read only one or two chapters of Nature's book? To be interested in many things—I had almost said in everything—and thus to have unfailing agreeable occupation for our leisure hours, is no bad receipt for happiness. But life is short, and its duties leave scant time for such pursuits; so that to acquire a specialist's knowledge of one subject would often be to exchange the choice things of many subjects for the uninteresting things of one. And how uninteresting many of them are! How is it possible for any human being to take pleasure in being able to distinguish between a dozen similar creatures, that differ from one another in some trifling matter; that have a spike or two more or less on their backs, or a varying number of undulations in the curve of their jaws, or differently set clumps of bristles on their foreheads? Why should we waste our time, and our thoughts, on such matters? The specialist, unfortunately, must know these things, as well as a hundred others equally painful to acquire and to retain, and no doubt he has his reward; but that reward is not the deep delight that is to be found in the varied study of the humbler animals; of those beings "whom we do but see, and as little know their state, or can describe their interests or their destiny, as we can tell of the inhabitants of the sun and moon; . . creatures who are as much strangers to us, as mysterious, as if they were the fabulous, unearthly beings, more powerful than man, yet his slaves, which Eastern superstitions have invented."

Those, then, who are blest with a love of natural history should never dull their keen appreciation of the wonders and beauties of living things, by studying minute specific differences; or by undertaking the uninteresting office of finding and recording animals, that may indeed be rare, but which differ from those already known in points, whose importance is due solely to arbitrary rules of classification.

This eagerness, to find something new, errs not only in wasting time and thought on matters essentially trivial and dull, but in neglecting things of the greatest interest, which are always and everywhere within reach. Take, for instance, the case of *Melicerta ringens*. What is more common, what more lovely, than this well-known creature? And yet how much there remains to be found out about it. No one, for example, has ever had the patience to watch the animal from its birth to its death; to find out its ordinary length of life, the time that it takes to reach its full growth, the period that elapses between its full growth and death, or, indeed, if there be such a period. And yet even these are points which are well worth the settling. For, if *Melicerta* reaches its full growth any considerable time before the termination of its life, it would seem probable that, owing to the constant action of its cilia, it would either raise its tube far above the level of its head, or else be constantly engaged in the absurd performance of making its pellets and then throwing them away. Who has ever found it in such a condition, or seen it so engaged? yet the uninterrupted action of the pellet cup would turn out the six thousand pellets, which form the largest tube that I am acquainted with, in about eight days, and those of an average tube in less than three; while the animal will live (according to Mr. J. Hood)[1] nearly three months in a zoophyte trough, and no doubt much longer in its natural condition. It is true that the creature's industry in tube-making is not continuous. It is often shut up inside its tube, when all ciliary action ceases; and, moreover, when expanded, it may be seen at times to allow the formed pellet to drift away, instead of depositing it; but, allowing for this, there is no little difficulty in understanding how it is that, with so vigorous a piece of mechanism as the pellet-cup, the tube at all ages, except the earliest, so exactly fits the animal. I am aware that it has been stated that the whole of the cilia (including those of the pellet-cup) are under the animal's control, and that their action can be stopped, or even reversed, at pleasure. But this, I think, is an error. Illusory appearances, like those of a turning cog-wheel, may be produced by viewing the ciliary wreath from certain points, and under certain conditions of illumination; and these apparent motions are often reversed, or even stopped, by a slight alteration either in the position of the animal, in the direction of the light, or in the focussing of the objective. When, however, under any circumstances, the cilia themselves are distinctly seen, they are invariably found to be simply moving up and down; now turning sharply towards their base, and now recovering their erect position. Even the undoubtedly real reversal of the revolution of the pellet in its cup, which is constantly taking place, can be easily explained by purely mechanical considerations, and consistently with the continuous up and down motion of the cilia. Moreover, of the actual stoppage of the cilia, in the expanded Rotiferon, I have never seen a single instance. In all cases, on the slightest opening of the corona, the cilia begin to quiver, and they are always in full action, even before the disk is quite expanded; while, should a portion of the coronal disk chance to be torn away, its cilia will continue to beat for some time after its severance: so that there is good reason for believing, that the ciliary action is beyond the animal's control.

It is possible, indeed, that *Melicerta* may continue to grow (as Mr. Hood says that the Floscules appear to do) as long as it lives; or it may adopt the plan of some species of *Œcistes*, which, to prevent themselves from being hampered by their ever-growing tubes, quit their original station at the bottom of the tube, and attach themselves to it above, creeping gradually upwards as the tube lengthens. At any rate it would be interesting and instructive to watch the growth of a *Melicerta*, and the building of its tube, from the animal's birth to its death. An aquarium, in which *Melicerta* would live healthily and breed freely, could easily be contrived, and a little ingenuity would enable the observer to remove any selected individual to a zoophyte trough and back again, without injury; and his trouble perhaps would be further repaid by such a sight as once delighted my eyes at Clifton, where I picked, from one of the tanks of the Zoological Gardens, some *Vallisneria*, whose ribbon-like leaves were literally furred with the yellow-brown tubes of

[1] Mr. Hood, of Dundee, has kept in his troughs *Melicerta ringens* for 79 days, *Limnias ceratophylli* for 83 days, *Cephalosiphon limnias* for 89 days: the *Floscularia* usually lived about 50 days; but *F. Hoodii* died, before maturity, in 16 days.

Melicerta. I coiled one of these round the wall of a deep cell, and thus brought into the field of view, at once, more than a hundred living *Melicertæ* of all ages and sizes, and all with their wheels in vigorous action; a display never to be forgotten.

Such a tank, so stocked and managed, would probably enable a patient and ingenious observer to decide several other points, about which we are, at present, in ignorance: to say whether the same individual always lays eggs of the same kind, or whether it may lay now female eggs, now male, now ephippial eggs; and to say what determines the kind of egg that is to be laid; whether it is the age of the individual, or the supply of food, or temperature, or sexual intercourse that is the potent cause.

It would, too, hardly be possible for the male, to escape the observation of a naturalist, who possessed a tank in which were hundreds of *Melicertæ*: and the male is as yet almost unknown.

Judge Bedwell found in the tubes of the female, in winter, a small Rotiferon resembling the supposed male, that I had seen playing about *M. tubularia*; only the former had a forked foot, and sharp jaws that were at times protruded beyond the coronal disc. Its frequent occurrence in the tubes in various stages of development, and the nonchalance with which the female suffered it to nibble at her ciliary wreath, inclined the observer to conclude, that the animal was the long sought-for male. Unfortunately it was only observed when in motion, so that its internal structure was not made out; and the matter therefore still rests in some doubt.

No doubt it is a strong argument that the female would probably suffer nothing but a male to take such liberties with her; but it would seem, from the following account, that it is possible for such freedoms to be pushed too far.

Mr. W. Dingwall, of Dundee, was on one occasion watching a male Floscule circling giddily round a female, and constantly annoying her by swimming into her fully expanded coronal cup. Again and again she darted back into her tube, only to find her troublesome wooer blocking up her cup, and sadly interfering with, what to a Floscule is, the very serious business of eating—for these animals will often eat more than their own bulk in a few hours. It was clear at last that the lady would not tolerate this persistent interference with her dinner; for when—after waiting, rather a longer time than usual, closed up in her tube—she once more expanded, only to find him once more in his old position, she lost all patience, and effectually put an end to his absurdities, by giving one monstrous gulp, and swallowing her lover. It will not surprise you to hear that he did not agree with her, and that after a short time she gave up all hope of digesting her mate, and shot him out into the open again, along with the entire contents of her crop. He fell a shapeless, motionless lump; the two score and ten minutes of a male Rotiferon's life cut short to five; but, strange to say, in a second or two, first one or two cilia gave a flicker, then a dozen; then its body began to unwrinkle and to plump up; and, at last, the whole corona gave a gay whirl, and the male shot off as vigorous as ever, but no doubt thoroughly cured of its first attachment.

I have taken *Melicerta ringens*, as an example of what yet remains to be done, even with an animal which is as common in a ditch, as a fly is in a house; but almost every other Rotiferon would have done equally well, for there is scarcely a single species, whose life-history has been thoroughly worked out.

To me, natural history in many of its branches seems to resemble a series of old, rich mines, that have been just scratched at by our remote ancestors, and then deserted. Our predecessors did their best with such feeble apparatus as they had; it was not much, perhaps, but it was wonderful that they did it at all with no better appliances; and it irks me to think that we, who are equipped in a way which they could not even dream of, should turn our backs on the treasures lying at our feet, and go off prospecting in new spots, contented too often with a poor result, merely because it is from a new quarter.

Besides, the love of novelty is a force too valuable to be wasted on a mere hunt for new species in any one group of animals, especially unimportant ones. It should rather be used to make us acquainted with the more striking forms of many groups. Let us have no fear of the reproach of superficial knowledge; everyone's knowledge is superficial about almost everything; and even in the case of those few who have thoroughly mastered some one subject, their knowledge of that must have been superficial for a great portion of their time. Indeed, the taunt is absurd. I can imagine that a superficial knowledge of law, or surgery, or navigation may bring a man into trouble; but what possible harm can it do himself, or anyone else, that he is content with knowing five Rotifera instead of five hundred? And yet if any naturalist were to study only *Floscularia, Philodina, Copeus, Brachionus*, and *Pedalion*, it would give him the greatest possible pleasure, as well as an excellent general notion of the whole class. Let any tyro at the seaside watch the ways and growth of a *Plumularia*, or of a rosy feather-star, his knowledge of the groups to which they belong could certainly not be dignified even with the term "superficial"—"linear" or "punctiform" would be more appropriate; but the pleasure, that he would derive from such a study, could not be gauged by counting the number of animals that he had examined. It would depend on the man himself; and might, I should readily imagine, far exceed that derived by the study of a hundred times the number of forms in books; especially when such a study had been undertaken, not from a natural delight in it, but from some irrelevant reason, such as to support a theory, to criticize an opponent, to earn a distinction, or to pass an examination.

In truth that knowledge of any group of animals, which would rightly be called superficial when contrasted with the knowledge of an expert, is often sufficient to give us a satisfactory acquaintance with the most interesting creatures in it; to make us familiar with processes of growth and reproduction too marvellous to be imagined by the wildest fancy; and to unfold to us the lives of creatures who, while possessing bodily frames so unlike our own that we are sometimes at a loss to explain the functions of their parts, yet startle us by a display of emotions and mental glimmerings, that raise a score of disquieting questions.

Moreover, there is another excellent reason why we should not confine our attention to one subject; and that is, that even the most ardent naturalist must weary at times of his special pursuit. Variety is the very salt of life; we all crave for it, and in natural history, at all events, we can easily gratify the craving. If we are tired of ponds and ditches, there are the rock-pools of our south-western shores, and the surface of our autumn seas. A root of oar-weed torn at random from a rocky ledge, an old whelk shell from deep water, a rough stone from low-water mark, the rubbish of the dredge,—each and all will afford us delightful amusement. It is wonderful, too, what prizes lurk in humble things, and how often these fall to beginners. The very first time that I tried skimming the sea with a muslin net, I picked a piece of green seaweed off the muslin, intending to throw it away; but, seeing a little brown spot on it, I dropped the weed (not a square inch) into a bottle of sea-water, instead. At once the brown speck started off and darted wildly round the bottle. It was too small to be made out with the naked eye, but by the time I had brought my lens to bear, it had vanished. I hunted all over the bottle, and could see nothing, neither with the lens nor without it. I was half inclined to throw away the water; but, as I was certain that I had seen something in it two minutes before, I corked up the bottle and took it home. When I next looked at it, there was the little brown creature flying about as wildly as ever. I soon made out, now, that I had caught a very tiny cephalopod—something like an octopus—and with a pipette I fished it out, and dropped it into a glass cell. At least I dropped the water from the pipette into the cell; but the animal itself had vanished again; I could not see it either in the bottle or the cell. I was not going to be tricked again; so I pushed the cell under the microscope, and there was my prize; motionless, but for its panting; and watching me, as it were, up the microscope with its big blue-green eyes. It was almost colourless, and was dotted at wide intervals with very minute black spots, set quincunx fashion—spots absolutely invisible to the sharpest unaided sight.

As I looked it began to blush—to blush faint orange, then deeper orange, then orange-brown; a patch of colour here, another there, now running across one side of the body, now fading away, again to appear on a tentacle; till at last, as it recovered from its alarm, each black spot began to quiver with rapid expansions and contractions, and then to spread out in ever varying tints, till its wavering outlines had met the expansions of its neighbouring spots; and the little creature, regaining its colour and its courage at the same moment, rushed off once more in a headlong course round the cell.

I was the merest beginner when I saw this, but I had the good luck, knowing nothing whatever about it, and never having given the subject a thought, to see, with my own eyes, how effectually cuttlefishes are protected by their loss of colour, and also to see how the loss takes place.

No doubt the sea-side of our south-western coasts—I mean its creeks, not "the thundering shores of Bude and Bos"—is a paradise for microscopists; but there is no need that we should travel so far afield. Our inland woods, our lanes and pastures, will yield to us a thousand beauties and wonders. The scarlet pimpernel will show its glorious stamens, the flowers of the wound-wort glow like a costly exotic; wild mignonette will rival in its fantastic shape the strangest orchid; the humblest grass will lift a tuft of glistening crystals; the birch and salad-burnet shake out their crimson tassels; the Jungermanns will display their mimic volcanoes, the mosses unfold the delicate lacework of their dainty urns. But the time would fail me to name one tithe of those sources of wonder and delight that lie all around us; and most of which, as in the case of the Rotifera, contain numberless points on which we are all happily ignorant, and therefore in the best of all possible conditions for deriving endless pleasure and instruction from them. Besides, my time and your patience must, I think, be drawing to a close; I would then only once more suggest, that we should not only explore for ourselves all these "pastures new"—no matter how imperfectly—but that we should encourage those, who can be our most efficient guides, to indulge us with the main results in the simplest language. Surely one of the most charming subjects, that can interest human beings, admits of being so treated; and there can be no good reason why the Muse of Natural History (for no doubt there is such a Muse) should resemble that curious nymph among the *Oribatidæ*, whom Mr. Michell found lying under the moss of an old tree, half smothered in a heap of her cast-off skins, admirable types of successive classifications, and abandoned nomenclature.

Happily, however, books in such matters are of little importance; and names and classifications of still less: both these latter, indeed, are of ephemeral interest; they are the pride of to-day, and the reproach of to-morrow. It is to the living animals themselves that we must turn, fascinated not only with their beauty and their actions, but with the questions which the contemplation of them perpetually provokes, and very rarely answers.

For, in the long procession of the humbler creatures, who can tell where life first develops into consciousness, and why it does so; where consciousness first stretches beyond the present so as to include the past, and why that happens; or at what point, and why, memory and consciousness themselves are lighted up by the first faint flashes of reason?

We know nothing now of such matters, and probably we never shall know much; but the mere fact that the study of natural history irresistibly draws us to the consideration of these questions, gives to her pleasant features an undoubted dignity, and raises the charming companion of our leisure hours to the rank of an intimate sharer of some of our gravest thoughts.

THE TOTAL ECLIPSE.

THE U.S.S. *Pensacola* arrived at Saint Paul de Loanda on December 6, after a voyage of 51 days from New York, having made the ports of Horta, Fayal, in the Azores, November 2–3; of Saint Vincent, in the Cape Verdes, November 10–12; of Saint George's Parish, Sierra Leone, November 18–20; and of Elmina, on the Gold Coast, November 26–28.

Immediately on landing at Loanda, it was found that the Rio Quanza steamer, sailing bi-weekly for Muxima, had left two days previously, and that recent washouts along the line of the Caminho de Ferro Trans-Africano made it impracticable for the Expedition to reach either Muxima or Cunga early enough to allow sufficient time for mounting and adjusting the instruments for the eclipse.

I therefore at once decided to locate the Expedition at or near Cape Ledo. Mention should be made here of the courteous civilities of His Excellency the Governor of Loanda, for his kindly interest in the Expedition, and the facilities he offered for the prosecution of the various fields of its work.

The *Pensacola* came to anchor alongside H.M.S. *Bramble* in the little bay to the north of Cape Ledo, on the afternoon of Sunday, December 8. The Eclipse Station was selected in a very favourable spot close to the shore cliffs, and the sites of the principal instruments were determined before night.

A week or ten days' hard work sufficed for getting a large amount of the apparatus in readiness for the eclipse. I placed Prof. Bigelow in charge of the direct photoheliograph of nearly 40 feet focal length, and detailed Mr. Davis to assist him. Mr. Jacoby was intrusted with the charge of the time-determinations, and longitude and latitude work. The *Bramble* was at Cape Ledo on a mission like that of the *Pensacola*, and attending upon the English Eclipse Expedition in charge of Mr. A. Taylor, F.R.A.S.; and through the courtesy of her commanding officer, Captain Langdon, R.N., advantage was taken of her run to St. Paul de Loanda and return, December 14–17, to make a chronometric determination of the longitude, by comparison with the time at Loanda as determined by Mr. Preston, who was left there by the Expedition for the gravity and magnetic work. Also, on the *Bramble's* second return to Loanda, on December 23, another comparison was made.

Prof. Abbe was in charge of the meteorological work and of the organization of parties of observers from the ship's company. A large amount of valuable material results from his work.

The mounting and adjustment of the extensive apparatus for the total eclipse, I reserved for myself. A duplex polar axis eleven feet in length had been constructed of six-inch iron tubings, and mounted with great stability. This axis was driven by powerful clock-work of extreme precision, made by Mr. Saegmueller, of Washington. On this single axis was mounted the totality-battery, consisting of 2 Brashear reflecting telescopes of 8 inches diameter, four Clark telescopes of $3\frac{1}{2}$, 5, $7\frac{1}{2}$, and 8 inches aperture, the second being rigged with an eyepiece enlarging the sun's image to a diameter of $4\frac{1}{2}$ inches, the third being used as a high power directing telescope, while the fourth, a photographic doublet with 10 inch back lens, loaned by the Harvard College Observatory, was arranged for a series of twelve exposures, two of which were made through an orthochromatizing screen provided by Mr. Carbutt; two six-inch Dallmeyer rapid rectilinear lenses of 24 and 38 inches focus; one Schroeder triple objective, of 6 inches aperture and 22 inches focus; one Gundlach orthoscope of 3 inches aperture and 21 inches focus; two flint spectroscopes and one quartz spectroscope loaned by Harvard College Observatory; a duplex photometer of 75 inches focus also provided by Prof. Pickering, and his reversing layer spectroscope for photographing a spectrum trail for fifteen seconds both before and after second and third contacts; a 5 inch Ross lens of 42 inch focus; a 4 inch Spencer objective of 36 inch focus, and a 6·4 inch Merz-Clark objective, both rigged with the means of automatic variation of aperture during totality; and lastly, two duplex cameras provided by Dr. Wright of the Sloane Laboratory of Yale University, for photographic record of the polarization of the corona. In all there were 23 objectives and two mirrors, with their axes adjusted into parallelism.

With the exception of the Gundlach camera, which was reserved for a special investigation of the extreme outer corona, all this apparatus was operated automatically, by an adaptation of the pneumatic organ-valve system of Mr. Merritt Gally, of New York. Exposing shutters were opened and closed, sensitised plates were exchanged for others as soon as exposed, and all the mechanical movements were accomplished with entire precision. Also, by employing an ordinary chronograph in conjunction with the valve system, the exact time of beginning and end of each exposure became a matter of accurate record.

All this apparatus was brought into operation during the period of total eclipse, and over 300 exposures were made in a period of 3m. 10sec.; but no photographs of the corona were secured, as the sun was completely obscured by clouds. However, the entire success of the pneumatic movements is a result of no little value in view of eclipse work in the future.

In addition to this, a silver-on-glass mirror, of 20 inch diameter and 75 feet focal length, by Brashear, lent to the Expedition by Prof. Langley, was so mounted as to throw an image of the corona up the cliff and just underneath the sun at the time of totality. At the focus a beautiful 10 inch image of the sun was formed, and 20 × 24 inch plates of the highest sensitiveness were in readiness to record the coronal streamers. This unusual apparatus was also rendered inoperative by clouds.

With the direct photoheliograph, however, very gratifying success was secured. Seventy pictures of the partial phases were made before totality, and forty after. The serious obstacles to the operation of so long a tube were successfully overcome by means of a skeleton mounting, a combined form of an equatorial stand and tripod; and Prof. Bigelow's sand-clock enabled the precise and easy following of the sun. The revolving plate-holder, of 22 inches diameter, actuated automatically by compressed air, in which the principles of the apparatus of the

National Electric Service Company were employed, was a thorough success. Exposures were made at intervals of six seconds.

A few hours before the eclipse came on, the *Pensacola* went out to sea, and stood in the centre of the eclipse-track at the time of totality. Atmospheric conditions were slightly more favourable there than at the main station of the Expedition, and some interesting results were obtained. During totality, however, the clouds were so thick that it is very doubtful whether the true solar corona was seen at all.

The Eclipse Station was completely dismantled by December, 27, and the *Pensacola* left Cape Ledo on the afternoon of the same day.

Returning to Loanda, it was found that two of the three detached parties of the Expedition sent into the interior to observe the eclipse were unsuccessful on account of clouds. The third has not yet been heard from.

DAVID P. TODD.

U.S.S. *Pensacola*, December 31, 1889.

SCIENTIFIC SERIALS.

Rendiconti del Reale Istituto Lombardo, December.—Results obtained from Dr. L. Weigert's therapeutic treatment of pulmonary phthisis, by Prof. A. Visconti. Seven patients in various stages of consumption have been subjected to this treatment for the purpose of testing its efficacy. It consists in administering superheated dry air (150° to 180° C.), which is inhaled through a specially prepared apparatus, for which Dr. Weigert claims that it acts directly on Koch's bacillus of tuberculosis. In the incipient stages of the disease satisfactory results were obtained in some respects, such as relief of the cough, greater freedom of respiration, less profuse perspiration, and increased appetite. But it was doubtful whether the germ itself was killed, while in the advanced stages the malady continued its normal development without being perceptibly arrested by the treatment. Without actually condemning Weigert's method, Prof. Visconti cannot at present regard it as an efficacious remedy against phthisis.—On the determination of the coefficient of dynamic and electromotor produce, by P. Guzzi. The author here describes a method of determining this coefficient, for which he claims certain advantages over that proposed by Dr. J. Hopkinson in the *Electrician* of December 3, 1886, especially in the case of engines of over 100 horse-power. His method of calculating the yield of the dynamo and electric motors is based exclusively on electric measurements made with safer and more handy instruments than Hopkinson's dynamometers. Two dynamos of about the same type and dimensions are connected together in such a way that one moves the other as motor, as in the Hopkinson apparatus. But instead of communicating to the system the *dynamic energy* required to maintain it in motion with the velocity and intensity of the normal current, Guzzi's instrument communicates the equivalent *electric energy* derived from any external source whatsoever.

Rivista Scientifico-Industriale, December 31, 1889.—Researches on the absorption of hydrogen by iron, and on the tenacity of certain metals after absorbing gases, by Prof. M. Bellati and S. Lussana. It has already been shown by Hughes (NATURE, vol. xxi., 1880, p. 602) that steel and iron immersed in diluted sulphuric acid become very brittle, and that the same phenomenon is produced when these metals are used as negative electrodes in a voltameter. Prosecuting the same line of research, the authors here describe a series of experiments tending to show that the action of electrolytic oxygen on the tenacity of platinum, and of hydrogen on that of copper and zinc, is uncertain; also, that the absorption of hydrogen produces very probably an increase of tenacity in platinum, as it certainly does in iron, but, on the contrary, a diminution in nickel. Nor can these different results be explained by the simple passage of the current, Möbius having already shown that the elasticity of metals is not perceptibly affected by this cause.—Action of arsenate of hydrogen on potassium permanganate, by D. Tivoli. Some experiments are described, from the results of which the author infers that the solution of potassium permanganate is capable of rapidly and completely absorbing arsenate of hydrogen.—S. Giuseppe Terrenzi gives a somewhat complete list of the land and fresh-water mollusks occurring in the Narni district, Umbria. This fauna presents nothing remarkable, all the species being common to other parts of Umbria, and generally to Central Italy. All are described or mentioned by the Marchese Paolucci in his "Étude de la Faune Malacologique terrestre et fluviale de l'Italie et de ses îles" (Paris, 1878).

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, January 30.—"On the Germination of the Seed of the Castor-oil Plant (*Ricinus communis*)." By J. R. Green, M.A., B.Sc., F.L.S., Professor of Botany to the Pharmaceutical Society of Great Britain. Communicated by Prof. M. Foster, Sec. R.S.

The work embodied in this paper deals (*a*) with the agencies which, during germination, render the reserve materials available for the use of the embryo, (*b*) with the forms in which these are absorbed by it and the mode of their absorption, and (*c*) with the parts played in the process by the endosperm and the embryo respectively.

A ferment is found to exist as a zymogen in the resting seed, which is readily developed by warmth and weak acids into an active condition. The results of its activity are the splitting up of the fat with formation of glycerine and (chiefly) ricinoleic acid. Further changes, brought about by the protoplasm of the endosperm cells, form from the latter a lower carbon acid which, unlike ricinoleic acid, is soluble in water and is crystalline. These changes do not take place in the absence of free oxygen. A quantity of sugar also is formed, which appears to have the glycerine as its antecedent.

The proteids of the seed, which consist of globulin and albumose, are split up by another ferment, with formation of peptone and asparagin.

The only products which enter the embryo are a crystalline acid, sugar, possibly some peptone, and asparagin. Consideration of the structure of the cotyledons, which are the absorbing organs, shows that the mode of absorption is always dialysis.

"Investigations into the Effects of Training Walls in an Estuary like the Mersey." By L. F. Vernon Harcourt, M.A., M.Inst.C.E. Communicated by A. G. Vernon Harcourt, F.R.S.

The present investigations were carried out with a working model of the Mersey estuary, from near Warrington to the open sea beyond the bar. The experiments were directed to the solution of two problems—namely, (1) the influence of training walls in the wide upper estuary on the channel below Liverpool, and across the bar; and (2) the effects of training walls in the lower estuary on the channel across the bar.

The experiments indicate that, whereas training walls in the upper estuary would be injurious, owing to the resulting accretion, training walls in the lower estuary would improve the depth of the outlet channel; and that such training walls, combined with dredging, offer the best prospect of forming a direct, stable, and deepened channel across the bar.

February 6.—"Memoir on the Symmetrical Functions of the Roots of Systems of Equations." By Major P. A. MacMahon, Royal Artillery. Communicated by Prof. Greenhill, F.R.S.

The object of the present memoir is the extension to systems of algebraical quantities of the new theory of symmetric functions which has been developed by the author in regard to a single system in vol. xi. and succeeding volumes of the *American Journal of Mathematics*. In the theory of the single system the conceptions and symbolism are to a large extent arithmetical, and are based upon the properties of single integral numbers and their partitions into single integral parts. In this sense the former theory may be regarded as being unipartite.

In the present generalization to the case of m systems of quantities the fundamental ideas proceed, not from a single number, but from a collection of m single numbers. In regard to number, weight, degree, part, and suffix, the collection of m numbers invariably replaces the single number of the theory of the single system. In this view the theory of the m systems is m-partite.

The quantities, to which the symmetric functions relate may be regarded as the solutions common to m non-homogeneous equations each in m variables. Schläfli, in the Vienna Transactions (*Denkschriften*) for 1852, added another linear non-homogeneous equation in m variables, and then forming the eliminant

of the $m + 1$ equations, thereby obtained an identity which is fundamental in the subject. This identity involves those symmetric functions which are here termed fundamental, and marks the starting-point of the present investigation.

In particular, three distinct laws of symmetry are established, large generalizations of those established by the author in the *American Journal of Mathematics* (vol. xi.). Of these the first two are of fundamental importance, and are examined in detail. A leading idea in these theorems, as in the whole investigation, is the "separation" of a partition; the separation bearing the same relation to the partition as the partition to the number or collection of numbers.

In conclusion, the memoir consolidates and largely generalizes the author's recent researches alluded to above.

February 13.—"On the Unit of Length of a Standard Scale by Sir George Shuckburgh, appertaining to the Royal Society." By General J. T. Walker, R.E., F.R.S.

In the determinations of the length of the seconds pendulum, which were made in London by Kater and at Greenwich by Sabine, and are described in the Philosophical Transactions for 1818, 1829, and 1831, the distance between the upper and lower edges of the pendulum was measured off on a standard scale which had been constructed by Sir George Shuckburgh. The scale had not yet been compared with any of the modern standard scales, but it had been preserved with much care with the instruments appertaining to the Royal Society.

In the autumn of 1888, M. le Commandant Defforges, an officer of the French Geodetic Survey, came to England to take a share in operations for the determination of the difference in longitude between Greenwich and Paris, and also to determine the length of a French seconds pendulum at Greenwich. He kindly undertook to comply with a suggestion which was made to him by me, to compare the portion of Shuckburgh's scale which had been employed by Kater and Sabine with one of the standard metre bars of the International Bureau of Weights and Measures in Paris. The Council of the Royal Society assented, and the scale was sent across to Paris and brought back again by special agent.

The details and results of the comparison are given in a special account by Commandant Defforges, from which it will be seen that the scale was compared with the French metrical brass scale, N, at the temperature of 48°·7 F., at which the distance between Kater and Sabine's divisions, 0 and 39·4, of the Shuckburgh scale was found equal to 1·0006245 metre. On reducing to the temperature of 62° F., which was employed by Kater and Sabine, this distance becomes 1·0007619 metre, which is equivalent to 39·400428 inches if we adopt the relation 1 metre = 39·370432 inches, which was determined by Colonel Clarke, C.B., of the Ordnance Survey, and is given in his valuable work on the comparisons of standards of length. Thus the actual length of the space 0 to 39·4 on the Shuckburgh scale may be regarded with some probability as differing by not more than about 0·0004 inch, or, say, the 100,000th part, from the quantity which the scale indicates.

Physical Society, February 7.—Annual General Meeting.—Prof. Reinold, F.R.S., President, in the chair.—The reports of the Council and of the Treasurer were read and adopted. The former stated that there had been a very satisfactory increase in the number of members during the year. The number now exceeds 360, of whom 80 are Fellows of the Royal Society. During the year the Council had proposed to change the time of meeting of the Society from Saturday afternoon to Friday evening. The change was adopted by the members by a vote of 129 to 30, and had resulted in a larger attendance at the meetings. During the year the second part of vol. i. of the translations of important foreign memoirs had been issued to the members, and it was hoped that a third part would be published early in the present session. The Council had to regret the loss by death of three well-known members—James P. Joule, Warren de la Rue, and Father Perry. A valuable collection of books had been given the Society by the Royal Astronomical Society. From the Treasurer's report, it appeared that the balance of the Society had been increased by £120 during the year. Prof. Hittorf, of Münster, was, at the recommendation of the Council, elected an honorary member of the Society. The result of the new election of officers was declared as follows:—President: Prof. W. E. Ayrton, F.R.S.; Vice-Presidents: Dr. E. Atkinson, Walter Baily, Shelford Bidwell, F.R.S, and Prof. S. P. Thompson; Secretaries: Prof. J. Perry and T. H. Blakesley; Treasurer: Prof. A. W. Rücker, F.R.S.; Demonstrator: C. V. Boys, F.R.S.; other Members of Council: W. H. Coffin, Sir John Conroy, Bart., Conrad W. Cooke, Major-General Festing, F.R.S., Prof. J. V. Jones, Prof. O. Lodge, F.R.S., Prof. W. Ramsay, F.R.S., W. N. Shaw, H. Tomlinson, F.R.S., and G. M. Whipple. Votes of thanks were then passed (1) to the Lords of the Committee of the Council on Education for the use of the room in which the Society met; (2) to the auditors, Prof. Minchin and Dr. Fison; (3) to the President and officers of the Society for their services during the year.—The meeting was then resolved into an ordinary science meeting. Messrs. E. W. Smith and C. E. Holland were elected members of the Society, and Mr. Sidney Evershed was proposed as a member.—The paper on galvanometers, by Prof. W. E. Ayrton, F.R.S., Mr. T. Mather, and Dr. W. E. Sumpner, was then resumed by Prof. Ayrton. A long table of numbers accompanying the paper, and representing the result of experiments on many galvanometers, was explained. From this it appeared that galvanometers of the D'Arsonval type were exceedingly efficient in proportion to the amount of wire used in the coils. It was for this reason that voltmeters with strong permanent magnets could be made sensitive even with an exceedingly large external resistance in series so as to diminish the power absorbed by the instrument. The space occupied by the wire was so exeedingly valuable that the extra resistance did not too much diminish the sensibility. The most sensitive galvanometers should therefore be made of the permanent magnet type. If, however, the magnets were to form part of the moving system, as in most galvanometers, the experiments showed that instruments of the Rayleigh, Gray, or Rosenthal type were the best. The coils should be numerous and small, as Mr. Boys had previously shown. As an astatic system of needles sets itself perpendicular to the earth's field, it was recommended that astatic galvanometers should be placed so that the needles pointed east and west. The controlling magnet would then not need to be turned round as it was raised or lowered. It was recommended to calibrate low-resistance ballistic galvanometers for quantity by measuring the deflection for a known current. This obviates the necessity for large condensers or high potentials. The method, although not new, is not described in text-books. In conclusion, Prof. Ayrton asked for information with regard to microscope galvanometers. C. V. Boys, F.R.S., thought that the factor of merit of galvanometers should not be given in scale divisions per micro-ampere under the condition of constant controlling moment. This gave too great an advantage to instruments of the Gray or Rosenthal type. Great sensibility could be obtained by diminishing the moment of inertia of the suspended parts, the practical limit being determined by the trouble due to the silk fibre. Spider lines, when used in place of silk fibres, gave better results. It was possible by using a good suspending arrangement to use needles $\frac{1}{8}''$ long and a period of 20 seconds, and to gain a sensibility far greater than those indicated in the paper. Ballistic galvanometers should be made with needles as light as possible. The method proposed, of winding the central part of the coil in the opposite sense to the rest, would probably not be good, owing to the unevenness of the field produced. The conclusion come to by the author, that D'Arsonval galvanometers of great sensibility should be made with small coils placed in a very strong field, was one he had himself come to, but had finally abandoned owing to difficulties caused by diamagnetism in the copper and to excessive damping due to Foucault currents. Mr. Swinburne thought that the factor of merit of a galvanometer should be determined differently according as it was to be used for the measurement of current, or quantity, or for null methods merely. He saw no great advantage in making practical instruments proportional. The name D'Arsonval should be dropped, as the instrument denoted by it was invented by Varley years ago. He would like to know the relative sensibility of the telephone and the Lippman galvanometer. Prof. Fitzgerald stated that Lord Rayleigh had shown that the microscope method of observing angular deflections was as sensitive as the ordinary method of mirror and scale, even when only the mirror was used as a pointer, so that if a pointer were attached it would be far more sensitive. The drawback, however, was that it was impossible to distinguish with the microscope between lateral displacements of the needles and the angular motion whose measurement was required. To get over this error it was necessary to read both ends of the pointer, but this was hard to do. Prof. Ayrton replied to the different points raised in the discussion.

Entomological Society, February 5.—The Right Hon. Lord Walsingham, F.R.S., President, in the chair.—The President announced that he had nominated Mr. J. W. Dunning, Captain H. J. Elwes, and Mr. F. D. Godman, F.R.S., Vice-Presidents for the session 1890–91.—Mr. F. D. Godman exhibited a specimen of *Papilio thoas*, from Alamos, Mexico, showing an aberration in the left hind wing. Mr. R. Trimen, F.R.S., remarked that butterflies of the genus *Papilio* were seldom liable to variation.—Mr. C. G. Barrett exhibited a series of specimens of *Phycis subornatella*, Dup., from Pembroke, the east and west of Ireland, the Isle of Man, and Perthshire; and a series of *Phycis adornatella*, Tr., from Box Hill, Folkestone, Norfolk, and Reading; also a number of forms intermediate between the above, taken in the Isle of Portland by Mr. N. M. Richardson. He said that these forms proved the identity of the two supposed species, which he believed were both referable to *P. dilutella*, Hb. He also exhibited specimens of *Hesperia lineola*, and a pale variety of it taken in Cambridgeshire; specimens of *Epischnia bankesiella*, a recently-described species, taken in Portland; and a specimen of *Retinia margarotana*, H.-S., a species new to Britain, discovered amongst a number of *Retinia pinivorana*, which had been collected in Scotland.—Mr. W. H. B. Fletcher showed a series of *Gelechia fumatella*, from sandhills in Hayling Island and near Littlehampton, and, for comparison, a series of *G. distinctella*, from the same places. He also showed a few bred specimens of *G. terrella*, and a series of preserved larvæ. He stated that on the downs the larvæ live in the middle of the tufts of such grasses as *Festuca ovina* and allied species.—Mr. H. Goss read a communication from Dr. Clemow, of Cronstadt, St. Petersburg, on the subject of the coincidence of vast flights and blights of insects during the years 1510, 1757, 1763, 1782, 1783, 1836, and 1847, and the epidemic of influenza. During the year 1889 no unusual activity in the insect world had been recorded. Mr. H. T. Stainton, F.R.S., and Mr. McLachlan, F.R.S., made some remarks on the subject, the purport of which was that there was no connection between epidemics and the occurrence of swarms of insects.—Mr. G. A. J. Rothney communicated a paper entitled "Notes on Flowers avoided by Bees." It appeared, according to the author's observations, made in India, that dahlias were exceptionally attractive, but that the passion-flower was only resorted to by a few species of *Xylocopa*; and that, with one exception, he had never seen any insects feeding on the flowers of the oleander. Mr. Slater, Colonel Swinhoe, Mr. Trimen, Lord Walsingham, and Mr. McLachlan took part in the discussion which ensued.—Dr. D. Sharp read a paper entitled "On the Structure of the Terminal Segment in some male Hemiptera."—Colonel Swinhoe read a paper entitled "On the Moths of Burma," which contained descriptions of several new genera and 107 new species.—Dr. F. A. Dixey read a paper entitled "On the Phylogenetic Significance of the wing-markings in certain genera of the *Nymphalidæ*." A discussion ensued, in which Lord Walsingham, Mr. Jenner-Weir, Captain Elwes, and Mr. Trimen took part.

Zoological Society, February 4.—Prof. W. H. Flower, F.R.S., President, in the chair.—The Secretary read a report on the additions that had been made to the Society's Menagerie during the month of January 1890.—A communication was read from Mr. W. K. Parker, F.R.S., containing an account of the morphology of the Hoatzin (*Opisthocomus cristatus*). The author treated of the early stages of the development of this Reptilian Bird, and its shoulder-girdle, sternum, and hind limbs.—A communication was read from Mr. A. D. Bartlett, containing observations on Wolves, Jackals, Dogs, and Foxes. Mr. Bartlett's remarks tended to show that all the varieties of Domestic Dogs owe their origin to Wolves and Jackals, and that the habit of barking has been acquired by, and under the influence of, domestication; also that the Dog is the most perfectly domesticated of all animals.—A communication was read from Mr. G. E. Dobson, F.R.S., containing a synopsis of the genera of the family Soricidæ. The author recognized nine genera, and divided them into two sub-families. New methods of defining the genera were introduced, each genus was briefly characterized, and remarks on certain genera, not admitted in the synopsis (although hitherto generally recognized), were appended.—Mr. F. E. Beddard read a paper containing observations upon some species of Earthworm of the genus *Perichæta*.—A communication was read from Mr. J. M. Leslie, containing notes on the habits and oviposition of the clawed Aglossal Frog (*Xenopus lævis*), as observed at Port Elizabeth, Cape Colony, where this species was said to be of ordinary occurrence.—Mr. Oldfield Thomas read an account of a collection of Mammals from Central Vera Cruz, Mexico, made by a scientific expedition organized by the authorities of the Mexican Museum, under the superintendence of Dr. F. Ferrari-Perez. The collection consisted of about 100 specimens, belonging to 21 species. Amongst these, two (a Hare and a Squirrel) were described as new, and proposed to be called *Sciurus niger melanonotus* and *Lepus veræ-crucis*.

Geological Society, February 5.—W. T. Blanford, F.R.S., President, in the chair.—The following communications were read:—The variolitic rocks of Mont-Genèvre, by Grenville A. J. Cole and J. W. Gregory.—The propylites of the Western Isles of Scotland, and their relations to the andesites and diorites of the district, by Prof. John W. Judd, F.R.S.

EDINBURGH.

Royal Society, January 27.—Rev. Prof. Flint, Vice-President, in the chair.—Prof. Calderwood read a paper on evolution and man's place in Nature. A discussion followed.

February 3.—Sir W. Thomson, President, in the chair.—Dr. William Peddie read a paper on new estimates of molecular distance. He showed that the ratio of the latent heat of vaporisation of a liquid to six times its surface-tension gives an approximation to the number of molecules per linear unit in that liquid. The liquids water, alcohol, ether, chloroform, carbon bisulphide, turpentine, petroleum, and wood spirit, have, according to this method, 50, 52, 30, 15, 19, 30, 40, and 70 millions, respectively, of particles per linear centimetre. Of course no stress is to be laid upon the relative values of these numbers; the point of interest is the complete agreement as to the order of the unknown quantity.—Prof. Tait communicated a paper by Prof. Dittmar on the gravimetric composition of water.—Mr. John Aitken read a paper on the number of dust-particles in the atmosphere of certain places in Great Britain and on the Continent, with remarks on the relation between the amount of dust and meteorological phenomena. He believes that dust condenses moisture before the air is saturated. For the same number of dust-particles per cubic centimetre, the atmospheric transparency depends upon the depression of the wet bulb, being large when the depression is large, but becoming small before the depression vanishes. Increase of temperature also reduces transparency when the number of particles remains the same, for increase of temperature means increase of vapour-pressure. As a rule, quantity of dust decreases when the wind increases. When calms occur dust accumulates. This increases the radiating power of the air, so that it cools quickly and fog forms. Thus a fog may be regarded as a suspended dew.—The dust-measuring instruments intended for use at Ben Nevis were exhibited.

PARIS.

Academy of Sciences, February 10.—M. Hermite in the chair.—Note on an unpublished memoir of Descartes', indicating the right of the author to the priority of a discovery in the theory of polyhedrons, by M. De Jonquières. Some passages are pointed out in the memoir which show that Descartes knew and applied the formula $F+S=A+2$, and furnished the elements of the demonstration, hence his name should be associated with that of Euler as an independent discoverer of the famous formula.—A physical process for the measurement of the inclination of the declination-thread of meridian-circles, by M. Hamy. With ordinary astronomical methods this value can be determined to within half a degree, but using the process described, it is possible to obtain it within a few seconds. The complete description will be given in the coming number (January) of the *Bulletin Astronomique*.—Upon the exponential function, by M. Stieltjes. A demonstration is given of a relation of the form

$$N+e^{a}N_1+e^{b}N_2 \ldots\ldots +e^{h}N_n=0 \quad \ldots\ldots (1)$$

$a, b, \ldots, h$ being whole numbers, $N, N_1, N_2, \ldots N_n$ coefficients. Starting with the polynomial function

$$F(z)=z^{\mu}(z-a)^{\mu+k_1}(z-b)^{\mu+k_2} \ldots\ldots (z-h)^{\mu+k_n}$$

the author deduces that assuming (1) to hold

$$\int_0^h \phi(z)e^{-z}F(z)dz=0,$$

and then proves this function not to hold if μ be an even number.—Note on a method of transformation in kinematic geometry,

by M. A. Mannheim. In a preceding communication the author has shown how to transform the properties relating to the displacement of a straight line, of which the points describe trajectory surfaces; he now extends his method to the case where the points of the movable line describe trajectory lines only, and taking as examples several theorems relating to the former case, derives therefrom corresponding theorems in the latter.—On a generalization of Euler's theorem relating to polyhedrons, by M. R. Perrin. Attention is drawn to some relations bearing upon Euler's formula, published by the author in 1882 (*Bulletin de la Société Mathématique de France*, t. x.).—On bodies which give a tension of dissociation equal to the tension of the vapour of their saturated solutions, by M. H. Lescœur. Experiments are referred to which are antagonistic to the theory of M. Bakhuis-Roozeboom. According to experiment, the curves representing the tensions referred to as functions of the temperature are tangential, and do not intersect at an acute angle as required by the theory.—Action of fluorine upon different varieties of carbon, by M. Henri Moissan.—A general method for the preparation of fluorides of carbon, by M. C. Chabrié.—On the blue flame of common salt and the spectroscopic reaction of copper chloride, by M. G. Salet. The author finds that the bands seen in the spectrum of salt burning in a common fire, and of which the strongest are situated in the indigo and blue-green, are due to copper chloride, and coincide with bands given by Lecoq de Boisbaudran in his "Spectres Lumineux."—On the electrical resistance of iron and its alloys at high temperatures, by M. H. Le Chatelier. The electrical resistances for a considerable range of temperature of a number of iron alloys have been examined. When the results are graphically shown, the curve for ferro-manganese (13 per cent. Mn) is found to be regular, just as is the case with platinum or platinum-rhodium alloy, while the curves for mild and hard steels show distinctly two singular points at 820° and 710°, and a silicon steel (Si = 3 per cent.) shows the former only. Ferro-nickel (25 per cent. Ni) behaves very peculiarly, as below 550° two modifications having quite distinct properties exist, and nickel itself shows a sudden change of curvature at 340°.—Thermochemical researches upon silk, by M. Léo Vignon. Investigations have been made to determine the heat disengaged when various reagents are absorbed by raw and prepared silk. A discussion of the results seems to indicate that the method may be employed to elucidate the theory of dyeing.—Estimation of potassium and humus in soil, by M. J. Raulin. A method of estimating potassium by weighing it on a tared filter as phosphomolybdate is described, together with the application of the modified permanganate process of J. H. Schmidt to the determination of humus.—On a colouring-matter from Diaptomus, analogous to the carotin of vegetables, by M. Raphael Blanchard. The colouring-matter, isolated from these animal organisms, is shown to differ considerably in spectroscopic properties and in its solubility in alcohol from the lipochromes, and it does not prove to be identical with any of the red pigments from the Cœlenterata, Echinodermata, Bryozoa, or Mollusca; while on the contrary it is found to show many analogies to carotins ($C_{26}H_{38}$), which are so marked as to lead to the conclusion that it is itself a carotin and so possesses great interest as a colouring substance common to both the animal and vegetable kingdoms, and as an instance of the production of a hydrocarbon by animal agency.—On the intercellular substance, by M. Louis Mangin. It is shown that among Phanerogams and Cryptogams (with the exception of Fungi and many Algæ) the tissues of the softer parts are composed of cells cemented together by an intercellular substance composed of insoluble pectates.—On the localization of colouring-matters in the seminal integuments, by M. Louis Claudel.—Formation of quartz at the spring of Maubourot at Cauterets, by M. Beaugey.—On the existence of leucite rocks in Asia Minor, and on some hypersthene rocks from the Caucasus, by M. A. Lacroix. It is found that the leucitic rocks from near Trebizonde fall under two main types, leucitite and leucotiphrite.—Upon the composition of some pseudo-dolomitic chalks from the north of France, by M. L. Cayeux.

BERLIN.

Meteorological Society, January 7.—Dr. Vettin, President, in the chair.—Dr. Wagner spoke on the behaviour of water in the soil. The relationships between surface water and springs and deposits, possessing as they do a distinct meteorological interest, have as yet been but slightly investigated, probably because the behaviour of water in soil occupies the border-land between the subjects of meteorology, geology, agriculture, and hygiene. A review of scientific investigations which have so far been made on the subject of surface water and the formation of springs, shows that the problems of most importance are still awaiting their solution. In the speaker's opinion the task to be undertaken in the interests of meteorology is the establishing of as many lysimeters as possible, so that by keeping a continuous record of their indications a continued set of observations on surface water would be provided. He further considered it to be essential that the relationship of water to the soil should be investigated at depths far greater than has as yet been the case. A lengthy discussion followed the above communication, which turned chiefly upon a consideration of the forces, as yet but little known, which determine the collecting of water on internal impervious layers of the earth.—Prof. Spörer gave a short statistical statement on sun-spots during 1889. The chief point of interest was that the spots appeared during the first half of the year in the lower latitudes and in the second half in the higher. Taking the year as a whole, there were considerably more spots in the southern than in the northern hemisphere; this has been the case in each year since 1883.—The Secretary then handed in his annual report, and the Society proceeded to elect its officers for the year 1890. Prof. Schwalbe was elected President.

Physical Society, January 27.—Prof. Kundt, President, in the chair.—The President opened the meeting by a short address in memory of civil engineer G. A. Hirn, who died recently at Logelbach in Alsace.—Dr. Lehmann spoke on the testing of tuning-forks. After the International Congress met for the establishing of a uniform standard of tone, and selected for this purpose a vibration frequency of 435, it devolved upon Government to construct a standard fork, and to devise some ready method for testing ordinary forks to an accuracy within half a vibration per second, and standard forks within 0·1 of a vibration. The speaker discussed the various methods in use for comparing two forks and for counting the number of vibrations per second which they yield. For the first purpose the vibrations of the respective forks are employed, these being observed either acoustically or optically; a further means of effecting the comparison is by the stroboscopic method or by the acoustic wheel. The vibration frequency of a fork is determined either graphically or by means of a tuning-fork clock, or by means of the undulations obtained by oscillating or rotating acoustical instruments. An important factor in all these methods is the temperature of the fork. To determine this a special thermostat is employed, by means of which the fork can be set in vibration in an air-bath whose temperature is constant and accurately known. The standard fork for reference is one of König's, whose vibration-frequency has been accurately determined by several methods. The comparison of any new fork with the standard is made by means of the acoustic wheel, and by a simultaneous graphic recording of the movements of the fork which is vibrating inside the thermostat, and of the magnetic interrupter; the latter consists of a tuning-fork vibrating to the octave below the note yielded by the standard fork.—Dr. Eschenhagen exhibited curves of the three elements of terrestrial magnetism recorded by the new instruments in the Observatory of Potsdam, and gave a short description of the arrangement of the apparatus. The curves were taken on white photographic paper, and were of such dimensions that the greatest variations which have as yet been observed were completely recorded.—Prof. Kundt exhibited some quartz-fibres which he had received from Prof. Weinhold. He made, in addition, some remarks on the preparation of these fibres by Boys's method, and gave some data as to the dimensions of an apparatus which Prof. Weinhold had constructed for the measurement of gravitation constants, and had employed in several determinations.

AMSTERDAM.

Royal Academy of Sciences, Dec. 28, 1889.—Prof. van de Sande Bakhuyzen in the chair.—M. Hugo de Vries related the results of the scientific researches made by the Committee of Advice, appointed in July 1887 at Rotterdam, to report on the appearance of Crenothrix in the drinking-water of the Rotterdam water-supply. He gave an account of the organisms met with in the mains and basins before and after the filtration of the water, and of the degree of the pollution caused by these creatures in the colder and warmer months of the year. He spoke also of the influence of darkness on the water-organisms, which, under ordinary circumstances, live in the sunlight; of the

proposals made by the Committee to mitigate or remove the evil; and of the improvements effected, or about to be effected, in accordance with those suggestions.—M. Kapteijn treated of chronographical observations for the purpose of determining parallaxes of fixed stars. After having explained the precautions taken to prevent systematic error, he gave the results and subjected them to several tests showing their absolute trustworthiness within the limits defined by the probable errors.

Jan. 25.—Prof. van de Sande Bakhuyzen in the chair.—M. Hoogewerff, giving an account of joint work by himself and M. van Dorp, spoke of the action of potassium hypobromite on succinphenylamide, and on the amide of cinchonic acid.—M. van Bammelen communicated certain results of a research relating to the composition of volcanic and other soils, on which, in Deli and Java, tobacco is cultivated. The extraordinary fitness of the soil of the cleared forest grounds in Deli for the production of exquisite tobacco is to be attributed, he thinks, to the peculiar composition of the amorphous silicate occurring therein, to the looseness of the forest soil, and to the auspicious climate with regard to the rainfall. He concluded by insisting on the urgent need for the establishment of a scientific experimental station at Deli. Such an establishment would be favourable to the culture of tobacco, and would enlarge our knowledge of the soil, of the vegetable world, and of geological formations.

DIARY OF SOCIETIES.

LONDON.

THURSDAY, FEBRUARY 20.

ROYAL SOCIETY, at 4.30.—A Comparative Study of Natural and Artificial Digestions (Preliminary Account): Dr. A. Sheridan Lea.—On a Fermentation causing the Separation of Cystin: Sheridan Delépine.—Some Stages in the Development of the Brain of Clupea harengus: Ernest W. L. Holt.

LINNEAN SOCIETY, at 8.—On the Fruit and Seed of Juglandia; on the Shape of the Oak-leaf; and on the Leaves of Viburnum; Sir John Lubbock, Bart., P.C., M.P., F.R.S.

CHEMICAL SOCIETY, at 8.—The Behaviour of the most Stable Oxides at High Temperatures: G. H. Bailey and W. B. Hopkins.—The Influence of Different Oxides on the Decomposition of Potassium Chlorate: G. J. Fowler and J. Grant.

ZOOLOGICAL SOCIETY, at 4.

INSTITUTION OF ELECTRICAL ENGINEERS, at 8.

ROYAL INSTITUTION, at 3.—The Three Stages of Shakspeare's Art: Rev. Canon Ainger.

FRIDAY, FEBRUARY 21.

GEOLOGICAL SOCIETY, at 3.—Annual General Meeting.

PHYSICAL SOCIETY, at 5.—On a Carbon Deposit in a Blake Telephone Transmitter: F. B. Hawes.—The Geometrical Construction of Direct Reading Scales for Reflecting Instruments: A. P. Trotter.—A Paralle Motion Suitable for Recording-Instruments: A. P. Trotter.—On Bertrand's Refractometer: Prof. S. P. Thompson.

INSTITUTION OF CIVIL ENGINEERS, at 7.30.—Some Types of American Locomotives, and their Construction: C. N. Goodall.

ROYAL INSTITUTION, at 9.—Magnetic Phenomena: Shelford Bidwell, F.R.S.

SATURDAY, FEBRUARY 22.

ROYAL BOTANIC SOCIETY, at 3.45.

ROYAL INSTITUTION, at 3.—Electricity and Magnetism: Right Hon. Lord Rayleigh, F.R.S.

SUNDAY, FEBRUARY 23.

SUNDAY LECTURE SOCIETY, at 4.—Our Ancestors, the Sea-Kings: Justin H. McCarthy, M.P.

MONDAY, FEBRUARY 24.

SOCIETY OF ARTS, at 8.—Stereotyping: Thomas Bolas.

TOYNBEE PHILOSOPHICAL SOCIETY, at 8.—Will and Reason: B. Bosanquet.

TUESDAY, FEBRUARY 25.

ANTHROPOLOGICAL INSTITUTE, at 8.30.—Exhibition of Stanley's Spirometer: Dr. J. G. Garson.—Some Borneo Traps: S. B. J. Skertchly.—The Dieri and other Kindred Tribes of Central Australia: A. W. Howitt.

INSTITUTION OF CIVIL ENGINEERS, at 8.—The Shanghai Water-Works: J. W. Hart.—The Tytam Water-Works, Hong-Kong: Jas. Orange.—The Construction of the Yokohama Water-Works: J. H. T. Turner. (Discussion.)

ROYAL INSTITUTION, at 3.—The Post-Darwinian Period: Prof. G. J. Romanes, F.R.S.

WEDNESDAY, FEBRUARY 26.

GEOLOGICAL SOCIETY, at 8.—On a Crocodilian Jaw from the Oxford Clay of Peterborough: R. Lydekker.—On the Relation of the Westleton Beds or "Pebbly Sands" of Suffolk to those of Norfolk, and on their Extension Inland; with some Observations on the Period of the Final Elevation and Denudation of the Weald and of the Thames Valley, Pari III.: Prof. Joseph Prestwich, F.R.S.—On a Deep Channel of Drift in the Valley of the Cam, Essex: W. Whitaker, F.R.S.

SOCIETY OF ARTS, at 8.—The English in Florida: Arthur Montefiore.

THURSDAY, FEBRUARY 27.

ROYAL SOCIETY, at 4.30.

SOCIETY OF ARTS, at 8.—The Northern Shan States and the Burma-China Railway: William Sherriff.

INSTITUTION OF ELECTRICAL ENGINEERS, at 8.

ROYAL INSTITUTION, at 3.—The Three Stages of Shakspeare's Art: Rev. Canon Ainger.

FRIDAY, FEBRUARY 28.

AMATEUR SCIENTIFIC SOCIETY, at 8.—Practical Coal-mining: H. S. Streatfeild.

ROYAL INSTITUTION, at 9.—Evolution in Music: Prof. C. Hubert H. Parry.

SATURDAY, MARCH 1.

ESSEX FIELD CLUB, at 7.—Micro-Fungi of Epping Forest; how to Collect, Preserve, and Study Them: Dr. M. C. Cooke.

ROYAL INSTITUTION, at 3.—Electricity and Magnetism: Right Hon. Lord Rayleigh, F.R.S.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Elementary Dynamics of Particles and Solids: Prof. W. M. Hicks (Macmillan).—La Vie des Oiseaux: Baron D'Hamonville (Paris, J. B. Baillière).—A Naturalist's Voyage round the World, new edition, illustrated: C. Darwin (Murray).—A Naturalist among the Head Hunters: C. M. Woodford (Philip).—Geology of the Quicksilver Deposits of the Pacific Slope, and Atlas to accompany same: G. F. Becker (Washington).—Fossil Fishes and Fossil Plants of the Triassic Rocks of New Jersey and the Connecticut Valley: J. S. Newberry (Washington).—Il Teorema del Parallelogramma delle Forze Dimostrato Erroneo: G. Casazza (Brescia).—Materials for a Flora of the Malayan Peninsula: Dr. G. King (Calcutta).—Journal of Physiology, vol. xi. Nos. 1 and 2 (Cambridge).—Transactions of the Wagner Free Institute of Science of Philadelphia, vol. 2 (Philadelphia).—Observaciones Magnéticas y Meteorológicas del Real Colegio de Belen de la Comp. de Jesus en La Habana, Julio-Dic. 1887 (Habana).—Bulletin of the U.S. Geological Survey, Nos. 48 to 53 (Washington).—Department of Agriculture, Melbourne, Bulletin No. 4 (Melbourne).—"Timehri," being the Journal of the Royal Agricultural and Commercial Society of British Guiana, December 1889 (Stanford).

CONTENTS.

THURSDAY, FEBRUARY 27, 1890.

THE NEW CODES, ENGLISH AND SCOTCH.

THE country is once more within a month of a new Education Code. Once more the Lord President and the Vice-President of the Council are being besieged by representatives of all interests and opinions, anxious to impress them with the exclusive importance of their particular views. Last year, it will be remembered, the Code—great advance as it was on its predecessors—fell a victim to the fears of one party and the lukewarmness of the other. The extreme School Board partisans gave but scant support to any scheme which did not practically embody the recommendations of the minority of the late Royal Commission, while the champions of voluntary schools shrank from any changes which, by raising the standard of efficiency, seemed likely to accentuate the difference between the Board school, which has the ratepayers' pocket to draw on, and the voluntary school, which depends on a fast-shrinking fund of private subscriptions. And so the Code was sacrificed, and the friends of education were condemned to wait another year.

This is what is constantly happening, and what will continue to happen, so long as there are ten experts forthcoming on all matters relating to educational machinery for one who knows and cares about education itself. Whether elementary schools should be free; whether they should be under representative control; whether they should all receive rate-aid—these and the like disputes are always sure to gain the ear of the public, while the problem of making the education provided worth disputing about is passed by almost unnoticed.

How few among our so-called "educationists" (a newly-introduced word with an ominous ring about it) ever sit down deliberately to face the central problem of elementary education—the only problem of fundamental importance: Given a child between the ages of 5 and 13, with the limitations imposed by its age, by its home surroundings, by the pressing necessity that it should begin to earn a living as soon as possible, and by the fact (most neglected of all by theorists) that there are only a certain number of school hours in the day—what is the best kind of training through which it shall pass? How can those few precious years be best utilized?

Theories, indeed, there are, enough and to spare, till we could wish sometimes that all those in high places who talk of education were made to go through an apprenticeship as school managers, in order to gain some practical acquaintance with the limits imposed on the range of instruction by the nature of the child-material with which they have to deal. For no designer trained to make "designs-in-the-abstract"—who produces patterns for carpets which cannot be woven, for wall-papers which cannot be printed, for copper that cannot be beaten, and for wood that cannot be carved—could be more out of touch with the material in which his designs have to be executed than the educational "reformer-in-the-abstract," who sketches fabulous plans for Universal National Systems of Education which have only one defect—that they are impossible to carry out.

And now, having relieved our feelings, we may turn to the question of immediate importance—namely, the prospects of educational advance under the new Code which is so eagerly expected.

It is rumoured that the authorities at the Education Department are earnestly engaged in the attempt to make the Code a real advance on former efforts. They have many difficulties. If they can successfully run the gauntlet of the Treasury, they have to reckon with the factious criticism of political partisans. We hope, however, that we may assume that the draft Code as it issues from the Department will embody at least all the purely *educational* reforms which appeared in its unlucky predecessor. The clause requiring English as a class subject will go, the curriculum and regulations for evening schools will be made more elastic, an attempt will be made to spread the teaching of drawing, and further facilities will be afforded for science instruction at central schools and classes. It will be the task of outside critics to see that these proposals, already made in last year's Code, are not whittled down, and that they are supplemented by other changes on which al educational reformers are practically agreed. What these changes are may be gathered from the discussion on elementary education, especially in its relation to scientific and technical instruction, which followed Dr. Gladstone's paper at the Society of Arts last November. The programme has been since embodied in a more definite and concrete form in the suggestions which have just been submitted to the Education Department by the Committee of the National Association for the Promotion of Technical and Secondary Education. Among other suggestions they propose that drawing should be made compulsory in boys' schools, of course being allowed a due interval before the regulation comes into operation, during which schools may adapt their staff for the purpose. Elementary drawing should be introduced into infant schools for boys to correspond to needlework for girls, as proposed in last year's Code. The absurd minute of the Science and Art Department—forced on them, it is only fair to say, by the Treasury—confining grants on drawing in girls' schools to departments where cookery is taught, ought of course to be repealed; not so much in the interests of the girls, as of the boys in mixed schools, for whom under the existing regulations provision for drawing cannot well be made. Drawing is not only the basis of all technical instruction, but is a subject of very high educational value, and on both grounds its spread is much to be desired. A further change which is to be hoped for is the extension of the Kindergarten methods from the infant school into the lower standards, and their continuation by means of graduated object-lessons so as to lead up to more distinctive scientific and manual instruction for the more advanced scholars of the school. Manual instruction of some kind ought to be introduced throughout boys' schools to balance needlework instruction for girls.

By manual instruction we do not merely mean instruction in woodwork (called, rather unhappily, the "use of tools" in the recent Act), which is evidently only suitable for the higher standards, say the sixth and seventh. We doubt if it can be profitably given to children below the age of 11, and even in the case of these it can of course only take the form of the "hand and eye" training—not of specific

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instruction in carpentry. For younger children, however, much might be done in the way of modelling (or, as it has been called, "applied drawing"), designed to carry on the training of the fingers which are often made so nimble by the paper-cutting and the Kindergarten exercises of the infant school, only at present to lose their pliancy and dexterity by want of practice as soon as the child emerges from the fairy-land of the Kindergarten into the dull, prosaic atmosphere of Standard I.

To introduce this change it will doubtless be necessary to abolish individual examination in the lower standards at least, and assimilate them in this respect to the infant school. Another change will also be necessary, in the mode of interpreting the Education Acts which has hitherto been customary at Whitehall. Up to the present time there has been a tendency in the Government Departments to decline to recognize manual training as a form of instruction contemplated by the Acts, and in the well-known case of the Beethoven Street Board School, the London School Board were surcharged by the auditor with the cost of tools. The School Board failed to carry the question to the law courts, and so for a time the matter rested. Since then, however, the question has entered on a new phase. The Liverpool School Board, wishing to provide manual instruction in its schools, has obtained the opinion of Sir Horace Davey, Q.C., to the effect that such provision clearly comes within the power of School Boards. The Board has consequently taken steps to make the necessary provision, has appointed an instructor, and now only waits to be surcharged in order to carry the whole question to the Queen's Bench. Other School Boards are following suit, so that we must very shortly see the matter settled in one way or another. The legal question is interesting, not only in its bearing on manual training, but on the general powers of School Boards to give *any* extra instruction they please, provided they comply with all the regulations and requirements of the Education Department for the time being. If Sir Horace Davey's opinion is sustained, it carries with it the right of School Boards to provide any form of technical or manual instruction that can be given consistently with the regulations of Whitehall. Up to the present year, as we stated above, the Education Department was not altogether favourable to the views of Sir Horace Davey. But it is rumoured that of late the views of the authorities on the subject have undergone a change, and that it is probable that manual instruction may not only be recognized as legal, but actually incorporated as a grant-earning subject in the forthcoming Code. The rumour, which we sincerely hope is true, is confirmed by the fact that in the Scotch Code just issued a clause is inserted for the first time inviting school managers to submit as a class subject (earning a grant of 2*s.* or 1*s.* a head) "a course of manual instruction on a graduated system." The Scotch Education Department, therefore, has conceded the whole principle, and though of course Scotland has a separate Act, the admission is full of significance. It would be a trifle too absurd for the English Education Department to refuse to "recognize as educational" a subject which the Scotch Office thinks important enough to be encouraged by a grant.

In other respects the new Code just issued from Mr. Craik's office is a valuable index, if not of what we shall get, yet of what we may justly press for, in the coming English Code. It is, indeed, an enormous advance. Scotch members of Parliament sometimes complain that Scotch business attracts no attention at Westminster. The evil, however, has at least some compensating advantages. Unchallenged—almost unnoticed—the officials at the Scotch Education Office can quietly introduce by a stroke of the pen the reforms in the Code for which we in England have to wait year after year. It may serve a useful purpose if we recount a few of the reforms which Mr. Craik has been able to carry out this year in Scotch education. Of the abolition of fees we say nothing, for that was the result of legislation last session.

In the first place, individual examination in the elementary subjects, which had already been abolished in the first three standards, is now replaced by collective examination throughout the school. This change gives much greater elasticity and liberty of classification to the teacher, and to a great extent modifies the pressure of the system of payment by results.

In the next place, the system of class subjects is entirely revised. Several alternative courses in elementary science are suggested, including courses of "nature knowledge" in "animals," "vegetables," and "matter," for each of which simple and suitable suggestive syllabuses are laid down. Any other progressive scheme of teaching may be submitted to the inspector for approval. "In elementary science this scheme may be so framed as to lead up to the teaching of scientific specific subjects. It may include the subjects of navigation or the elementary principles of agriculture; and a course of manual instruction on a graduated system may also be submitted."

At the same time the regulation requiring either English or elementary science to be taken as one of the class subjects is rescinded. It is to be noticed that in Scotland an attempt was made in the previous Code to encourage science teaching by making it alternative to English as a compulsory class subject. It is somewhat disappointing to be told, as we are in the last Scotch Report, that the change has as yet produced but little increase in science teaching. This fact seems to support the suggestion of the Technical Association that science instruction (which gives more trouble and requires more appliances) should be encouraged by a slightly higher scale of grant than that allotted to other class subjects. But it also tends to suggest the possibility that part of the price which Scotland has to pay for the ease with which it can get educational changes carried out is a certain popular indifference to those changes which may go far to make them nugatory. Thus it is quite possible that the Departmental invitation to submit courses of manual instruction may produce far less effect on schools in Scotland than would be produced in England by a favourable decision of the law courts on a hotly disputed case such as that which may come before them in connection with the Liverpool School Board. The steam which has to be got up on this side of the Tweed in order to get a reform permitted will often supply the motive force which will get that reform carried out. The different fate which has attended the Scotch and the English Technical Instruction Acts hitherto is a case in point. The Scotch Act, passed with ease through

an apathetic House, has fallen flat, while the English Act, badly drawn as it is, is arousing a great and increasing amount of interest in the country, and within the first six months is already in full swing in several districts.

But this is a digression. The recasting and improvement of the system of class subjects in Scotland is interesting not only in itself but as indicating a probable change of a similar kind in the English Code. Under these circumstances we must not fail to note the parallel change carried out in the schedule of "specific subjects." Almost the whole of the schedule which relates to science subjects—chemistry, mechanics, electricity, light and heat, physiology, botany, and physical geography—is entirely cancelled, and for the detailed syllabuses of these subjects is substituted a simple invitation to school managers to submit graduated courses in subjects not mentioned in the schedule. At first sight this seems a loss—as though the Department were moving in the direction of paying less instead of more attention to science. The alteration, however, must be read in conjunction with the reforms in class schedules and the observations on class and specific subjects in the last Report of the Scotch Education Department. Commenting on the fact that "the general development of class subjects tends to restrict the specific subjects," the Report proceeds: "this is a result not altogether to be regretted, as the influence of the class subjects is general, while that of the specific subjects is restricted to a few selected scholars."

Again, in the instructions to inspectors just issued, Mr. Craik explains one of the objects of the Department to be "to spread the beneficial results of any such higher teaching as may be given, to the whole school, instead of confining it to a few selected scholars."

It is clear, therefore, that the changes in the fourth and fifth schedules (which are probably the precursor of similar changes in the English Code) are dictated by a desire to extend class instruction in science, even if at the expense of specific subjects; in other words, to transfer natural science from its former position, as a smattering of a few special branches of physics imparted to a few pupils, to its proper place as a course of general stimulating instruction in the elements of "nature knowledge," given as an integral part of the school course to the school as a whole. More specialized science teaching can still be provided if desired in the form of specific instruction framed to suit local wants by the various school managers, or it may be given, as is already the case in many elementary schools, by means of science classes in connection with the Science and Art Department.

We cannot doubt that the Scotch Department is right in its policy, but the probable extension of class teaching under the new and more elastic *régime* suggests a doubt whether the proper way of introducing manual instruction is by means of including it among the class subjects, so long at least as the possible number of class subjects is restricted. Drawing—the only form of manual training previously recognized for boys—has already been put outside the range of class subjects. Needlework—the only other manual subject in the Code—may be taught either as a class subject or as part of the ordinary curriculum of the school. Is there not a chance that in including manual instruction among the class subjects an unnatural rivalry may be set up between this subject and elementary science, which may restrict the spread of both? All this, however, is a matter for the future. Meanwhile we have only to congratulate the Scotch on the improvement of the conditions under which in the future their schools will be carried on, and to express the hope that England will not lag behind.

One word in conclusion. It may be wondered why in this article, dealing with scientific and technical instruction in elementary schools, so little reference is made to the Technical Instruction Act of last session, either in respect of the powers which it confers on elementary school managers, or of those which, much to the regret of many politicians, it appears to withhold.

The real fact is that we have our doubts as to the need of any general Technical Instruction Act for elementary schools, and have a suspicion that their exclusion from the late Act was in reality a blessing in disguise. Of course, if the opinion of Sir Horace Davey (and now we are glad to be able to add, of the Scotch Education Department) should be upset in the law courts, it may be necessary to rectify the anomaly by a short Act of a single clause recognizing the legality of manual instruction. But, with this possible exception, no new powers are required by School Boards, and no new rate need be imposed. Mr. Mundella, in complaining of the exclusion of elementary schools from the late Act, compared the scheme to an educational ladder with the lower rungs left out. Let him be reassured—no rung is wanting so far as legislation is concerned. As at present advised, we feel clear that the managers of a public elementary school, so long as they comply with the requirements of the Department, may teach what extra subjects they please. The rating power possessed by a School Board is limited only by the wishes of the ratepayers. What really retards the introduction of technical and manual instruction is the want of imperial grants (which may and ought to be given through changes in the Code), the want of time, the pressure of other subjects, the ignorance of the public, and the parsimony of the ratepayers. But none of these obstacles can be removed by legislation. What legislation could and probably would do, would be to restrict the present powers of School Boards by defining them; and, perhaps, even to confine the rate for technical instruction within the limit of a penny in the pound. But this can hardly be what Mr. Mundella wants.

A DICTIONARY OF APPLIED CHEMISTRY.

A Dictionary of Applied Chemistry. By T. E. Thorpe, B.Sc. (Vict.), Ph.D., F.R.S., &c. Assisted by Eminent Contributors. In Three Volumes. Vol. I. (London: Longmans and Co., 1890.)

THE first volume of the "Dictionary of Applied Chemistry," edited by Prof. Thorpe, is a welcome addition to our scientific books of reference, and forms an admirable companion to the "Dictionary of Theoretical Chemistry," the second volume of which was reviewed some weeks ago.

In the preface Prof. Thorpe points out that, as this

work has special reference to the applications of chemistry to the arts and manufactures, it deals but sparingly with the purely scientific aspects of the science, unless these have some direct and immediate bearing on the business of the technologist. How direct and how immediate such a bearing is at the present day, and how difficult, not to say impossible, it is to separate theory from practice, may be judged of by turning over the pages of this most useful volume.

Take, for example, the article on the azines, written by the most competent authority on that subject, Dr. Otto Witt, of Berlin. The untrained technologist will be completely at sea with the honeycomb of benzene rings with which he clearly explains the constitution of such well-known compounds as the safranenes, the splendid yellow dyes so ably investigated by Dr. Witt himself, whereas the manufacturer who has the theory of the subject at command is complete master of the situation. Or, again, let us turn to the next article, on the azo-colouring matters, communicated by another equally trustworthy authority, Prof. Meldola, covering 28 thickly-printed pages, in which the same necessary connection is seen. And no other example, perhaps, indicates more forcibly the enormous advance which applied chemistry has made in the last ten years, and its entire dependence upon abstract research. In proof of this, it needs only to be pointed out that the article concludes with a list of no less than 95 distinct patents on this one group of colouring matters, from March 12, 1878, to June 30, 1888, all of which are the result of original, chiefly German, research.

An examination of other important articles written by specially-qualified contributors indicates that each subject is brought up to the level of the present state of our knowledge. Let us look for a moment at the article on ammonia, contributed by Prof. Lunge, of Zurich. Here we find detailed reference to the newest forms of apparatus for the manufacture of ammonium salts, illustrated by excellent woodcuts of the Feldmann-still. Again, turning to the article on chlorine, we have to note the same completeness and technical grasp of the questions discussed. Thus, on p. 526, we find the method patented so long ago as 1866 by Mr. Brock, of Widnes, and now for the first time coming into general use, which has for its object the treatment of the exit gases from the bleaching-powder chambers by means of a dry lime-sprinkler, this not only removing a serious nuisance in the manufacture, but also recovering chlorine otherwise wasted.

Prof. Hummel, of Leeds, contributes an excellent article on bleaching; and here again we see that the newest processes are fully described, *e.g.* on p. 323 the Mather-Thompson bleaching process is fully noticed, and the electrical bleaching process of Hermite likewise referred to. As regards this latter, the conclusion arrived at is that now generally admitted by practical authorities, viz. that electrolytic bleaching cannot reasonably be expected to replace bleaching-powder at a price of £7 per ton.

One of the most valuable articles in the book is written by Mr. John Heron on brewing, in which he not only describes the most modern forms of brewing plant and processes, but gives a clear statement of the important researches of Pasteur and Hansen on the alcoholic ferments. As we all know, it was Pasteur who first directed attention to those other forms of Saccharomyces known as "wild" yeasts in fermenting yeasts and beer; but it is not so commonly understood that it was Hansen who taught us how to introduce into the liquid a seed yeast really free from "wild" forms. Since 1883 carefully selected types of yeast from pure cultures, according to Hansen's researches, have been introduced into Denmark, Norway, and Bavaria, with the most satisfactory results, whilst in England nothing of the kind has yet been done, although, at Burton several experiments have been made in this direction. Sufficient has already been done to show that several varieties of *Saccharomyces cerevisiæ* can be separated, which, however, do not differ morphologically, but may be distinguished from each other, inasmuch as they give entirely different results, both as to flavour brightness, attenuation of the beer, and to the mode of separation of the yeast. The proportion of these different varieties in various breweries seems to remain constant, and to give the peculiar flavour and appearance which the various fermented liquors possess.

Another article is that by Prof. Noel Hartley on cements, a subject which though of great importance is not usually considered of great chemical interest, but it has been made so by the writer. He points out the fact, certainly not known to the majority of chemists, that we owe to Lavoisier the first explanation of the phenomena of the baking and hardening of plaster of Paris. At so early an age as 21, he published a short note in the *Comptes rendus* of February 17, 1765, in which he showed that water is removed from the gypsum in two stages, that the first three-quarters of the combined water must be removed in order that the plaster shall afterwards set, but that if the whole of the combined water be removed, the gypsum becomes overburnt and loses its value as plaster.

It is probable that this volume will have even a larger sale than that of the corresponding "Dictionary of Pure Chemistry," and, as with that important work, so with this, the public may well be congratulated on possessing such a valuable book of reference so creditable to all concerned in its production. H. E. ROSCOE.

OATES'S ORNITHOLOGY OF INDIA.

The Fauna of British India, including Ceylon and Burma. Published under the authority of the Secretary of State for India in Council. Edited by W. T. Blanford. *Birds.* Vol. I. By Eugene W. Oates. Pp. i.—xx., 1—556. (London: Taylor and Francis, 1889.)

The Nests and Eggs of Indian Birds. By Allan O. Hume, C.B. Second Edition. Edited by E. W. Oates. Vol. I. Pp. i.—xii., 1—397. (London: R. H. Porter, 1889.)

THE two volumes on the birds of India, which Mr. Oates has recently published, will supply a much needed want. The period of twenty-six years which has elapsed since the publication of Jerdon's "Birds of India" has been prolific in ornithological work, to such an extent that a new adjustment of the scattered details which had accumulated since that time had become an

absolute necessity. Mr. Oates has already won his spurs in the field of Indian ornithology; for his "Hand-book of the Birds of Burma," published in 1883, has always been looked upon as a standard work; and by coming to England, at great personal sacrifice, to write the bird volumes of the "Fauna of British India," he has deserved the gratitude of all zoologists. Those of us who are acquainted with the "Hand-book" before mentioned, will not be surprised to find that in the present volumes Mr. Oates has done his work in a thoroughly conscientious manner. Without commencing, as Jerdon did, with a general outline of ornithology, for which space was not available, Mr. Oates has contrived to give a condensed introduction, which will give the student some small idea of classification of passerine birds, with which this volume deals. We could have wished that the author had followed a more natural arrangement of passerine families, as his scheme of arrangement results in some very incongruous affinities, but these will doubtless be further explained when the author gives a detailed arrangement of the orders and families of birds in his third volume. As the furlough which has been granted to Mr. Oates is quite insufficient for him to finish the work in anything like a reasonable period, we are glad to learn that a representation has been made to the Government of India, by some of our leading men of science, for a further extension of leave, to enable the author to finish the work, which he has begun so creditably. It would be a thousand pities to see the completion of this book intrusted to less capable hands, of which there seems to be some fear expressed in Mr. Blanford's preface.

Since Mr. Seebohm, in the fifth volume of the "Catalogue of Birds in the British Museum," laid stress on the importance of the plumage of the young as distinguishing characters between the Thrushes and the Warblers, this character has been thoughtfully considered by many ornithologists; but Mr. Oates has been the first to apply it in any large measure to the bulk of the passerine birds, and it enables him to divide them into five sections, characterized by the plumage in the nestling. This arrangement brings about some rather startling results, for the Titmice (*Paridæ*) become merged in the family *Corvidæ*, and the Dongos (*Dicruridæ*) range in close proximity to the Nuthaches (*Sittidæ*) and the Creepers (*Certhiidæ*). This character of the plumage of the nestlings, like all single characters, carries the author too far, and it is becoming more and more plain every day that the natural classification of birds in the future will be founded on a combination of characters, not on any single one alone. Mr. Oates himself, in his arrangement of the *Crateropodidæ*, shows how this can be done.

It is impossible to praise too highly the method in which the present book has been worked out, though it is to be regretted that four volumes were not allowed for the birds, instead of three, for the constriction of the work has compelled the author to treat of 563 species in 544 pages, which is an allowance of less than a page to each species, including the space necessary for family characters and "keys" to genera and species. We notice that the author has been driven to create a good many new genera, but we are not disposed to quarrel with him on this account, though we notice that, like ourselves, in writing the "Catalogue of Birds," he has found it hard to be consistent, and he certainly varies somewhat in his estimate of characters in different families. Thus he divides the Bulbuls into a number of slenderly defined genera, yet he places the Rook and the Jackdaw in the same genus, *Corvus*, as the Raven. What was sauce for a Bulbul ought to have been sauce for a Rook! It is very interesting to notice the immense strides which our knowledge of Indian ornithology has made in the last twenty years. This is mostly due to the energy of Mr. Allan Hume, whose marvellous collection of Oriental birds was given by him to the British Museum in 1885. Since that date the registration and arrangement of the Hume Collection, has occupied the bulk of our own time and that of our colleagues in the Bird Room, so that the whole of the Indian Passeres have been placed conveniently at Mr. Oates's disposal for the present work. It may, indeed, be said that Mr. Hume sowed, the officers of the British Museum watered, and Mr. Oates came over from India in time to gather the increase. It must be a great pleasure to Mr. Hume, and to Major Wardlaw Ramsay, who gave the Tweeddale Collection and Library to the Museum two years ago, to see that already their magnificent donations have been turned to such good account.

The number of new species described by Mr. Oates is, as might be expected, small; but ornithology has now reached a stage when the description of new species will be surpassed in interest by the study of greater facts, of which the geographical distribution of birds is likely to prove the most absorbing. For this purpose the splendid Collection of skins amassed by Mr. Hume will be invaluable, for in most instances the specimens in the Hume collection trace out definitely the range of each species, and Mr. Oates has shown great talent in condensing into his limited space the large amount of material which was at his command. It is, in fact, impossible to speak too highly of the way in which he has performed his task.

The volume before us is profusely illustrated with woodcuts, which will undoubtedly be of great service to the student in enabling him to identify the species of birds which are to be met with in India. These woodcuts are, almost without exception, well executed, and are the best specimens of ornithological work which we have seen from the pencil of Mr. Peter Smit. We are not quite able to grasp the plan on which the names of Indian localities have been altered in the present book to bring them into a recognized system of correct orthography, but we suppose that there is some sound reason for the changes. If, however, our old friend "Mooleyit" is to become "Muleyit," and "Malewoon" to become "Malawun," why does not "Masuri" take the place of "Mussoorie"? Surely it is pedantic to alter the specific name of "nipalensis" to "nepalensis," because it suits modern notions to speak of "Nepal" instead of "Nipal." As this mode of orthography does not appear in any of Mr. Oates's previous writings, we suppose that the editor is responsible for the changes in the spelling of the names of places. We would gladly adopt a complete method of spelling the names of Indian localities, but that adopted in the present work seems neither one thing or the other.

It was a happy idea of Mr. Oates's to issue the new edition of Mr. Hume's "Nests and Eggs of Indian

Birds" in volumes of simultaneous issue with his volumes of birds. This egg-book of Mr. Hume's is one of the best oological works ever published, and has long been out of print. A good deal of the additional matter which Mr. Hume had accumulated for a second edition, was stolen by a dishonest servant, and sold for waste paper in the Simla Bazaar, but enough has remained to enable Mr. Oates to put before us a very interesting record of the breeding habits of Indian birds; and if any tribute be wanted to Mr. Hume's energy and ability, the reader has but to refer to the present work, to study the oological records of the best circle of field-ornithologists which ever rallied round the central figure of any zoologist. The portraits of naturalists who have contributed to the development of our knowledge of Indian birds lend an additional interest to Mr. Oates's volume on the "Nests and Eggs of Indian Birds."

R. BOWDLER SHARPE.

EPHEDRA.

Die Arten der Gattung Ephedra. Von Dr. Otto Stapf. Pp. 112, 1 Map and 5 Plates. (Vienna: R. Tempsky, 1889.)

EPHEDRA is one of the three genera of the small Gymnospermous order Gnetaceæ, the two others being Gnetum and Welwitschia, that most curious of all gymnospermous plants. Ephedra is a type of remarkable habit, specially modified, though in a different way from Welwitschia, to inhabit the dry and sandy regions of the world. It has shrubby stems, with copious slender, whip-like, straight or turning branches, foliar organs and flower-wrapper reduced to a minimum, unisexual mostly dioicous flowers in small catkins with dry imbricated scales, the female catkins containing one or two flowers only, and the males several, with from two to eight stamens with the filaments usually joined in a column. The species are numerous and difficult of determination, partly because the leaves are nearly suppressed, partly because the stems of all the species are very similar, and that it is needful to have both staminate and pistillate flowers to study before any given plant can be determined confidently.

The map shows clearly at a glance the geographical range of the genus. It surrounds the basin of the Mediterranean, climbs the lower levels of the Central European Alps, attains its highest development in Central Asia, reaching southward to the north of India and all through Arabia, northward to Lake Baikal and the Ural Mountains, and eastward to the western provinces of China; and reappears in the New World—in North America in California and Mexico, and in South America in the Andes and over a wide area south of the tropic from Chili across to Buenos Ayres. Though spread so widely over extra-tropical South America, it does not reach either the Cape or Australia, where the climate and soil seem so suitable for it. None of the single species have a very wide range, but it is one of the instances where a well-marked, sharply isolated generic type is represented in many different geographical areas by distinct specific types.

The present monograph is one of the best and most complete works of the kind that have lately appeared. It is extracted from the second part of the sixteenth volume of the *Denkschriften der Mathematisch-Naturwissenschaftlichen* class of the Kaiserlichen Akademie der Wissenschaften in Vienna. Dr. Stapf is one of the officials of the Botanic Garden of the University of Vienna, and has had the advantage of full command of material, both in the way of specimens and books. Two of the plates and a large proportion of the letterpress are devoted to the anatomy and morphology of the vegetative and reproductive organs of Ephedra. In the structure of the woody bundles Gnetaceæ establish some links of transition between Coniferæ and the typical Dicotyledons. Ephedra approximates in some points towards Casuarina. In the veining of its well-developed leaves Gnetum recedes from the ordinary Gymnospermous type. In Ephedra there is an unmistakable perianth to the male flower, but the homology of the outer wrapper of the seed is not so clear. Then follows the systematic portion of the monograph. Dr. Stapf admits twenty-eight certain and three imperfectly-known species, and for each of these he gives a diagnosis, a figure showing its essential characters, an extended description, and a full account of its synonymy and geographical distribution. He makes three sections, Alatæ, Asarea, and Pseudo-baccatæ, dependent mainly upon whether the seed is fleshy in a mature state, or dry and furnished with a wing. Then follows a list of local names, and a very full list of the books in which the genus is noticed, extending from Gerarde and Ray down to the present time. The monograph is one that deserves to be studied carefully, both by structural and systematic botanists.

J. G. B.

OUR BOOK SHELF.

Geological Mechanism; or, An Epitome of the History of the Earth. By J. Spottiswoode Wilson, C.E. (London and Manchester: John Heywood, 1890.)

THE nature of this little work of 135 pages will be best indicated by a brief statement of its contents. The book is divided into three portions of not very unequal length.

The first of these is "autobiographical," and relates, with much circumstance, the author's adventures at the Geological Society and Club, where, on the invitation of the late Sir Roderick Murchison, he read a paper in the year 1854. This is followed by an account (his own) of the causes which led to a disagreement between himself and the leaders of an exploring expedition of which he had been appointed a member. This part of the book is relieved from the charge of being prosaic, however, by the introduction of some very remarkable, and undoubtedly original verses.

Having devoted more than forty pages to himself, the author has left for the earth little more than fifty pages more; and in this space he contrives to dispose of a great number of highly important problems, beginning with "intelligence supreme; the nebular theory of Laplace; hypothesis of incandescence; theory of the crystalline rocks; hypothesis of metamorphism," &c.; and finishing up with "the lunar, magnetic, and solar tides; the progressive desiccation of the atmosphere and earth; the change of time; and the theory of creation."

Comprehensive as is this portion of the book, however, the author still finds much to put into his third part, or appendix—such as, "tails or atmospheres of planets and comets; the magnetic pole and change of climate; the magnetic tide of the atmosphere, &c." As in the first part he rose into poetry, here, in the appendix, he

soars into the realms of prophecy, and tells us about the climate which may be expected in these islands in the years 1970, 2020, and 2130!

The author assures us that he writes especially for civil engineers, and is not careful to conceal his contempt for "prominent men in other branches of science" and their opinions. But as there are some works "profitable for instruction," so there are others calculated to afford amusement; and it is very hard indeed that civil engineers should have a monopoly of all the fun that is to be got out of this one.

The Scenery of the Heavens. By J. E. Gore, F.R.A.S. (London: Roper and Drowley, 1890.)

THE title of this work is so suggestive of pictures that one cannot help feeling disappointed with the limited number of illustrations, especially as the book is designed for general readers. We look in vain, for example, for representations of Saturn and Mars, solar prominences, and many other celestial objects, of which no descriptions can convey so much to the mind as good illustrations. Some of the illustrations are reproduced more or less faithfully from photographs by Mr. Roberts and the Brothers Henry, but we regret to note that the wonderful photograph by Mr. Roberts of the Great Nebula in Orion is not amongst these. We may suggest also that in future editions some account be given of the instrument which reveals to us the greater part of the "scenery of the heavens."

On the whole, the text is excellent, and will no doubt greatly interest the general reader. There is, however, a very loose statement on p. 24—namely, "if we assume that the attraction of gravitation at the earth's equator is 32·2 feet, we have the accelerating force of gravity on the sun equal to 895 feet per second." One of the most notable features of the book is the large number of poetical selections having reference to astronomical phenomena. The book contains a good deal of information, in some cases perhaps too much to serve the avowed purpose of the author, unless his readers intend to become amateur observers. The long lists of red stars, doubles, variables, and star clusters, for example, are much too detailed for general readers, although not sufficiently so for regular observers. The chapter on variable stars, as might be expected from Mr. Gore, is especially good. There is also an excellent chapter on shooting-stars, by Mr. Denning, who is eminently fitted for such a task.

We may remind Mr. Gore that probably no one now supposes that the so-called "gaseous" nebulæ consist of nitrogen (pp. 197, 206), and that the structure of the Great Nebula in Andromeda as revealed in Mr. Roberts's photograph indicates that the nebula is probably not "a vast cluster of very small stars placed at an immense distance from the earth" (p. 204).

No attempt is made to touch upon any theoretical astronomy, and the scope of the book is therefore correctly described by the title.

A Trip through the Eastern Caucasus. By the Hon. John Abercromby. (London: Edward Stanford, 1889.)

IS it worth while for a traveller to make a six weeks' tour the subject of a book? Probably most people would answer promptly and emphatically, No; but any one who reads Mr. Abercromby's work will see that the reply may be wrong, and that everything depends on the nature of the scenes visited, and on the traveller's ability to give an account of his impressions. In the course of six weeks Mr. Abercromby twice crossed the main chain of the Caucasus by passes which are little used except by natives. He was fortunate enough to secure, through the instrumentality of Prince Dondukoff Korsakoff, the Governor-General of the Caucasus, a circular letter in Russian and Arabic to all in authority wherever he might wish to go. This, he says, acted like a charm, securing for him at every place the utmost hospitality. He had, therefore, the best possible opportunities of seeing what he desired to see, and of forming just opinions as to the characteristics of the people whom he visited.

Particularly good is his description of the strange village called Kubächi, in which there was at one time a flourishing school of the higher kinds of artistic craftsmanship. The village is "a long, narrow, extremely compact agglomeration of houses, built on the southern face of a very steep slope with a shallow ravine on both sides." A high round tower, commanding a wide view, stands at the top. All the roofs are flat, and, seen against the sky, the profile of the village is not unlike "a gigantic staircase." Before reaching Kubächi, Mr. Abercromby heard all sorts of wonderful stories about the inhabitants, and was assured that they were of Frankish origin. He found that there was nothing specially European-looking in the type of face either of the men or women. They appeared to him "quite like the Lesgians, though milder in their manners, and less wild-looking." Their speech has no sort of relation to the Indo-European languages, but belongs to the Lesgian family. There are in the village many sculptured stones and other relics of a period when the workers of Kubächi had a genuinely artistic impulse; and of these remains Mr. Abercromby gives a remarkably clear and attractive account. Not less interesting in its way is his description of the extraordinary wall of Derbend, which, according to the current native belief, is 3000 years old. For this idea there is of course no real foundation. Mr. Abercromby, with the enthusiasm of a thorough antiquary, investigated this structure with the greatest care, and even readers who are not generally attracted by archæological research will find much to please them in his narrative. Altogether, the work is fresh and bright, and we recommend it to the attention of those who find in good works of travel intellectual refreshment and stimulus.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Royal Society's Catalogue of Scientific Papers: a Suggested Subject-Index.

THE method advocated by Mr. J. C. McConnel (NATURE, February 13, p. 342) would undeniably be feasible. But I should pity the fellow-craftsman who should have to carry it out. The idea of numerical subdivision has been worked out by Prof. Dewey with great ingenuity and industry in his "Decimal Classification and Relative Index," 1885. We find, on referring to p. 31, that 016·9289551 will indicate the "Bibliography of Persian poets." Natural science occupies a place from 500–600, and does not seem to have been as yet reduced to an equal degree of elegant simplicity, for the subject of "observing chairs, &c.," is merely denoted by 522·28.

After this it does not seem over bold to pronounce the result one of the most amusing things in cataloguing literature. It is, however, surpassed by Mr. J. Schwartz's "King Aquila's Library," in which the system is fairly demolished. But the London inquirer into the actual working of such a cumbrous device may gain a useful hint by noting that at the Guildhall Library there is an alphabetical index to these totally unnecessary numbers. Indeed, one is found in Prof. Dewey's own book, and would, of course, be an absolute necessity in the proposed case.

No, a good subject-index can be constructed on much simpler lines. See, for example, Poole's "Index to Periodical Literature," which includes in its first supplement (1882–87) some 1090 volumes (indexed in 483 pages). Another example may be found in the subject-index at the end of the "List of Books of Reference in the British Museum Reading Room," 1889. In this some twenty thousand volumes are included, which would

lead one to suppose that the size Mr. McConnell suggests is ample, not to say generous. I had hitherto supposed that a scientific writer does not necessarily treat of a fresh subject each time he writes.

Might I add that an index is not a pedigree or diagram, any more than a gazetteer is the same thing as a map? I fear that to mix up such distinct things would merely introduce an altogether needless difficulty. A CATALOGUER.

The Period of the Long Sea-Waves of Krakatão.

IN connection with the great explosion at Krakatão at 10 a.m. on August 27, 1883, a great wave was generated, which at Batavia, 100 miles distant, reached a height of 7½ feet above the ordinary sea-level. It was followed by a fairly regular series of fourteen waves, at intervals of about two hours, gradually diminishing in height. Captain Wharton, who writes this part of the Royal Society Report, is much puzzled by the long period. He says :—"If the wave was caused by any sudden displacement of the water, as by the falling of large masses of ejected matter and huge fragments of the missing portion of Krakatão, or by the violent rush of steam from a submarine vent through the water, it is hardly to be conceived that two hours would elapse before the following wave, the second of the series, started after it. . . . If, however, upheaval of the bottom of the sea, more or less gradual, and lasting for about an hour, took place, we should have a steady long wave flowing away from the upheaved area, which as it approached the shore would be piled up considerably above its normal height. Thus these waves of long period would be set up. . . . The water would flow back on the motion ceasing."

I do not understand how the series of waves would be produced by the sea-bottom being upheaved in the manner described. When the upheaval ceased, the water would probably flow back, and, after the centre of disturbance was reached, a second wave would be generated. But there would be no reason for the water flowing back a second time, and no more waves would be generated. Further, in another part of the Report, we find Prof. Judd expressing the opinion that no upheaval has taken place (p. 25).

Another explanation has occurred to me, which seems satisfactory. Let us assume, with Prof. Judd, that the first wave was due to a great quantity of fragments falling into the sea. This wave would be reflected by the shores of the Straits several times backwards and forwards, each time giving rise to a fresh disturbance, travelling out towards Batavia through the narrow opening to the east. Opposite Krakatão both on the northern and on the southern shore of the Straits is a great bay. The time a wave would take to travel from Krakatão to the head of the bay on the north is given by Captain Wharton at sixty-one minutes, and the distance to the head of the other bay is much the same. This agrees very well with the two-hour period. Moreover the first disturbance at Batavia would be a rise of the water, which was the case.

In a similar way some of the short periods observed at distant stations may have been due to peculiarities of the channels in which the tide gauges were placed.

Hotel Buol, Davos. JAMES C. M. MCCONNEL.

The Distances of the Stars.

YOUR note of Prof. Eastman's address to the Philosophical Society of Washington in your columns of February 13 (p. 351) raises some questions of interest on which I think the Professor is mistaken.

As regards the nearness of particular stars, there are several indications which astronomers have sought to verify by observation and computation. One of these is brightness; a second is large proper motion, and a third is a binary system easily separated by the telescope (especially if the period is comparatively short). Some persons have also supposed that red stars, variable stars, &c., are nearer than most of their neighbours. Stars possessing one or more of these characteristics have been selected for parallax measurements.

One of these characteristics being brightness, almost every bright star in the northern hemisphere and a good many of those in the southern have been at one time or another measured for parallax. But no one has attempted to measure the parallax of *all* stars of the third, fourth, fifth, or sixth magnitudes. Astronomers have selected from among these stars those which afford some striking indication of nearness, such as the great proper motion of 61 Cygni. If, therefore, we take the parallaxes arrived at in this manner for comparison, we are comparing the results attained for *all* stars of the first magnitude with those attained for a small number of exceptional stars of the fifth or sixth.

How far Prof. Eastman's data are otherwise trustworthy I need not consider. I may refer your readers to a very full list of paraxalles hitherto determined, published by Mr. Herbert Sadler in the February number of *Knowledge*, by which it will appear how discordant and untrustworthy these results are. But the exceptional character of Prof. Eastman's faint stars is sufficiently evident from the table itself. His first group, with mean magnitude 5·57, has a mean proper motion of 4″·93; the second group, with a mean magnitude 5·59, has a mean proper motion 2″·33. Surely Prof. Eastman does not mean that the average proper motion of stars of the magnitude 5·58 is 3″·63. There is not one star in a hundred of this degree of faintness which possesses such a proper motion as this. W. H. S. MONCK.

Dublin, February 15.

P.S.—It is possible that a sphere enclosing the thirty nearest stars to us would include more faint stars than bright ones; but I think it certain that it would not include as large a percentage of fifth magnitude stars as of first magnitude stars. The first magnitude stars do not exceed twenty, and a few of them seem to be very distant. The fifth magnitude stars are reckoned by hundreds, and a few of them are comparatively near.

The Longevity of Textural Elements, particularly in Dentine and Bone.

WHATEVER views we may take of the theories of Weismann, which at present occupy the attention of biologists, they may be hailed as giving new directions to research, and one of the subjects about which his allusions will probably lead to further inquiry is the length of time during which textural elements continue individually. I have used the word longevity at the top of this letter; but, perfectly admitting the justice of Weismann's criticism—that division into two, each of which is a unity like the first, is not death—I feel driven to the dire necessity of inventing a new word, *permanunity*, to denote permanence without division; and it is of such permanence or longevity of the undivided unit that I wish to note a circumstance which has recently presented itself to my mind.

Every anatomist is aware that the living elements of dentine are nucleated corpuscles with elongated branches, which are embedded in the matrix, and lengthen as the dentine increases in thickness, while the corpuscles themselves retire inwards, remaining at the boundary of the lessening pulp-cavity. The continuity of the tubes containing these fibres furnishes, as soon as one thinks of it, convincing proof that they are the same branches and the same dentine-corpuscles which are found when the dentine begins to be deposited and when it is completed. But the dentine begins in childhood, and may go on increasing in thickness in old age, with its tubes still continuous, though losing their regularity of position. Therefore, dentine-corpuscles continue alive and without division through the greater part of the life of the organism.

The interest of this is exceedingly great, if the relation of dentine to bone be considered. Bone has a matrix similar to dentine, and has branched corpuscles; but the bone-corpuscles differ from the dentine-corpuscles in becoming completely embedded in the mineralized matrix, without any attempt to retire from it, and thus come to have branches on every side. Under the microscope one can see in compact bony tissue that there is a continual reabsorption and redeposition of bone going on; and these alternating processes are brought about in a way which is easy to understand, though very generally misapprehended. In consequence, probably, of the very pressure exercised by the bony deposit on the corpuscles, the corpuscles are excited to absorb it; and one sees absorption spaces commencing sometimes in the centres of haversian systems, and sometimes in individual lacunæ. The activity thus aroused in the corpuscles causes them to enlarge and to attempt proliferation; which being in the first instance modified by their close surroundings leads to their being converted into large multinucleated masses, the so-called giant-cells or osteoclasts. But when a greater amount of room has been obtained, these masses separate up into corpuscles with one nucleus each, bone-corpuscles or osteoblasts, which, arraying themselves around the cavity, initiate the formation of new

concentric laminæ of bone. Thus it is certain that the permanunity of the bone-corpuscle is very inconsiderable indeed. It may be difficult to define it exactly, but a general consideration of the rapid changes in the shafts of young bones leads me to think it probably much less than a year.

There is thus a very surprising contrast between the undivided persistence or permanunity of a bone-corpuscle and that of a dentine-corpuscle, which is in various respects so similar to it. While there are numerous instances of very short-lived corpuscles in the body, I am not aware that until now proof has been offered of the persistence of any living tissue-elements throughout the life of the organism. JOHN CLELAND.

Some Notes on Dr. A. R. Wallace's "Darwinism."

I HAVE just read this most interesting work, "Darwinism"—seeming to me the clearest and most useful account of the Darwinian theory of evolution ever yet published—and while reading it I have made note of a few matters which I may, perhaps, be allowed to touch on here.

On p. 43 are quoted the numbers of varieties of the two snails, *Helix nemoralis* and *H. hortensis*, enumerated by a French author—no doubt Moquin-Tandon. These numbers, however, fall far below those actually known at the present day. These snails vary in many ways, but taking variations of *banding alone*, I know of 252 varieties of *H. nemoralis*, and 128 of *H. hortensis*.

To further illustrate the extreme variability of the Mollusca, take the varieties of land and freshwater Mollusca found in the British Islands. Of the 88 species of land shells we have 465 named varieties, and of the 46 species of British freshwater shells are 251 varieties. So that, excluding probable synonymy, we have about 5 named varieties in Britain to every species of inland mollusc.

In the same way, the numbers of *Rosa* and *Rubus* quoted on p. 77 are below the mark. Of *Rosa canina*, 33 varieties are known in the British Islands, while the British *Rubi* number 63 supposed species.

A good example of a species "occupying vacant places in nature" (p. 110), is afforded by the little mollusc *Cæcilianella acicula*, which is simply organized, and lives in great numbers underground (*vide Naturalist*, 1885, p. 321).

The true cause (as it seems to me) of the variability of freshwater species seems hardly indicated on p. 110. All freshwater productions, except those inhabiting large river basins (as the Mississippi), present these peculiarities—they are exceedingly variable and plastic, so that we get few but polymorphic species. Now, for the successful spread of freshwater organisms, it is necessary that they should be *plastic*, to adapt themselves to the new environment of every pond or river, and the varieties thus required must *not become fixed species*, because it is their very changeability under new environment that makes them successful in the struggle for existence and increase. Freshwater forms migrate more than is commonly supposed, and the contents of any pond or river are ever varying. Hence the necessities I have indicated. These points are exceptionally clear in the case of the *Unionidæ* of Europe and North America (see *Science Gossip*, 1888, pp. 182–184).

Colorado presents an exception to the rule (p. 112), that two species of *Aquilegia* are rarely found in the same area. In Colorado we have *five* columbines, viz. *A. formosa*, *A. chrysantha*, *A. brevistyla*, *A. cærulea*, and *A. canadensis*. But *A. cærulea* is the only one that can be called abundant.

On p. 139, it is stated that specific characters are essentially symmetrical. Yet the ocelli and spots on the butterflies of the families *Satyridæ* and *Lycænidæ* surely afford specific characters, and they are frequently asymmetrical (see *Entomologist*, 1889, p. 6).

On p. 151, we are told that in Ireland hardly one of the land molluscs has undergone the slightest change. This is not quite true, as the following forms seem to be peculiar to Ireland: *Arion ater* var. *fasciata*, *Geomalacus maculosus* vars. *allmani*, *verkruzeni*, and *andrewsi*, *Limax arborum* var. *maculata*, *L. arborum* var. *decipiens*, *Succinea vitrea* var. *aurea*, and *S. pfeifferi* var. *rufescens*. But these peculiar forms are not more numerous (but less so) than would be found in almost any continental area of equal size.

The theory (p. 206) that a recent change of food-plant has to do with the presence of green and brown varieties of the larva of *Macroglossa stellatarum* seems hardly tenable, as so many larvæ of different species and genera vary in the same manner.

I have thought (*Ent. Mo. Mag.*, 1889, p. 382) that asymmetrical variation in insects occurred most often on the left side. On p. 217 it appears that the same thing occurs in some Vertebrata.

On p. 230 the idea of environment directly influencing the prevalent colours of organisms is put aside as improbable. Yet it has seemed that moisture was the cause of a certain phase of melanism, especially among Lepidoptera. Evidence bearing on this point has been given during the last few years in the *Entomologist*.

The land shells on the small islands off the coast of Kerry, Ireland, are pale in colour, as I have recorded in Proc. South London Entom. and N.H. Soc. for 1887, pp. 97–98.

The point on p. 233, about the conspicuous colours of the Aculeate Hymenoptera, seems open to question. In temperate regions, at least, the *Aculeata* are mostly of very dull colours—as the *Andrenidæ*, many of the *Apidæ*, and hosts of others. Even the brilliant green *Agapostemon* flies among bright green foliage and yellow flowers, and is not very conspicuous when alive in its native haunts. On the other hand, the non-aculeate *Chrysididæ* and *Chalcididæ* are often exceedingly brilliant in colouring.

It seems quite doubtful whether the abundance and wide distribution of *Danais archippus* (p. 238) is due to immunity from parasites, &c., while its migratory habits are a quite sufficient explanation of the facts. Besides, it has at least one parasite—the *Pteromalus archippi*.

The "progressive change of colour" (p. 298) is well illustrated by the change from yellow to scarlet exhibited by so many groups of species. Scarlet species nearly always occasionally revert to yellow, and there are generally yellow species in the same genus. For details see Proc. South Lond. Ent. and N. H. Soc. for 1887.

Yellow flowers (see p. 316) seem the most attractive to insects in Colorado, and Mr. F. W. Anderson tells me that the same is the case in Montana. From reasons given in *Canadian Entomologist*, 1888, p. 176, I am of the opinion that insects cannot distinguish red from yellow.

It has seemed to me (see p. 359) that the agency of wind in distributing insects is greatly exaggerated. I believe whirlwinds may be most important as distributing agents, but ordinary gales less so. Many species of insects migrate, but usually *during calms*. Also (p. 360) the opinion that insects are often carried to the summits of mountains by winds seems to me without sufficient support. Many species of insects *live* only or habitually at high altitudes, and their presence there is no proof that they were carried there by winds, especially when they are specifically distinct from the species of lower regions. *Plusia gamma*, on the summit of Mont Blanc, is not very remarkable, as the moth is a great wanderer, and quite capable of finding its own way to high altitudes. Finally, I believe winds very rarely blow *up* mountain slopes. I have lived some time at the base of the great Sangre de Cristo Range in Colorado, and although violent winds blow *down* very frequently, I have *never observed an upward wind*, and residents whom I have questioned are unanimous in saying that they have never known a strong wind blow *up* the mountains. And the way the trees are bent and twisted at timber-line (11,500 feet), often with only branches on the side towards the valley, well indicates the direction of the winds.

I think, perhaps, the scarcity of Monocotyledons in the Rocky Mountains (p. 401) as compared with northern regions, is more apparent than real—the difference indicated in the books being due to the fact that the western grasses are not so well known as the eastern ones. Ferns are rarer on continents than on islands, and the dryness of the Rocky Mountain region is unfavourable to them.

A good instance of the effect of environment (see p. 419) recently came under my notice. The polymorphic snail *Helix nemoralis* was introduced from Europe into Lexington, Virginia, a few years ago. Under the new conditions it varied more than I have ever known it to do elsewhere, and up to the present date 125 varieties have been discovered there. *Of these, no less than 67 are new, and unknown in Europe, the native country of the species!* The variation is in the direction of division of the bands. An incomplete list of these varieties is given in *Nautilus*, 1889, pp. 73–77.

It seems doubtful (see p. 433) how far prickles are a protection from snails and slugs. I found prickles in the stomach of *Parmacella* (a slug), as recorded in *Journal of Conchology*, 1886, pp. 26–27.

It is a minor matter, but it seems a pity that the nomenclature of the species in a standard work like "Darwinism" should not be scrupulously exact. Thus (p. 17), "*Phalæna*" *graminis* should be *Charæas graminis*. "*Helisonia*" (p. 44) should be *Helisoma*, and it is only a section, or subgenus, of *Planorbis*. On p. 235, "*filipendula*" and "*jacobeæ*" should read *filipendulæ* and *jacobææ*. "*Sphinx fuciformis*," of Smith and Abbott (p. 203), is really *Hemaris diffinis*, while on p. 204, "*Sphinx*" *tersa* is a *Chærocampa*, and "*Sphinx pampinatrix*" is *Ampelophaga myron*.

T. D. A. COCKERELL.

West Cliff, Custer Co., Colorado, January 22.

A Formula in the "Theory of Least Squares."

SOME time ago, having had occasion to investigate the relation between $\Sigma(x^2)$ and $\Sigma(v^2)$ in the "Theory of Least Squares," I found a simple formula which connects them, and which I have never seen given in any of the text-books on the subject. I inclose it, and hope it is worth publishing in your journal.

University of Toronto, February 1. W. J. LOUDON.

Let a number of observations be made on a quantity whose true value is T. If these observations be represented by M_1, M_2, M_3, . . . M_n, then the most probable value is A, the arithmetic mean, and $A = \frac{\Sigma(M)}{n}$. If, moreover, the true errors be denoted by x_1, x_2, x_3, . . . x_n, and the residuals by v_1, v_2, v_3, . . . v_n, then $\Sigma(v) = 0$ by the definition of the arithmetic mean. It is required to find a relation between $\Sigma(x^2)$ and $\Sigma(v^2)$. We have—

$$\begin{array}{lcl} x_1 = T - M_1 & \text{and} & v_1 = A - M_1 \\ x_2 = T - M_2 & & v_2 = A - M_2 \\ x_3 = T - M_3 & & v_3 = A - M_3 \\ \&c., & & \&c., \end{array}$$

from which $\Sigma(v) = 0$.

$\therefore$ equating equal values of M_1, M_2, M_3, . . . , &c., we get—

$$\left.\begin{array}{l} T - x_1 = A - v_1 \\ T - x_2 = A - v_2 \\ T - x_3 = A - v_3 \\ \&c. \end{array}\right\} \quad \text{or} \quad \begin{array}{l} x_1 = v_1 + T - A \\ x_2 = v_2 + T - A \\ x_3 = v_3 + T - A \\ \vdots \end{array}$$

$$\text{and adding} \quad \Sigma(x) = \Sigma(v) + n(T - A)$$
$$\text{and} \quad \Sigma(v) = 0.$$
$$\therefore \quad \Sigma(x) = n(T - A) \quad . \quad . \quad (1)$$

Again—

$$\begin{array}{l} x_1 = v_1 + T - A \\ x_2 = v_2 + T - A \\ \&c. \end{array}$$

$\therefore$ squaring, we have—

$$\begin{array}{l} x_1^2 = v_1^2 + 2v_1(T - A) + (T - A)^2 \\ x_2^2 = v_2^2 + 2v_2(T - A) + (T - A)^2 \\ x_3^2 = v_3^2 + 2v_3(T - A) + (T - A)^2 \\ \&c. \end{array}$$

$$\therefore \quad \Sigma(x^2) = \Sigma(v^2) + 2\{\Sigma(v)\}\{T - A\} + n(T - A)^2$$

But $\Sigma(v) = 0$; and from (1), $T - A = \frac{\Sigma(x)}{n}$;

$$\therefore \quad \Sigma(x^2) = \Sigma(v^2) + n\left\{\frac{\Sigma(x)}{n}\right\}^2$$

$$\Sigma(x^2) = \Sigma(v^2) + \frac{\{\Sigma(x)\}^2}{n}.$$

This is the exact formula; from which it may be seen that, as positive and negative errors are equally likely, a close approximation will be obtained by taking $\{\Sigma(x)\}^2 = \Sigma(x^2)$, neglecting $2\Sigma(xx^1)$.

And we obtain Gauss's formula—

$$\Sigma(x^2) = \Sigma(v^2) + \frac{\Sigma(x^2)}{n}, \quad \text{or} \quad \frac{\Sigma(x^2)}{n} = \frac{\Sigma(v^2)}{n - 1}.$$

Galls.

ADMITTING, with Prof. Romanes (NATURE, February 20, p. 369), the plausibility of Mr. Cockerell's view that galls may be attributed to natural selection acting on the plants directly, I beg leave to point out a very obvious difficulty—viz. the much greater facility afforded to the indirect action through insects, by the enormously more rapid succession of generations with the latter than with many of their vegetable hosts—oaks, above all.

Freiburg, Badenia, February 22. D. WETTERHAN.

The Cape "Weasel."

IN Prof. Moseley's account of his visit to the Cape of Good Hope ("Notes of a Naturalist on the *Challenger*," p. 153), the following sentence occurs:—"Again, there are tracks of the Ichneumon (*Herpestes*), called by some name sounding like 'moose haunt.'"

In Todd's "Johnson's Dictionary," 1827, we find: "*Mouse-hunt*, a kind of weasel;" two quotations being given:—(1) "You have been a mouse-hunt in your time" ("Romeo and Juliet," iv. 4). (2) "The ferrets and mouse-hunts of an index" (Milton, "Of Ref. in Engl.," B. 1).

Halliwell's "Dictionary of Archaic and Provincial Words" (1847) gives, on p. 564: "*Mouse hound, East.* A weasel." Halliwell denies the identity of this word with Shakespeare's mouse-hunt; and Nares ("Glossary") inclines to a similar view. But in any case it seems clear that Prof. Moseley's "moose-haunt" is a dialectical English form—mouse-hunt or mouse-hound; a general word for "weasel." E. B. TITCHENER.

3 Museum Terrace, Oxford, February 17.

The Chaffinch.

THE chaffinch sings almost throughout the year in this locality. The male bird never leaves us in winter like the female, and can be seen in large flocks daily. A singular circumstance that occurred here in December 1888 with regard to a chaffinch may be of interest. At one o'clock in the morning, during a gale, a chaffinch tapped at my study window. On this being opened, it flew into the room and roosted on a bookshelf; next morning it was liberated. This was repeated on two subsequent gales. Not only did it sing each time on being liberated, but all through the winter and spring it followed me about the garden, singing. E. J. LOWE.

Shirenewton Hall, near Chepstow, February 11.

ON THE NUMBER OF DUST PARTICLES IN THE ATMOSPHERE OF CERTAIN PLACES IN GREAT BRITAIN AND ON THE CONTINENT, WITH REMARKS ON THE RELATION BETWEEN THE AMOUNT OF DUST AND METEOROLOGICAL PHENOMENA.[1]

THE portable dust-counting apparatus, with which the observations given in the paper were taken, was shown to the meeting. The apparatus, which was described in a previous communication to the Society, is small and light. It is carried in a small sling-case measuring 8 × 5 × 3 inches. The stand on which it is supported when in use packs up, and forms, when capped with india-rubber ends, a handy walking stick, $1\frac{1}{4}$ inch in diameter and 3 feet long. No alterations have been made in the original design, and the silver mirrors which at first gave trouble and required frequent polishings, have been used every day for two or three weeks without requiring to be polished, when working in fairly pure country air.

With the paper is given a table containing the results of more than two hundred tests made with the apparatus. In addition to the number of dust particles there is entered in the table the temperature and humidity of the air, the direction and force of the wind, and the transparency of the air at the time.

The first series of observations were made at Hyères, a small town in the south of France, situated about 2 miles from the Mediterranean. The observations were made on the top of Finouillet, a hill about 1000 feet high. The number of particles on different days varied here from 3550 per c.c. to 25,000 per c.c., the latter number being observed when the wind was blowing direct from Toulon, which is distant about 9 miles.

Cannes was the next station, the observations being

[1] Abstract of Paper read before the Royal Society of Edinburgh, on February 3. Communicated by permission of the Council of the Society.

made on the top of La Croix des Gardes. The number here varied from 1550 per cubic centimetre, when the wind was from the mountainous districts, to 150,000 when it came from the town.

At Mentone the number varied from 1200 per cubic centimetre in air from the hills to 7200 in the air coming from the direction of the town.

Tests were made of the air coming towards the shore from the Mediterranean at three different places—at La Plage, Cannes, and Mentone. In no case was the amount of dust small. The lowest was 1800 per cubic centimetre, and the highest 10,000 per cubic centimetre.

Observations were also made at Bellagio and Baveno, on the Italian lakes. At both stations the number was always great—generally from 3000 to 10,000 per cubic centimetre. This high number was owing to the wind, during the time of the observations, being light and southerly—that is, from the populous parts of the country. Smaller numbers were observed at the entrance to the Simplon Pass and at Locarno, at both of which places the wind blew from the mountains when the tests were being made.

A visit of some days was made to the Rigi Kulm. On the first day, which was May 21, the top of the mountain was in cloud, and the number of particles was as low as 210 per cubic centimetre. Next day the number gradually increased to a little over 2000 per cubic centimetre, after which the number gradually decreased till on the 25th the number was a little over 500 per cubic centimetre at 10 a.m. On descending the mountain to Vitznau the same day, the number was found to be about 600 per cubic centimetre at midday, and in the afternoon at a position about a mile up the lake from Lucerne the number was 650 per cubic centimetre.

Most of the observations taken of Swiss air show it to be comparatively free from dust. This is probably owing to the vast mountainous districts extending in many directions. It is thought that much of the clearness and brilliancy of the Swiss air is due to the small amount of dust in it.

Owing to the kindness of M. Eiffel an investigation of the air over Paris was made on the Tower on May 29. The day was cloudy and stormy, with southerly wind. Most of the observations were taken at the top of the Tower, above the upper platform, and just under the lantern for the electric light. The number of particles was found to vary very rapidly at this elevation, showing that the impure city air was very unequally diffused into the upper air, and that it rose in great masses into the purer air above. Between the hours of 10 a.m. and 1 p.m. the extreme numbers observed were 104,000 per cubic centimetre and 226 per cubic centimetre. This latter number was obtained while a rain-cloud was over the Tower, and, as the shower was local, the descending rain seems to have beaten down the city air. The low number continued some time, and was fairly constant during the time required for taking the ten tests of which the above low number is the average.

The air of Paris was tested at the level of the ground on the same day, the observations being made through the kindness of M. Mascart in the garden of the Meteorological Office in the Rue de l'Université. The number on this day varied from 210,000 to 160,000 per cubic centimetre.

Very few tests have been made of the air of London. The air coming from Battersea Park, when a fresh wind was blowing from the south-west, on June 1, was found to vary from 116,000 to 48,000 per cubic centimetre. The numbers observed in cities are of no great value, as so much depends on the immediate surroundings of the position where the tests are made; so that, while no ow number can be observed, a very high one can always be obtained. Those recorded were taken where it was thought the air was purest.

Observations have been made in Scotland for periods of two or three weeks at three stations—namely, at Kingairloch, which is situated on the shore of Loch Linnhe, and about fourteen miles to the north of Oban, at Alford in Aberdeenshire, the observations being made at a distance of two miles to the west of that village, and at a situation six miles north-west of Dumfries.

At Kingairloch the number varied from 205 per cubic centimetre to 4000 per cubic centimetre. At Alford from 530 to 5700 per cubic centimetre, and at Dumfries from 235 to 11,500 per cubic centimetre. These three stations were in fairly pure country air—that is, pure as regards pollution from the immediate surroundings.

Tests were also made of the air on the top of Ben Nevis on August 1, when the number was found to be 335 per cubic centimetre at 1 p.m., and 473 two hours later. On the top of Callievar, in Aberdeenshire, on September 9, the number was at first 262, and rose in two hours to 475 per cubic centimetre.

The pollution of the earth's atmosphere by human agencies is then considered, and it is pointed out that, while on the top of the Rigi and in the wilds of Argyllshire air was tested which had only a little over two hundred particles per cubic centimetre, near villages the number goes up to thousands, and in cities to hundreds of thousands. The increase, though great, is shown not to be in proportion to the sources of pollution, and it is pointed out that part of this is owing to the impure stream of air being deepened as well as made more impure.

About 200 particles per cubic centimetre is the lowest number yet observed, but we have no means of knowing whether this is the lowest possible, or of knowing how much of this is terrestrial and how much cosmic, formed by the millions of meteors which daily fall into our atmosphere. Even in the upper strata there seems to be dust, as clouds form at great elevations.

The effect of dust on the transparency of the atmosphere is then discussed with the aid of the figure in the table. It is shown that the transparency of the atmosphere depends on the amount of dust in it, and that the effect of the dust is modified by the humidity of the air. With much dust there is generally little transparency, but it is pointed out that air with even 5000 particles per c.c. may be clear, if it is so dry as to depress the wet-bulb thermometer 10° or more. By comparing days on which there was the same amount of dust, it is seen that the transparency varied with the humidity on two days with the same amount of dust; but the one with a wet-bulb depression of 13° was very clear, while the other, with a wet-bulb depression of only 2°, was very thick.

To show the effect of the number of particles on the transparency, a number of days are selected on which the humidity was the same, when it is seen that when the wet-bulb was depressed 4°, with 550 particles the air was clear, medium clear with 814, but thick with 1900. From the table a number of cases are taken illustrating the dependence of the transparency of the air on the number of particles in it, and on the humidity, both dust and humidity tending to decrease the transparency. Humidity alone seems to have no influence on the transparency apart from the dust, but it increases the effect of the dust by increasing the size of the particles.

The modifying effect of the humidity is shown to be influenced by the temperature. The same wet-bulb depression which will give with a given number of particles a thick air at a temperature of 60° will give a clearer air if the temperature be lower. This is illustrated by examples taken from the table. The increased thickening effect accompanying the higher temperature will be due to the increased vapour-pressure permitting the dust particles to attach more moisture to themselves. These remarks all refer to what takes place in what is called dry air—that is, air which gives a depression of the wet-bulb thermometer.

The conclusion come to from the consideration of all the observations is that the dust in the atmosphere begins to condense vapour long before the air is cooled to the dew-point. It seems probable that in all states of humidity the dust has some moisture attached to it, and that, as the humidity increases, the load of moisture increases with it.

Another method of testing the condensing power of dust for water-vapour is then described. In working this method the dust is collected on a glass mirror, and its condensing power is determined by placing the mirror over a cell in which water is circulated, in the manner of a Dines hygrometer. The temperature at which condensation takes place on the dust and on a cleaned part of the glass is observed. The difference in the two readings gives the condensing power of the dust. One kind of dust artificially prepared was found to condense vapour just at the dew-point, while another condensed it at a temperature 17° above the saturation-point. The atmospheric dust was collected on the mirrors on the same principle as that used in the thermic filter described by the author in a previous paper, the dust being deposited by difference of temperature, the necessary heat being obtained by fixing the collecting mirrors on a window-pane. Dust was also collected by allowing it to settle on the plates. The atmospheric dust was found to condense vapour at temperatures varying from 1°·8 to 4°·5 above the dew-point. This condensing power of dust explains why glass such as that in windows, picture frames, &c., often looks damp while the air is not saturated; and in part it explains why it is so necessary to keep electrical apparatus free from dust, if we wish to have good insulation.

The constitution of haze is then considered. It is shown that in many cases it is simply dust, on which there seems to be always more or less moisture. But as what is known as haze is generally seen in dry air, the effect is principally due to dust.

Some notes from the Rigi Kulm are given, where "glories" and coloured clouds were seen. The condition of the transparency of the lower air as seen from the top of the mountain is discussed with the aid of the observations made by observers at the lower levels. These observations were kindly supplied by M. Bilwiller, of the Swiss Meteorological Office. The difference observed at the top of the mountain in the transparency of the air in different directions is shown to have been caused by a difference in the humidity of the air in the different directions. The variation in the number of particles on the top of the mountain is considered, and it is shown that the great increase in the number which took place on the second day was probably due to the valley air being driven up the slopes, reasons being given for this supposition. The colouring in clouds, and on scenery at sunrise and sunset, as seen from the tops of mountains and valleys, is remarked upon, and it is shown that there is reason for supposing that when seen from the lower level the colours will generally be the more brilliant and varied.

The relation of the amount of dust to the barometric distribution is then investigated—as to whether cyclonic or anticyclonic areas have most dust in them. It is shown that there is most dust in the anticyclonic areas. The interpretation of this, however, is shown to be that the amount of dust depends on the amount of wind at the time, and as there is generally little wind in anticyclonic areas, there is generally much dust. Diagrams are given showing by means of curves the amount of dust on each day, and also the velocity of the wind. The curves are found to bear a close relation to each other—when the one rises the other falls. The only exceptions to this are when the stations where the observations were made are not equally surrounded in all directions by sources of pollution. In that case, even with little wind, if it blows from an unpolluted direction the amount of dust is not great.

The increase in the dust particles which takes place when the wind falls, seems to point to a probable increase of the infection germs in the atmosphere when the weather is calm. As, however, the conditions are not quite the same, the organic germs being much larger than most of the dust particles, and settling more quickly, it may be as well, while accepting the suggestion, to refrain from drawing any conclusion.

In all the fogs tested, the amount of dust has been found to be great. This is shown to be what might now be expected from a consideration of the conditions under which fogs are formed. One condition necessary for the formation of a fog is that the air be calm. But when the air is calm both dust and moisture tend to accumulate, and the dust, by increasing the radiating power of the air, soon lowers its temperature and causes it to condense vapour on the dust and form a fog. The thickness of a fog seems to depend in part on the amount of dust present, as town fogs, apart from their greater blackness, are also more dense than country ones. The greater amount of dust in city air, by increasing its radiating power, it is thought, may be the cause of the greater frequency of fogs in town than in country air.

At the end of the paper some relations are pointed out between the amount of dust and the temperature at the time the observations were made, showing that when there was a large amount of dust there was also a high temperature; and some speculations are entered into as to the effect of dust on climate. But it is at the same time pointed out that the observations are far too few and imperfect to form a foundation for any important conclusi n on that subject.

In a short appendix is given the result of some tests made between January 23 and 29 of this year at Garelochead. During the gale on Saturday, the 25th, the number was rather under 1000 per cubic centimetre. On Monday, though the wind was still high, the number fell to about 250; and on Tuesday, when the wind had fallen and veered to the north, the number fell lower than had been previously observed. The number varied from a little over 100 to about 90 per cubic centimetre. On this day the air was remarkable for its clearness, the sun was very strong, and the evening set in with a sharp frost.

JOHN AITKEN.

P.S.—The author of the paper also showed at the same meeting of the Society the apparatus which have just been constructed from his designs for the Observatory on Ben Nevis. The apparatus has been constructed by the aid of a Government grant, obtained by the Council of the Scottish Meteorological Society, for the purpose of carrying on the investigation on the dust in the atmosphere at the top of Ben Nevis. Two complete sets of apparatus were shown. The one is the large laboratory form of the dust-counter, and is to be fixed, in the meantime, in the tower of the Observatory; the air being taken in to it by means of a pipe. The other is the small portable form of instrument, to be used when the direction of the wind is such as to bring the smoke of the Observatory towards the tower. This latter instrument has for a short time been in the hands of Mr. Rankin, one of the Ben Nevis observers, who has been practising with it near Edinburgh before beginning regular work at the Observatory.

A UNIFORM SYSTEM OF RUSSIAN TRANSLITERATION.

UP to the present time no one system of transliterating Russian names and titles into English has been generally adopted. Some of those most interested in the cataloguing and recording of Russian scientific literature have therefore arranged the following scheme in order to secure the general use of a system which will enable

those unacquainted with Russian, not only to transliterate from that language into English, but also to recover the original Russian spelling, and so to trace the words in a dictionary.

RUSSIAN-ENGLISH.

Roman. Capital.	Roman. Small.	Written. Capital.	Written. Small.	English equivalents.	Roman. Capital.	Roman. Small.	Written. Capital.	Written. Small.	English equivalents.
А	а	*А*	*а*	*a*	Ф	ф	*Ф*	*ф*	*f*
Б	б	*Б*	*б*	*b*	Х	х	*Х*	*х*	*kh*
В	в	*В*	*в*	*v*	Ц	ц	*Ц*	*ц*	*tz*
Г	г	*Г*	*г*	*gh*	Ч	ч	*Ч*	*ч*	*ch*
Д	д	*Д*	*д*	*d*	Ш	ш	*Ш*	*ш*	*sh*
Е	е	*Е*	*е*	*e*	Щ	щ	*Щ*	*щ*	*shch*
Ж	ж	*Ж*	*ж*	*zh*					
З	з	*З*	*з*	*z*	Ъ	ъ	*Ъ*	*ъ*	ʻ Not indicated at end of word.
И	и	*И*	*и*	*ī*					
І	і	*І*	*і*	*i*	Ы	ы	*Ы*	*ы*	*ui*
К	к	*К*	*к*	*k*					
Л	л	*Л*	*л*	*l*	Ь	ь	*Ь*	*ь*	ʻ Not indicated at end of word.
М	м	*М*	*м*	*m*	Ѣ	ѣ	*Ѣ*	*ѣ*	*ye*
Н	н	*Н*	*н*	*n*	Э	э	*Э*	*э*	*é*
О	о	*О*	*о*	*o*	Ю	ю	*Ю*	*ю*	*yu*
П	п	*П*	*п*	*p*	Я	я	*Я*	*я*	*ya*
Р	р	*Р*	*р*	*r*	Ѳ	ѳ	*Ѳ*	*ѳ*	*th*
С	с	*С*	*с*	*s*	Ѵ	ѵ	*Ѵ*	*ѵ*	*œ*
Т	т	*Т*	*т*	*t*	Й	й	*Й*	*й*	*ĭ*
У	у	*У*	*у*	*u*					

ENGLISH-RUSSIAN.

a	А	*ī*	И	*p*	П	*ui*	Ы
b	Б	*ĭ*	Й	*r*	Р	*v*	В
ch	Ч	*k*	К	*s*	С	*ya*	Я
d	Д	*kh*	Х	*sh*	Ш	*ye*	Ѣ
e	Е	*l*	Л	*shch*	Щ	*yu*	Ю
é	Э	*m*	М	*t*	Т	*z*	З
f	Ф	*n*	Н	*th*	Ѳ	*zh*	Ж
gh	Г	*o*	О	*tz*	Ц	ʻ	Ъ
i	І	*œ*	Ѵ	*u*	У	ʻ	Ь

With reference to some of the letters a few words of explanation are necessary.

gh is adopted in preference to *g* for г, since this letter is also the equivalent of *h* in such words as Гидра, which, if transliterated *gīdra*, would lose its resemblance to the word *hydra*, with which it is identical.

Although і and и have the same sound, and with a few rare exceptions the letter used in the original may be recognized by a simple rule, it is recommended that the latter should be distinguished by the sign ‒, since the use of the same English symbol for two Russian characters is objectionable.

The semi-vowels, ъ and ь, must be indicated when present, except at the end of a word, by the sign ʻ placed above the line; otherwise, the transliteration of two Russian characters might give the same sequence as one of the compound equivalents, and it would become difficult to trace the words in a dictionary.

As regards the compound equivalents, nine out of the twelve may be at once recognized, since *h* must always be coupled with the preceding, and *y* with the succeeding, letter.

Where proper names have been Russianized, it is better whenever possible to use them in the original form rather than to re-transliterate them; there is no reason why *Wales* should be rendered *Uelʻs*, or *Wight* written as *Uaĭt*. When a Russian name has a more familiar transliterated form, it is advisable to quote this as well as an exact transliteration with a cross reference.

The system will be adopted without delay in the following publications: the Catalogue of the Natural History Museum Library; the Zoological and Geological Records; the publications of the Royal Society, the Linnean, Zoological, and Agricultural Societies, and the Institution of Civil Engineers; the Mineralogical Magazine, and the Annals of Botany; and it is hoped that the system will be generally used.

An expression of grateful thanks is due to those who have assisted in the arrangement of this system by criticisms and suggestions; more especially to Madame de Novikoff and N. W. Tchakowsky.

The undersigned either accept the proposed system in the publications with which they are severally connected, or express their approval of the same:—

W. H. Flower, C.B. ... *Director, Natural History Museum.*
W. R. Morfill ... ... *Reader in Russian, &c., Oxford.*
F. Löwinson-Lessing ... *University, St. Petersburg.*
S. H. Scudder ... ... *U.S. Geological Survey.*
W. H. Dall ... ... *Smithsonian Institution.*
B. Daydon Jackson ... *Bot. Sec., Linnean Society.*
P. L. Sclater .. .. *Zoological Society.*
F. E. Beddard ... ... *Zoological Record.*
W. Topley ... ... }
C. Davies Sherborn ... } *Geological Record.*
I. Bayley Balfour ... ... }
S. H. Vines ... ... } *Annals of Botany.*
H. A. Miers ... ... *Index to Mineralogical Papers.*
J. T. Naaké.... ... ... *British Museum.*
B. B. Woodward ... ... *Natural History Museum Library.*
J. W. Gregory ... ... *Natural History Museum.*

THE BOTANICAL INSTITUTE AND MARINE STATION AT KIEL.

PROF. J. REINKE contributes to the *Botanisches Centralblatt* a very interesting account of the Botanical Institute at Kiel, and of the Marine Station attached to it, as far as they are employed for botanical researches.

The harbour of Kiel is remarkably favourable for the observation of marine Algæ and the investigation of their life-history. In brown seaweeds the immediate neighbourhood is exceedingly rich, being scarcely inferior in the number of species to any other spot on the coasts of Europe. One important order, the Dictyotaceæ, is

altogether wanting; but another very interesting order, the Tilopterideæ, is well represented. In green Algæ, the large Siphoneæ of the Mediterranean and other warmer seas are represented only by *Bryopsis*. Of red Algæ, the number of species and genera is inferior to that found in the Mediterranean or on the coasts of England and France; but almost all the different types of growth are well represented. Although the Baltic has, like the Mediterranean, no tides, the sea-level of Kiel harbour falls so considerably with a south wind, that many littoral Algæ are then completely exposed.

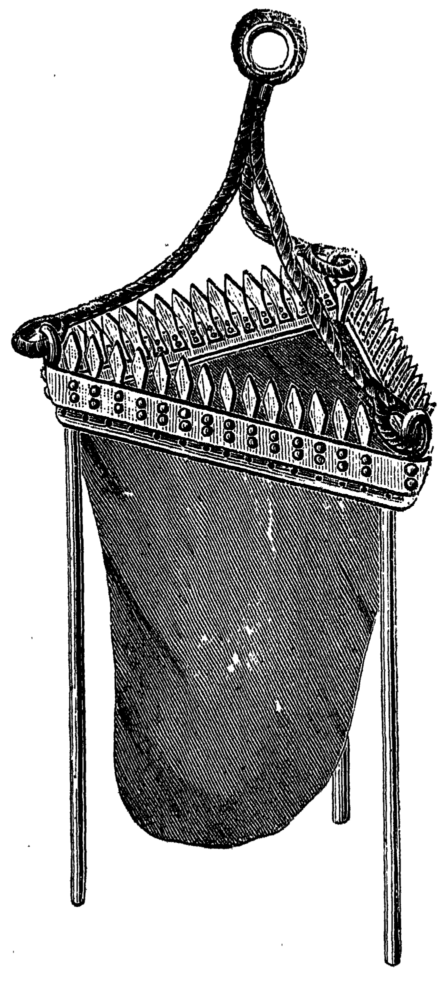
The growing-houses consist of a horse-shoe-shaped block of buildings, on one side of which is a long low house, and of a detached underground house. In designing the plan, the object specially kept in view was to furnish favourable conditions for the cultivation of all the important types of warmer climates; and the houses were therefore not built higher than seemed absolutely necessary. The chief part of the block consists of a higher and a lower cool-house, a higher and a lower hot-house, and a propagating-house. The higher houses are eight, the lower four metres in height, and the propagating-house still lower. Each of the lower houses is again divided into two, for different temperatures. The warmer division of the lower hot-house contains three basins for the culture of tropical fresh-water plants. The propagating-house is, in the same way, divided into two. The underground house is a long building entirely buried, the glass roof alone projecting above the surface of the ground. The heating is effected by hot-water pipes.

The various study-rooms are devoted partly to morphological and systematic, partly to physiological work. The former comprise a large herbarium in the top story, and four roomy work-rooms on the ground floor, in which are also kept those portions of the herbarium which are required for reference for the work in hand, and the whole of the dried Algæ. The first story is devoted to the residence of the Director. One of the work-rooms is devoted entirely to marine Algæ; each is fitted up with microscopical apparatus, and they are furnished with a very extensive reference-library. The second portion comprises a room with a small chamber opening out of it for chemico-physiological work; a room with stone floor, facing the north, for physico-physiological work; and a dark chamber with a balcony in the top story. Before the balcony a large sandstone slab is let into the wall of the building for the erection of a heliostat. In the basement story is a dynamo-machine.

For the collection of the seaweeds both row-boats and steamers are employed. For scraping the larger species off the rocks, Dr. Reinke has contrived a special drag-net, of which a drawing is appended, furnished with a row of sharp teeth at the mouth.

The culture of seaweeds presents greater difficulties in summer than in winter. They continue to grow in the Baltic at any temperature above zero C.; and, in cultivation, a low temperature is much more favourable to their growth than a high one. In the Institute they continue to fructify through the winter in the cool houses if protected from actual frost, the smaller species going through their complete cycle of development from the germinating spore; but a frequent change of the sea-water, or the addition of nutrient substances, is desirable. In summer the incidence of direct sunlight must be carefully avoided, and the temperature of the air must be kept as low as possible. For this purpose ice-cupboards have been built. Prof. Reinke has contrived a special arrangement for the cultivation of seaweeds in their native habitat. In the harbour near to the Botanic Garden, a wooden buoy is anchored, from which is suspended a wire basket by chains from 3 to 4 metres in length. In this floating aquarium the seaweeds grow exposed to their most favourable natural conditions of currents and variations of temperature in the water during the summer months. Next spring it is proposed to build an aquarium for seaweeds for public exhibition in connection with the Institute.

The Government of Prussia has rendered great assistance in the establishment of the Botanical Institute and



Marine Station at Kiel through its Minister for Education. The Director is very anxious that, especially in the department of marine Algæ, the herbarium and library, already so rich, should be rendered still more complete, by the addition of specimens or of treatises published in journals in which it may still be deficient.

SIR ROBERT KANE, LL.D., F.R.S.

SIR ROBERT KANE was born on September 24, 1810, in Dublin. This was the fiftieth year of King George III. and the tenth of the Union. Shortly afterwards his father established chemical works on the North Wall, by the side of the River Liffey, which in time developed into important and well-known sulphuric acid and alkali works. His mother was Ellen Troy, of whose family Dr. Troy, Roman Catholic Archbishop of Dublin, was a member. Sir Robert Kane very early in his life developed a taste for chemical knowledge, and in 1828 his first paper, "On the Existence of Chlorine in the Native Peroxide of Manganese," was published, and followed by a series of contributions on kindred themes. He entered Trinity College, Dublin, in 1829, and pro-

ceeded to his B.A. degree in the spring commencements of 1835, taking the LL.D. in the summer of 1868. In 1834 he was appointed Professor of Natural Philosophy to the Dublin (now the Royal Dublin) Society, and he at this period devoted himself with great ardour to original research in the field of chemistry, as the long list of his papers in the Royal Society's list will testify. He studied in Germany during his summer vacations under both Liebig and Mitscherlich, and passed some time under Dumas at Paris. In 1831 he was elected a member of the Royal Irish Academy; he was Secretary of its Council from 1842 to 1846, and was elected President in 1877. In 1849 he was made a Fellow of the Royal Society; shortly afterwards he was selected by the Government as head of the Museum of Irish Industry, which post he held until appointed the first President of the Queen's College, Cork. He was a Fellow of the King and Queen's College of Physicians, Ireland, a Commissioner of National Education, and a Justice of the Peace, Ireland.

After over twenty-two years of hard and earnest work in the development of the Cork College, he resigned the presidency in 1873, and took up his residence in Dublin, where he died on Sunday, the 16th instant.

Sir Robert Kane, in addition to the very numerous papers above referred to, was the author of a large and most important work on the industrial resources of Ireland, a theme which he handled in a painstaking and judicious manner. In his very early days he had acquired a practical knowledge of the value and importance of many of the neglected industries of Ireland, and from his chair in the lecture theatre of the Dublin Society, he often called attention to this subject, one which throughout his long life he never lost sight of. It is not without interest to note the fact that much is owing to the Royal Dublin Society for the ready help afforded to their two Professors, now both deceased, Sir Richard Griffith and Sir Robert Kane, in their efforts to advance the industries of Ireland.

In 1841, Sir R. Kane was awarded by the Royal Society a Royal Medal for his researches into the chemical history of archil and litmus; and in 1843, the Cunningham Gold Medal of the Royal Irish Academy, for his researches on the nature and constitution of the compounds of ammonia. These memoirs will be found published in the Transactions of the respective institutions.

In recognition of his scientific labours, and on his appointment to the presidency of Queen's College, Cork, he received knighthood in 1846 from Lord Heytesbury, the then Irish Viceroy. On the passing of Mr. Fawcett's Act in 1875, which altered the constitution of the University of Dublin, and appointed a Council, Sir Robert Kane was elected one of the first Roman Catholic members of that body, a post which he held until 1885, when the late Dr. Maguire was elected.

In this brief obituary notice, it is not necessary to attempt any analysis of the scientific work accomplished by Sir Robert Kane, but it is impossible to conclude it without a tribute of respect and affection to the many high and excellent qualities of the man, who in the various positions of Professor, head of a young educational establishment, or President of an Academy, won equally, from all with whom he came in contact, regard and esteem.

NOTES.

PROF. SCHUSTER has been elected Bakerian Lecturer for the present year. The lecture is to be delivered in the apartments of the Royal Society on March 20.

LAST week Mr. Justice Kay complained that judicial time is sadly wasted over patent cases, and he declared that the smaller and more petty the dispute the more time seemed to be expended. Now, as we have pointed out more than once, enormous waste of time is inevitable where the suitors in patent cases, especially in cases which involve scientific details, as most of them do at the present day, have to appear before a judge who is not himself a man of science. They have to begin by teaching his lordship the rudiments of that branch of science of which the disputed patent is a practical application. That our judges are painstaking, rapid, and acute pupils may readily be granted, but still time has to be consumed in the task, and there is something pathetic in the spectacle of an able and conscientious lawyer wrestling with the problems presented by the highest applications of, say, electricity or chemistry to industry, while scientific witnesses are contradicting each other all round him. We fear that judicial time will continue to be wasted so long as judges without a knowledge of science are left unaided to decide questions which demand long scientific training. There can be no change for the better until judges have sitting on the bench with them scientific assessors as they have now nava assessors, or until scientific cases are passed on as a matter of course to qualified referees as cases involving accounts are. It requires at least as much special training, and is as far outside the experience of ordinary lawyers, to settle a scientific case, as to decide whether a ship has been properly navigated, or whether a set of accounts tell in favour of a plaintiff or a defendant.

ON Tuesday evening there was some discussion in the House of Commons as to the supplemental vote of £100,000 for the purchase of a site at South Kensington for a suitable building for the housing of the science collections. Mr. Jackson explained that the extent of the land was four and a half acres, and the sum at which it was valued included a building for which the Government now paid a rent of £1500 a year, which would, of course, fall out of the Estimates when the Government became the proprietors of the land in question. No commission was to be paid to any person on either side in respect of this transaction, which was a direct one between the Commissioners of the 1851 Exhibition and the Government. Sir H. Roscoe thought it desirable that the money should be voted at once. The plot of land was the only one ever likely to be available for the purpose. Mr. Mundella said that as he had been pressing upon Governments for the last ten years the necessity for them to acquire this land, he thought that he ought to say something in defence of what the Government had done in asking for the sum on the present occasion. He did not approve of supplementary estimates, and he thought that no one would be more glad to get rid of them than the Government themselves. This question, however, had been pressing for the last ten years, because for the whole of that period the most valuable national science collections, such as no other country in the world possessed, had been housed in the most disgraceful manner. The Treasury had all along resisted the demands made upon them to sanction the expenditure necessary for the erection of a Museum to hold these collections, notwithstanding that three departmental committees had reported in favour of that expenditure. The only question, therefore, was whether the Government were getting good value for their money in making this purchase. He knew something of the value of the land, which had been fixed by eminent surveyors at £200,000, while the Government were going to get it for £70,000. The money which the Commissioners would receive in respect of the sale would be appropriated to providing scholarships for the promotion of technical education to the amount of £5000 per annum, which were to be open to all schools of every denomination in the United Kingdom. He therefore urged the Committee to agree to this proposal at once. Sir L. Playfair explained that the Commissioners of the Exhibition of 1851 had formed their estimate of

the value of the land upon the value of the surrounding property. The Commissioners had been pressed year after year to apply their surplus revenues to educational purposes. They had pressed the Government to come to some conclusion on the subject, as it had been going on for from three to ten years. They could not go on waiting continually, and the Government at last came to the conclusion—and, he thought, came to a wise conclusion—to accept the offer. He thought the Committee would see that they had been very patient. Mr. W. H. Smith, replying to the objection that the vote ought to have been included in the ordinary estimates, pointed out that if the vote were not taken at once, probably it could not be reached before June or July, or even August. It was unreasonable to ask the Commissioners to wait until that time. He had resisted the expenditure at South Kensington as long as he could, and until he was satisfied that in the interests of the country it was necessary. He strongly resisted the expenditure before, but when the Committee they had appointed reported that further accommodation was required, they had no alternative but to carry out their recommendations. The proposal of the Government was accepted by a majority of 77—the number of those in favour of the reduction of the vote being 67, while 144 voted on the other side.

WE regret to notice the death, on February 2, of M. Ch. Fievez, the assistant in charge of the spectroscopic department of the Royal Observatory of Brussels, at the comparatively early age of 45. M. Fievez did not enter the Observatory until 1877, having been originally intended for the military profession. M. Houzeau, then the Director of the Observatory, being desirous of creating a spectroscopic department, sent Fievez, to whom he proposed to commit its management, to study under Janssen at Meudon, with whom he remained six months. Fievez's most important work was the construction of a chart of the solar spectrum on a scale considerably greater than that of Ångström; but besides this he was not able to effect much in astronomical spectroscopy, owing to the unfavourable position of the Observatory for such observations. He therefore turned his attention principally to laboratory work, and in this department made a detailed study of the spectrum of carbon, besides numerous experiments on the behaviour of spectral lines under the influences of magnetism and of changes of temperature. M. Fievez was Correspondant of the Royal Academy of Belgium, and Foreign Member of the Society of Italian Spectroscopists.

STUDENTS of palæontology heard with much regret of the recent death of Prof. von Quenstedt, of Tübingen. He was the most famous of German palæontologists, and did much important work in mineralogy also. He had an especially profound knowledge of the Lias of Würtemberg and its fossils. His work on "Der Jura" is well known, and so recently as 1885 a new edition, greatly modified, of his "Handbuch der Petrefactenkunde" was issued. Dr. von Quenstedt died at an advanced age on December 21, 1889.

A WRITER who is contributing to *Industries* a series of articles on the "Recent Growth of Technical Societies," infers, from a comparison of the balance-sheet for 1878 with that for 1888, that the Proceedings of the Royal Society are "evidently less sought after than they were." An average of four years would have pointed to an opposite conclusion. For the years 1876–79 the average sale was £743 1*s*. 7*d*., while that of 1886–89 was £810 3*s*. 3*d*. The writer leaves out of account, moreover, that in 1878 the Royal Society, according to their published list, presented their Transactions and Proceedings to 276 institutions, while at present they give them to no fewer than 363 institutions.

MUCH interest has been excited by the announcement of the discovery of coal in Kent. The search for coal at a point near the South-Eastern Railway, adjoining the experimental heading for the Channel Tunnel, has been carried on for several years. The following report, by Mr. Francis Brady, C.E., the engineer-in-chief of the South-Eastern and Channel Tunnel Companies, was published in the daily papers on February 20:—"Coal was reached on Saturday last, the 15th inst., at 1180 feet below the surface. It came up mixed with clay, and reduced almost to powder by the boring tools. A small quantity of clean bright coal found in the clay was tested by burning, and proved to be of good bituminous character. The seam was struck after passing through 20 feet of clays, grits, and blackish shales belonging to the coal-measures, which at this point lie close under the Lias, there being only a few intervening beds of sand, limestone, and black clay separating them. The correspondence of the deposits with those found in the Somersetshire coal-field is thus pretty close, the difference consisting in the absence of the red marl at the Shakespeare boring. The lines of bedding in the shale are distinctly horizontal. This is an indication that the coal-measures will probably be found at a reasonable depth along the South-Eastern Railway to the westward. I beg to hand you herewith two specimens of the clay containing coal, one taken at 1180 feet, and the other at 1182 feet. I also inclose a specimen of clean coal taken to-day at 1183 feet 6 inches from the surface." With regard to this report, Prof. Boyd Dawkins writes to us:—"As the enterprise resulting in the discovery of coal near Dover was begun in 1886, and is now being carried on under my advice, I write, after an examination of the specimens from the boring, to confirm the published report of Mr. Brady, so far as relates to the coal. The coal-measures with good blazing coal have been struck at a depth of 1160 feet, well within the practical mining limit, and the question is definitely answered which has vexed geologists for more than thirty years. Further explorations, however, now under consideration, will be necessary before the thickness of the coal and the number of the seams can be ascertained. This discovery, I may add, with all the important consequences which it may involve, is mainly due to the indomitable energy of Sir Edward W. Watkin."

THE second meeting of the Australasian Association for the Advancement of Science seems to have been in every way most successful. It was held at Melbourne, and began on January 7. At the Sydney meeting last year there were 850 members. This year the number rose to 1060. Baron von Müller, F.R.S., was the President. Great efforts were made to secure that members from a distance should enjoy their visit to Melbourne, and the serious work of the various Sections was varied by pleasant excursions. An excellent "Hand-book of Melbourne," edited by Prof. Baldwin Spencer, was issued.

THIS year the University of Helsingfors will celebrate its two hundred and fiftieth anniversary. It was founded at Abo, but transferred to Helsingfors in 1820.

AT a recent meeting of the French Meteorological Society, M. Wada, of the Tokio Observatory, gave a *résumé* of the seismological observations made in Japan during 1887. The number of earthquake shocks amounted during the year to 483. The hourly and monthly distribution of the shocks at Tokio during the last 12 years shows a slight excess in favour of the night-time, above the day; and also an excess in winter and spring, over the other seasons. The area affected during the year 1887 represented five times the superficies of the empire. M. Wada gave details of the shocks, their direction, intensity, and distribution.

TIDINGS of another great volcanic eruption have come from Japan. Mount Zoo, near the town of Fukuvama, in the Bingo district, began to rumble at 8 o'clock on the evening of Jauuary 16, and the top of the mountain is said to have been soon "lifted off." There was a din like a dynamite explosion, and

sand and stones were belched forth. Stones and earth also fell at Midsunomimura, a village six miles away. No previous eruption of Mount Zoo is recorded. Only one man lost his life, but some cattle were killed, and 55 houses were destroyed. The total loss entailed by the eruption is estimated at nearly $3,500,000.

Two rather strong shocks of earthquake were felt at Rome on Sunday last, February 23, shortly after 11 p.m. They were more distinct in the environs than in the city itself, and especially at the Rocca di Papa in the Campagna. The Rome correspondent of the *Daily News* says it was remarked that flocks of sheep "showed great signs of fear some time before the shocks were felt." The correspondent of the *Standard* notes that in several public buildings the gas was almost extinguished, that electrical apparatus was disturbed, and that electric bells were set ringing. "My own experience," he adds, "was that of feeling lifted up from my seat, and then set down again with a slight, but sickening, jar, while doors rattled, and furniture was moved so as to produce noise in knocking against walls."

ACCORDING to a telegram sent through Reuter's agency from Lisbon, a slight shock of earthquake was felt on February 24 at Leiria and places between it and the sea coast.

THE Pilot Chart of the North Atlantic Ocean for February states that the month of January was remarkable for the tempestuous weather that prevailed almost uninterruptedly over the steamship routes. Storms succeeded each other in rapid succession, the majority of them having developed inland and moved east-north-east on very similar paths from Nova Scotia and across southern Newfoundland. The most notable storm of the month was probably one that developed in the St. Lawrence valley, and crossed the Straits of Belle Isle early on the 3rd. It then moved nearly due east, rapidly increasing in intensity until reaching the 20th meridian, when it curved to the north-eastward, and was central on the 5th about lat. 55° N., long. 17° W., and disappeared north of Scotland. The barometric pressure in this storm was remarkably low, 27·93 inches having been recorded at 4 p.m. on January 4, about lat. 53° N., long. 23° W. There was a slight increase in the amount of fog experienced; it was confined for the most part to the regions west of the Grand Banks. Much ice has been reported since the 5th; the positions and dates plotted on the chart indicate that the ice season is one of the earliest on record—nearly a month earlier than usual. This is due in a great measure to the prevalence of severe northerly gales east of Labrador, coincident with the heavy westerly gales of December and January along the Transatlantic route.

THE Japanese Government, we observe, is about to establish a meteorological observatory in the Loochoo Islands. This is one of the most important positions in the East for meteorological purposes, for it fills up the very large gap at present existing between Shanghai and Manilla in one direction, and Hong Kong and Tokio in the other. Besides, the Loochoo Archipelago is a specially valuable position for observing the phenomena connected with the course of the typhoons of the China seas.

THE meeting of the International Congress of Hygiene and Demography, which is to be held in London in 1891, will probably be thoroughly successful. An organizing committee, with Sir Douglas Galton as President, has been formed, and already delegates have been appointed by the leading scientific societies. On Tuesday, February 18, a deputation waited upon the Lord Mayor to discuss the arrangements that ought to be made for the meeting. The Lord Mayor, having heard what Sir Douglas Galton, Prof. Corfield, and other members of the deputation had to say as to the importance of the Congress, undertook that the matter should be brought forward at a public meeting in the Mansion House. This meeting will take place on Thursday, April 24, and the Lord Mayor will preside.

THE ninth annual meeting of the members of the Sanitary Assurance Association was held on Monday, February 17, Sir Joseph Fayrer, F.R.S., in the chair. Mr. Joseph Hadley, Secretary, read the annual report, which concluded as follows:—"Though the important bearing of the work of the Association on the public health is not yet fully appreciated by the general public, the financial statement for the past year proves that the Association is making progress, and that after nine years' experience its work continues to be appreciated. The income for the year was £398 8*s*. 10*d*., and after meeting all liabilities a balance is carried forward." The Chairman, in proposing the adoption of the report, said that the more he saw of the work of the Association, and the need for sanitary improvement, the more was he interested in its progress, and he expressed a hope that not only might this Association prosper, but that others might be formed, so great was the work to be done. General Burne and Dr. Danford Thomas were re-elected members of the executive council, and Sir Joseph Fayrer and Prof. T. Roger Smith were re-elected President and Vice-President respectively.

SOME time ago we referred to the fact that the Manchester Field Naturalists' and Archæologists' Society had appointed a committee for the purpose of promoting the planting of trees and shrubs in Manchester and its immediate suburbs. The idea has commended itself to the Corporation, and it is expected that evergreen shrubs, planted in boxes or tubs, will soon be placed in some of the principal squares. Meanwhile, the committee are trying to obtain the aid of experienced practical men. They have issued a circular with the following list of questions:—"What description of trees would you especially recommend for open spaces?" "What kind of shrubs, especially such as would succeed in tubs or boxes?" "What suggestions can you offer as to soil, treatment, and upon any important point relating to tree culture in towns?" When the best information that can be obtained has been brought together, it will be embodied in a pamphlet, which may, it is hoped, serve as a general guide for tree planting and culture.

AT the meeting of the Royal Botanic Society on Saturday, the Secretary called attention to several plants of hygrometric club moss from Mexico, which had been presented, with other specimens, by Mr. A. Gudgeon. The Secretary stated that these plants had the power, ascribed to the well-known rose of Jericho, of rolling themselves up like a ball when dry, and becoming apparently dead; but that they were able to unfold and grow again when exposed to moisture. The specimens shown had been kept for three months in a dry place, but now were green, and to all appearance flourishing.

THE following lectures will be given at the Royal Victoria Hall during March:—March 4, Mr. F. W. Rudler, on "Geology in the Streets of London"; 11th, Dr. Dallinger, on "The Infinitely Great and the Infinitely Small"; 18th, Prof. Beare, on "Australia"; 25th, Mr. W. North, on "Rome."

"OUR Earth and its Story" (Cassell and Co.) consists of three volumes, not two, as inadvertently stated in our notice of the work on February 13 (p. 341).

A SERIES of new compounds of hydroxylamine, NH_2OH, with several metallic chlorides, are described by M. Crismer in the current number of the *Bulletin de la Société Chimique*. The first member of the series obtained was the zinc compound $ZnCl_2.2NH_2OH$, whose existence was unexpectedly discovered during the course of experiments upon the action of metallic zinc on aqueous hydroxylamine hydrochloride. A ten per cent. solution of this latter salt was treated with an excess of pure zinc; no evolution of gas was noticed in the cold, but on warming

over a water-bath a slow disengagement of bubbles was found to occur. After allowing the reaction to complete itself during the course of a few days, the liquid, which had become turbid, was filtered, allowed to cool, and again filtered from a little more flocculent material which separated out, and finally concentrated and allowed to crystallize. A large quantity of hemispherical crystal aggregates then separated, which were found on analysis to consist of the new salt, $ZnCl_2.2NH_2OH$. Several other methods of obtaining it were investigated; it may be obtained by treating an aqueous solution of hydroxylamine hydrochloride, $NH_2OH.HCl$, with zinc oxide or carbonate, or with a mixture of zinc sulphate and barium carbonate, or by treating an alcoholic solution of hydroxylamine with zinc chloride. But the best method, and one which gives 97 per cent. yield, consists in dissolving ten parts of hydroxylamine hydrochloride in 300 c.c. of alcohol in a flask provided with an inverted condenser; the liquid is then heated to the boiling-point and five parts of zinc oxide added, the boiling being continued for several minutes afterwards. The clear liquid is then decanted and allowed to cool. After the deposition of the first crop of crystals, the mother liquor may be returned to the flask and treated with a further quantity of zinc oxide, four repetitions of this treatment being sufficient to obtain an almost theoretical yield of the salt. The white crystals are then washed with alcohol and dried in the air. They resist the action of most solvents, water only slightly dissolving them, and that with decomposition. Organic solvents are practically without action upon them. When heated in a narrow tube, as in attempting to determine the melting-point, the salt violently explodes. If a quantity is heated to about 120° C., in a flask connected with a couple of U-tubes, the second containing a little water, gas is abundantly liberated, and drops of hydroxylamine condense in the first U-tube together with a little nitrous acid. The water in the second tube is found to contain hydroxylamine, ammonia, and nitrous acid, while fused zinc chloride remains behind in the flask. A similar cadmium salt was also obtained, $CdCl_2.2NH_2OH$, in brilliant crystals which separated much more quickly than those of the zinc salt. This cadmium compound is much more stable under the action of heat, gas being only liberated in the neighbourhood of 190°-200°, and only a little hydroxylamine distils over. The barium salt, $BaCl_2.2NH_2OH$, is a specially beautiful substance, crystallizing from water in large tabular prisms, which are very much more soluble in water than either of the salts above described.

THE additions to the Zoological Society's Gardens during the past week include an Esquimaux Dog (*Canis familiaris* ♀), bred in England, presented by Mr. W. Tournay; two Barbary Turtle-Doves (*Turtur risorius*) from North Africa, presented by Miss Teil; a Bonnet Monkey (*Macacus sinicus* ♀), a Macaque Monkey (*Macacus cynomolgus* ♂) from India, a Common Raccoon (*Procyon lotor*) from North America, deposited; a Green Monkey (*Cercopithecus callitrichus*) born in the Gardens.

OUR ASTRONOMICAL COLUMN.

OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at Greenwich at 10 p.m. on February 27 = 8h. 30m. 43s.

Name.	Mag.	Colour.	R A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G.C. 1711	—	—	8 45 37	+51 44
(2) 58 Hydræ, U.A. ...	7	Yellowish-red.	8 39 53	−10 45
(3) ζ Hydræ	3	Yellowish-white.	8 49 36	+ 6 22
(4) ε Hydræ	3	Yellowish-white.	8 41 0	+ 6 49
(5) 115 Schj.	6	Yellowish-red.	8 49 11	+17 39
(6) W Tauri	Var.	Reddish-yellow.	4 21 45	+15 51

Remarks.

(1) "Very bright; very large; at first very gradually, then very suddenly much brighter in the middle." The spectrum of this nebula has not yet, so far as I know, been recorded.

(2) Dunér classes this with stars of Group II., but states that the spectrum is very feebly developed, and expresses a doubt as to the type. As I have before remarked, Mr. Lockyer's discussion of the stars of this group seems to indicate that the spectra which are described as "feebly developed" really represent stages in the passage from one group to another. If, for example, we consider a rather faint star with the banded spectrum a little more developed than in the case of Aldebaran, its spectrum would no doubt be described as "feebly developed," if classed with Group II. In such a case the star would be more condensed than those in which the spectrum is said to be well developed, and the flutings would have almost entirely given way to lines. Line absorptions would therefore indicate that the star belonged to a late stage of the group. On the other hand, if the star be at a very early stage of condensation, the flutings would still only be feebly developed, and might be accompanied by bright lines. In any case, further examination is necessary, as the star may belong to an early stage of Group VI., and not to Group II. at all.

(3) A star classed by Vogel with stars of the solar type. The usual differential observations are required.

(4) A star of Group IV. (Vogel). The usual observations are required.

(5) A "superb" example of stars of Group VI. (Dunér). The principal bands are very wide and dark, and the secondary bands 4 and 5 are also well seen. Bands 7 and 8 are doubtful.

(6) This variable will reach a maximum about March 7. The period is about 360 days, and the magnitudes at maximum and minimum are 8·2 ± and < 13 respectively. The star is not included in Dunér's catalogue, but Vogel states that the spectrum is of the Group II. type. Observations before and after maximum, with special references to changes of spectrum, should be made.

NOTE ON THE ZODIACAL LIGHT.—In favourable localities the zodiacal light should now be visible in the evening, and as further spectroscopic observations are desirable, it may be convenient to briefly summarize here the results already obtained. Ångström first observed the spectrum at Upsala, in March 1887, and noted the presence of the chief line of the aurora spectrum, at a wave-length stated as 5567. Respighi, in 1872, also observed this line, in addition to a faint continuous spectrum, and believed this to demonstrate the identity of the aurora and zodiacal light. He found, however, that at the same time the bright line was visible in almost every part of the sky, and this led to the suggestion that it originated from a concealed aurora. Prof. Piazzi Smyth, in Italy, observed nothing but a faint continuous spectrum, extending from about midway between D and E to F. A. W. Wright's observations led him to the following conclusions:—"(1) The spectrum of the zodiacal light is continuous, and is sensibly the same as that of faint sunlight or twilight. (2) No bright line or band can be recognized as belonging to this spectrum. (3) There is no evidence of any connection between the zodiacal light and the Polar aurora" (Capron's "Auroræ," p. 69). Mr. Lockyer believes the zodiacal light to be due to meteoritic dust, which is to a certain extent self-luminous, as indicated by the bright line in the spectrum, and argues in favour of a connection between auroræ and the zodiacal light (Proc. Roy. Soc., vol. 45, p. 247). He says:—"The observations of Wright and others, showing that the spectrum is continuous, are not at variance with Ångström's observation, for we should expect the spectrum to be somewhat variable. It is probable that the observations showing nothing but continuous spectrum were made when the temperature was only sufficient to render the meteoritic particles red hot. That the zodiacal light does consist of solid particles, or, at all events, of particles capable of reflecting light, is shown by the polariscope." He also quotes from a letter in which Mr. Sherman, of Yale College, states that he has reason to believe that the appearance of the bright line in the zodiacal light has a regular period.

On January 20 I saw the zodiacal light very well at Westgate-on-Sea, but was unable to detect anything beyond a faint continuous spectrum.

Mr. Maxwell Hall's observations at Jamaica (see NATURE, February 13, p. 351) also record continuous spectra, but with remarkable changes in the region of maximum intensity. He suggests comparative observations with the spectrum of twilight.

In connection with the suggestion of the variability of the spectrum, it is important to secure further observations. If the existence of the bright line at some periods be established, we may then safely conclude that the luminosity of the zodiacal light is not entirely due to reflected sunlight.

A. FOWLER.

OBSERVATIONS OF ζ URSÆ MAJORIS AND β AURIGÆ.—The periodic duplicity of the K line in the spectra of these stars before noted (January 23, p. 285) led Prof. Pickering to conclude that the time of revolution of the former system was 104 days. In the current number of the *Sidereal Messenger*, however, Prof. Pickering adds a note, dated January 11, 1890, in which he records that later observations make it probable that the period of ζ Ursæ Majoris is 52 days instead of 104, and that its orbit is noticeably elliptical. The velocity of the components of β Aurigæ seems to be 150 miles per second, their period 4 days, their orbit nearly circular, with a radius of 8,000,000 miles, and their masses 0·1 or 0·2, that of the sun being unity.

COMET BROOKS (*d* 1889).—The following ephemeris is given by Dr. Knopf in *Edinburgh Circular* No. 5, issued on the 22nd inst. :—

1890. March.	R.A. h. m. s.	Decl. ° ′	1890. March.	R.A. h. m. s.	Decl. ° ′
1 ...	2 22 54 ...	+17 58·6	15 ...	2 49 26 ...	+20 8·0
3 ...	26 40 ...	18 17·9	17 ...	53 17 ...	20 25·3
5 ...	30 26 ...	18 36·9	19 ...	57 8 ...	20 42·3
7 ...	34 13 ...	18 55·6	21 ...	3 0 59 ...	20 59·1
9 ...	38 1 ...	19 14·1	23 ...	4 51 ...	21 15·6
11 ...	41 49 ...	19 32·3	25 ...	8 43 ...	21 31·8
13 ...	45 37 ...	19 50·3			

The brightness on March 1 = 0·24, and on March 25 = 0·17, that at discovery being unity.

NEW SHORT-PERIOD VARIABLE IN OPHIUCHUS.—Mr. Edwin F. Sawyer announces the discovery that the star 175 (*Uranometria Argentina*) Ophiuchi, R.A. 17h. 45m. 57s., Decl. − 6° 6′·7 (1875·0), is a variable of short period (*Astronomical Journal*, No. 210). The range of variation appears to be from 6·2m. to 6·95m., and the period slightly greater than 17 days.

OBSERVATIONS OF THE MAGNITUDE OF IAPETUS.—In the January number of *Monthly Notices* is found an interesting communication to the Royal Astronomical Society by Mr. Barnard, of the Lick Observatory, on the eclipse of this outermost satellite in the shadows of the globe, crape ring, and bright ring of Saturn. By frequent comparison of the light of Iapetus with that of Tethys and Enceladus, the effect of the shadow of the crape ring on the visibility of the satellite was tested, seventy-five comparisons being made. It was found that, after passing through the sunlight shining between the ball and the rings, Iapetus entered the shadow of the crape ring. As it passed deeper into this, there was a regular decrease in light until it disappeared in the shadow of the inner bright ring. From the observations it appears that the crape ring is truly transparent, the sunlight sifting through it. The particles composing it cut off an appreciable quantity of sunlight, and cluster more thickly, or the crape ring is denser, as it approaches the bright rings.

GEOGRAPHICAL NOTES.

AT the ordinary meeting of the Royal Geographical Society, on Monday, Mr. C. M. Woodford read a paper on "Further Explorations of the Solomon Islands." He has visited these islands three times, and in the present paper he described what he saw during his third visit, in 1888. He took up his residence in the small island of Gavotu, off the coast of Gola, or Florida Island, a place centrally situated for visiting Ysabel, Guadalcanar, and other islands. He stayed with a trader named Lars Nielson, who had since been killed and eaten by the natives, as had also three of his boys. Since last June no fewer than six white men had been murdered by the natives of the Solomon Group, out of a total white population estimated at about thirty. Mr. Woodford's principal object in his last journey was to identify the places visited by the Spanish Expedition under Mendaña that discovered these islands in the year 1568. In this, he thought he might say, he had been entirely successful. The Spaniards related that when they were between Florida and Guadalcanar they passed an island in the centre of which was a burning volcano. This island was now conclusively identified with the Island of Savo. The lecture was illustrated with photographs of natives of Guadalcanar and other places, as well as specimens of rude architecture, by means of the dissolving-view apparatus.

ACCORDING to the Copenhagen correspondent of the *Frankfurter Zeitung*, an Expedition for the exploration of Greenland will start next summer from Denmark. The plan of work has been arranged by the Naval Lieutenant Ryder. The party will consist of nine persons. They will have three boats, and a steamer will convey them to the eastern coast as soon as the condition of the ice will allow of a landing. It is proposed that the region lying between 66° and 73° north latitude shall be explored in the course of the summer, and that the party shall push as far as possible into the interior. Sledges will be employed during the winter. The Expedition will be provisioned and equipped for two years, at the end of which time the steamer will return to take them away, cruising along the east coast till they get down to the shore. The expenses have been estimated at from 250,000 to 290,000 kroner (equal to from about £13,900 to £16,100), and the project is so popular, and looked on so favourably by the Government, that it is practically certain that the Diet will grant the money.

THE Geographical Society of Vienna issues a circular letter, dated February 1890, announcing the election of officers made last December. The new President is Herr Hofrath Ritter von Hauer, Intendant des naturhistorischen Hofmuseums.

LOCUSTS IN INDIA.

IN 1889, parts of Sind, Guzerat, Rajputana, and the Punjab were much troubled by locusts. A report on these destructive creatures is being prepared under the direction of the trustees of the Indian Museum, Calcutta; and, in the hope that information about them, with specimens, may be obtained from persons who have had opportunities of observing them, Mr. E. C. Cotes, of the Indian Museum, has issued a preliminary note, summing up some of the principal facts that have already been brought together. This note is very interesting, and has been compiled chiefly from the records of the Revenue and Agricultural Department of the Indian Government.

The generally received idea is that the locust which invades India belongs to the species usually spoken of as *Acridium peregrinum*, and supposed to have been the locust of the Bible. The identity of Indian locusts has not yet, however, been definitely ascertained, and this is one of the points which require elucidation. As far as we at present know, there seems reason to believe that while *Acridium peregrinum* extends its ravages into the dry plains of the Punjab and Rajputana, the locust which proved injurious in Madras in 1878, and in the Deccan in 1882–83, belongs to a very different species, which is probably *Acridium succinctum*. In order to settle the question it will be necessary to examine further specimens taken from destructive flights which have appeared in various localities, the material in the Indian Museum being at present insufficient.

Dealing with the natural history of locusts generally, Mr. Cotes observes that all the different species which occur in various parts of the world breed permanently in barren elevated tracts where the vegetation is sparse. In years when they increase inordinately they descend in flights from their permanent breeding-grounds upon cultivated districts, where they destroy the crops, lay their eggs, and maintain themselves through one complete generation, but are unable to establish themselves permanently, usually disappearing in the year following the invasion, to be succeeded, after an interval of years, by fresh swarms from the permanent breeding-ground.

Generally speaking, the life circle of a locust extends through one year, in which period it passes through its various stages of egg, young wingless larva, active pupa, and winged locust, which dies after laying the eggs that are to produce the next generation. The eggs are laid in little agglutinated masses in holes, which the female bores with her ovipositor in the ground. In temperate climates the eggs are usually deposited in the autumn, but in sub-tropical countries, such as India, where there is but little winter, the winged locusts live on through the cold season, and only die off after depositing their eggs in the following spring. In this case the eggs hatch after lying in the ground for about a month. In both temperate and sub-tropical regions

alike, the young wingless locusts, on emerging from the eggs in the spring or summer, feed voraciously and grow rapidly for two or three months, during which period they moult at intervals, finally developing wings and becoming adult. The adult insects fly about in swarms, which settle from time to time and devour the crops. The damage done by locusts is thus occasioned in the first instance by the young wingless insects, and afterwards by the winged individuals into which the young are transformed after a couple of months of steady feeding.

In Rajputana and the Punjab in 1869 the flights were said to have come chiefly from the vast tract of sand hills (*Teeburs*) between the Runn of Kutch and Bhawulpore, and partly from the Suliman Range in Afghanistan. Locusts were reported as usually to be found in the autumn in the *Teeburs*, and it is thought that this tract is probably a permanent breeding-ground. The whole question, however, of the permanent breeding-grounds of these locusts is one that requires further investigation. The winged flights appeared throughout Central Rajputana in the latter part of the hot weather, and laid eggs which hatched as the rains set in; the old locusts dying after they had deposited their eggs. From these eggs were hatched young locusts which became full grown and acquired wings in August and September. The eggs laid by the original flights at the end of the hot weather were distributed throughout the whole of Central Rajputana, and a vast amount of injury was done, the crops being damaged, in the first instance, by the young locusts before they acquired wings, and afterwards by the winged swarms which flew about the country and settled at intervals to eat what had escaped the ravages of the young wingless locusts.

In the Punjab, flights of locusts, from the Suliman Range, Afghanistan, appeared in the western border, in the end of April and in May. Eggs and young locusts were also found about this time near the hills in the sandy tracts of the same district. The flights seem generally to have moved from west to east, and by July to have spread themselves throughout the Punjab; but the laying of eggs and the hatching out of young went on, at least in the south-east, throughout August and September.

In Bombay, locusts were noticed in May and June 1882, in the south-west of the Presidency; but they attracted little attention, such swarms being annual visitors of the Kanarese forests, and neither in Kanara nor in Dharwar did they cause any material injury. With the setting in of the south-west monsoon, however, they spread in flights over the Presidency to the north and north-east, and early in the rains proceeded to lay their eggs and die. These eggs hatched in the end of July and beginning of August, and the young locusts did a large amount of damage, over a wide area, through the months of August and September. In the early part of October, with the setting in of the north-east monsoon, the young locusts, which had by this time acquired wings, took flight, and travelled with the prevailing wind in a south-westerly direction, doing some injury in the Poona Collectorate as they passed. They then struck the Western Ghâts, and spread slowly over the Konkan in November, and thence travelled into the Native States of Sawantvadi and the Kanara district. During the remainder of the cold season and the following hot weather (December 1882, to the end of May 1883), the flights clung to the Ghâts, occasionally venturing inland into Belgaum, Dharwar, the Kolhapur State, and Satara, and devouring the spring crops in the Coast Districts, but ordinarily keeping in the vicinity of the hill ranges. With the commencement of the south-west monsoon, in the latter part of May 1883, the flights began to move in a north-easterly direction, as they had done the preceding year, but in larger numbers.

At the commencement of the rains they began to alight in vast numbers over an immense tract of country, comprising six Deccan Collectorates and three Coast Collectorates. They deposited their eggs and died; and early in August the young locusts hatched out in countless numbers, but were apparently more backward, and possessed of less strength and stamina than were those of the previous year. The unusually heavy rainfall killed vast numbers of them in some parts of the country, and elsewhere the insects seemed stunted and feeble, and grew but slowly. They were destroyed in vast numbers by the vigorous measures initiated by Government officers, and were also said to be diseased and attacked by worms and other parasites. As late as November, the mass of the young locusts appeared still unable to fly, and made no general move, as they had done the year before, towards their permanent home in the south-west. The invasion was in fact at an end, and though swarms appeared in Sawantwadi in 1883–84, no further injury of a serious nature seems to have occurred.

The injury occasioned to the rain crops by the locusts was very considerable, over a great portion of the Deccan and Konkan, both in 1882 and 1883. But it was found, at the end of the invasion, that abundance of the cold weather crops had compensated to so great an extent for the injury done to the rain crops, that, on the whole, no very widespread suffering had arisen.

In 1878, when the Madras Presidency was invaded, the young locusts began to appear in January, and were found in great numbers in different districts from then on till September and October, the earlier swarms being found in the west and south of the Presidency, and the later ones in the north and east. Winged locusts were first observed, in the end of March and beginning of April, in the hills to the south-west (Wynaad and Nilgiri), where they may be supposed to breed permanently. Thence, aided by the south-west monsoon, they gradually worked their way over the Presidency to the east and north, finally disappearing about November and December.

The information hitherto obtained hardly justifies any very decided conclusion as to the life history of the locust. But it may be noticed that locusts were observed pairing in the Salem District, in the latter part of June, and also that the young locusts, which were found, in the early part of May, in the Udamalpet *Taluk*, were supposed to be the offspring of the large flights of winged locusts which had appeared in the preceding February in the same *taluk*. The connection between the autumn broods of locusts and those which appeared in the early part of the year has not been made out satisfactorily.

Mr. Cotes ends his paper with an account of the chief measures which have at different times been adopted in India against locusts, pointing out that, the locust of North-West India being distinct from that of South-West India, measures found useful in one invasion are not necessarily applicable in another.

FIELD EXPERIMENTS ON WHEAT IN ITALY.[1]

PROF. GIGLIOLI, of the Agricultural College at Portici, a graduate of the Royal Agricultural College, Cirencester, has given to the Association of Proprietors and Farmers of Naples a voluminous and most carefully compiled Report on the results of the first year's experiments on wheat-growing at the experimental field of Suessola, about six kilometres fom Acerra. The field is on the estate of Count Francesco Spinelli, who generously lends it to the Association for experimental purposes. The district was celebrated in olden time for its fertility, but was afterwards long neglected on account of its marshy nature, and the land became sour and productive of disease. Now, again, drainage and improved cultivation have changed these marshes into some of the best land of a fertile district. The soil of the experimental field is easily worked, friable, and bears a good natural vegetation; no analysis of it, however, is furnished. Giglioli points out that it is in too high condition at present for comparative manuring experiments, but admirably suited for comparing different varieties of corn and different methods of sowing and cultivation, as by dibbling and the Lois-Weedon system.

There are in all 102 plots devoted to trying the effects of different manures, each plot being about 43 square metres; 18 unmanured plots of a similar size devoted to different varieties of wheat; and 3 plots, each about twice the above-mentioned size, used for different methods of seeding and cultivation. Paths were made round each plot, the paths being at rather a lower level than the plots themselves The author discusses the question of large and small plots, but concluded that under the conditions obtaining, small plots were the best for use here.

On the 102 manured plots, Scholey squarehead wheat was sown, with a great variety of manures—organic, nitrogenous, phosphatic, and potassic; but it was afterwards found this variety of wheat was, unfortunately, not well suited to the climate and to the general purpose of these experiments.

The 18 varieties experimented with, on the second series, included several well-known English varieties, such as Hallett's pedigree white and red wheats, Chiddam, golden drop, Hunter's

[1] "Resultati del Primo Anno di Esperimento sulle Varietà e sui Concimi del Frumento al Campo Sperimentale di Suessola nell' Anno Agrario 1887–88." By Italo Giglioli. Pp. 508. (Naples, 1889.)

white and Victoria white, also some Hungarian wheats, besides Italian varieties.

It was found that the English varieties gave very poor results; the squarehead was a very poor sample indeed, and it was unfortunate that it was used for the manuring experiments. The degeneration of English wheats during the first year is probably due to the great amount of transpiration taking place in this climate, especially during such a hot and dry summer as that of 1888. Giglioli enters into an interesting discussion of this important physiological result.

The most productive wheat was a variety known as Noé, from the South of France, originally from Bessarabia—this yielded at the rate of 3485 kilograms per hectare; next in order were two Italian varieties, Rieti and Puglia grain, yielding at the rate of about 3150 kilograms per hectare. The Puglia wheat was the finest in quality of grain, but its yield of straw was very low.

The great importance of a careful selection of varieties is pointed out, and Giglioli is of opinion that much more good will be done by improving and selecting Italian varieties than by importing new varieties; which, if from colder countries, will probably not be able to stand the climate.

Incidentally, the experiments showed the great benefit of good cultivation and of surface draining, the plots being above the level of the surrounding paths, for the produce of the unmanured plots was double that of the neighbouring land under ordinary cultivation.

From the manuring experiments it was shown that farmyard manure gave fair results, but the season was unfavourable to the action of artificial manures, being much too dry. Of nitrogenous manures, acidified urine gave the best results, but nitrate of soda and sulphate of ammonia were often worse than useless. Phosphates had some good effect, and Thomas-Gilchrist slag was useful. Potash salts had no particular effect; the chloride seemed rather better than the sulphate.

The results of the manuring experiments, considering the great care and labour bestowed on them, must be disappointing; but the soil is in too high condition for manures to show great effects, also the variety of grain sown was unsuitable to the climate, and the season was against manures, especially nitrogenous manures.

In this Report the details of the experiments are given in full, with the appearance of the plots at different dates, and the whole results tabulated in various ways in nearly a hundred tables. All the weighings at harvest were carried out under the personal superintendence of Prof. Giglioli, who evidently has spared neither time, trouble, nor health, in conducting these important researches. Already the results have yielded important information, especially on the suitability or the reverse of special varieties of wheat to the climate of Southern Italy, and with their continuance there can be no doubt that results most valuable to the Italian farmer on the cultivation and manuring of wheat will be obtained.

Whilst heartily congratulating Prof. Giglioli and the Agricultural Association of Naples on having inaugurated these experiments with the prospect of continuing them for some years, we cannot but think that their value would be greatly increased if the plots were larger; or, if this cannot be arranged with the appliances at command, if the experiments were always in duplicate, or preferably in triplicate, and this might be rendered possible by reducing the number of experiments on manures in future seasons.

E. K.

SCIENTIFIC SERIALS.

American Journal of Science, February.—The magnetic field in the Jefferson Physical Laboratory, by R. W. Willson. One of the wings of this Laboratory in Harvard University has been constructed wholly without iron for the purpose of research, and the author has made a series of experiments to determine how far the end sought has been gained. He has found the magnitude of the disturbance which may arise in practice from such objects as stoves and iron pipes, and has made the interesting discovery that the brick piers of the building have a sufficient amount of free magnetism to produce quite an appreciable effect.—On Cretaceous plants from Martha's Vineyard, by David White. The author has studied a number of fossil plants collected at several localities and horizons in the Vineyard series for the purpose of solving the question as to the age of the underlying clays, lignites, and sands, of Martha's Vineyard. He concludes that the evidence from the fossil plants bespeaks an age decidedly Cretaceous, and probably Middle Cretaceous, for the terrane in which they were deposited.—Review of Dr. R. W. Ell's second report on the geology of a portion of the Province of Quebec, with additional notes on the "Quebec group," by Charles D. Walcott. The geological systems recognized in the area reported upon include the Devonian, Silurian, Cambro-Silurian (Ordovician), Cambrian, and pre-Cambrian.—Measurement by light-waves, by Albert A. Michelson. The telescope and microscope are compared with the refractometer, some remarkable analogies in their fundamental properties are pointed out, and a few cases in which the last-named instrument appears to possess a very important advantage over the others illustrated. Previous experiments have shown that the utmost attainable limit of accuracy of a setting of the cross-hair of a microscope on a fine ruled line was about two-millionths of an inch, whereas direct measurements of the length of a wave of green light emitted by incandescent mercury vapour, show that the average error in a setting was only about one ten-millionth of an inch. The method is also extended to angular and spectrometer measurements.—On lansfordite, nesquehonite, a new mineral, and pseudomorphs of nesquehonite after lansfordite, by F. A. Genth and S. L. Penfield. The authors have examined the crystallization of lansfordite ($3MgCO_3.Mg(OH)_2 21H_2O$), and another new mineral having the composition $MgCO_3.3H_2O$, which has been named nesquehonite. A crystallized artificial salt of the same composition is also described.—Weber's law of thermal radiation, by William Ferrel. An examination of Weber's new law, and a test of his formula by means of experimental results, in which the absolute rate of losing heat is determined from the observed rate of cooling of heated bodies of known thermal capacity, and the relative rate from the galvanometer needle of the thermopile.—Tracks of organic origin in rocks of the Animikie Group, by A. R. C. Selwyn. Traces of fossils, or what are supposed to be such, have been discovered in the Animikie rocks of Lake Superior. The fact is interesting and important, for, if the black Animikie shales represent the Lower Cambrian of the Atlantic border, the Paradoxides and Olenellus fauna will probably be found in them sooner or later.

In the numbers of the *Journal of Botany* for January and February, two important monographs are commenced—by Mr. E. G. Baker, a synopsis of genera and species of Malveæ; and by Mr. G. Massee, a monograph of the genus *Podaxis*. This last genus of Fungi, Mr. Massee proposes to transfer, in consequence of the mode of formation of the spores, from the Gastromycetes, where it has hitherto been placed, to the Ascomycetes.

The *Botanical Gazette* for October 1889 contains an interesting summary of our present knowledge of protoplasm, by Prof. Goodale, in the form of an address to the Botanical Section of the meeting of the American Association for the Advancement of Science held at Toronto.

With the exception of an interesting paper by Prof. Massalongo, descriptive of some curious instances of teratology in the floral and foliar organs, the number of the *Nuovo Giornale Botanico Italiano* for January is chiefly occupied by a report of the proceedings of the Italian Botanical Society. Among a number of short papers, is one on the fertilization of *Dracunculus vulgaris*, the most important insect agent in which is stated by Prof. Arcangeli to be *Saprinus subnitidus*; one on the fertilization of *Arum pictum*, by Prof. Martelli; and one on the development of the picnids of Fungi, by Prof. Baccarini.

SOCIETIES AND ACADEMIES.

London.

Linnean Society, February 6.—Mr. Carruthers, F.R.S., President, in the chair.—Referring to an exhibition at a previous meeting, Prof. Stewart communicated some interesting observations on the habits of certain seaweed-covered crabs. He also made some remarks on the "pitchers" of *Nepenthes Mastersiana*, upon which criticism was offered by Mr. Thomas Christy, Prof. Howes, and Mr. J. Murray.—Prof. G. E. Boulger exhibited a series of original water-colour drawings of animals and plants of the Falkland Islands.—Mr. W. H. Beeby exhibited some forms new to Britain of plants from Shetland.—Mr. C. B. Clarke,

F.R.S., then read a paper on the stamens and setæ of *Scirpeæ*, illustrated by diagrams, which elicited a detailed criticism from Mr. J. G. Baker, to which Mr. Clarke replied.—A paper was then read by Mr. B. D. Jackson, which had been communicated by the late Mr. John Ball on the flora of Patagonia, prefaced by some feeling remarks by the President, on the loss which the Society had sustained through the recent death of this able botanist.

Zoological Society, February 18.—Dr. St. George Mivart, F.R.S., Vice-President, in the chair.—Mr. Tegetmeier exhibited and made remarks on two Cats' skulls, out of the large quantity of remains of these animals recently brought to this country from Egypt.—Mr. G. A. Boulenger read a report on the additions made to the Lizard collection in the British Museum since the publication of the last volume of the British Museum Catalogue of this group. A list was given of 91 species new or previously unrepresented in the collection. Ten species and three genera were described as new.—Mr. P. L. Sclater, F.R.S., read some notes on a Guinea-fowl from the Zambesi, allied to *Numida cristata*, and gave a general account of the recognized species of this group of Gallinaceous birds.—Dr. Mivart, F.R.S., read some notes on the genus *Cyon*, mainly based on an examination of the specimens of this genus of Canidæ contained in the British Museum.—Mr. P. L. Sclater, F.R.S., read a paper containing the characters of some new species of the family Formicariidæ.—Dr. Augustine Henry read some notes on the Mountain Antelopes of Central China (*Nemorhedus argyrochætes* and *N. henryanus*).

Royal Meteorological Society, February 19.—The following papers were read:—Observations on the motion of dust, as illustrative of the circulation of the atmosphere, and of the development of certain cloud forms, by the Hon. Ralph Abercromby. The author has made numerous observations on the motion of dust in various parts of the world, especially on deserts on the west coast of South America. He finds that the wind sometimes blows dust into streaks or lines, which are analogous to fibrous or hairy cirrus clouds; sometimes into transverse ridges and furrows, like solid waves, which are analogous to certain kinds of fleecy cirro-cumulus cloud; sometimes into crescent-shaped heaps with their convex side to the wind, which are perhaps analogous to a rare cloud form called "mackerel scales"; sometimes into whirlwinds, of at least two if not of three varieties, all of which present some analogies to atmospheric cyclones; sometimes into simple rising clouds, without any rotation, which are analogous to simple cumulus-topped squalls; and sometimes into forms intermediate between the whirlwind and simple rising cloud, some of which reproduce in a remarkable manner the combination of rounded, flat, and hairy clouds that are built up over certain types of squalls and showers. Excessive heating of the soil alone does not generate whirlwinds; they require a certain amount of wind from other causes to be moving at the time. The general conclusion is, that when the air is in more or less rapid motion from cyclonic or other causes, small eddies of various kinds form themselves, and that they develop the different sorts of gusts, showers, squalls, and whirlwinds.—Cloud nomenclature, by Captain D. Wilson-Barker. The author proposes a simple division of cloud-forms under two heads, viz. cumulus and stratus, and recommends that a more elaborate and complete division should be made of these two types. A number of photographs of clouds were exhibited on the screen in support of this proposal.—An optical feature of the lightning flash, by E. S. Bruce. It has been stated in the Report of the Thunderstorm Committee of the Royal Meteorological Society, that there is not the slightest evidence in the photographs of lightning flashes of the angular zigzag or forked forms commonly seen in pictures. The author, however, believes that this is an optical reality, as the clouds on which the projection of the flash is cast are often of the cumulus type, which afford an angular surface. In support of this theory he exhibited some lantern slides of lightning playing over clouds.

Anthropological Institute, February 11.—Dr. Garson, Vice-President, in the chair.—Mr. T. W. Shore read a paper on characteristic survivals of the Celts in Hampshire. He considered the round huts of the charcoal-burners a survival of the huts which were common in the Celtic period; and some of the industries of the Celtic period appear to have survived in Hampshire to the present day, such as that of osier-working or basket-making. There can be little doubt that Hayling, anciently spelt Halinge, has derived its name from the Celtic word *hal*=salt; the salt works which still exist there are in all probability an example of a survival of a Celtic industry. Several instances were given of earthworks which must be ascribed to the Celts, and it was suggested that the mounds upon which many ancient churches in Hampshire are built may have been sacred sites of the same people. Reference was made to the peculiar orientation of many Hampshire churches, 20° north of east, and it was explained as a survival of a reverence for the May Day sunrise from Celtic pagan time to Saxon Christian time, and under a modification to a later date.—Dr. Garson exhibited and described some skulls dredged from the bed of the Thames by Mr. G. F. Lawrence, who afterwards gave an account of the strata in which they were found.

Mathematical Society, February 13.—J. J. Walker, F.R.S., President, in the chair.—Mr. S. Roberts, F.R.S., read a paper concerning semi-invariants.—Mr. Tucker (Hon. Sec.) communicated papers by Prof. K. Pearson, on ether-squirts; by Prof. G. B. Mathews, on class-invariants; and a note on the imaginary roots of an equation, by Prof. Cayley, F.R.S.

PARIS.

Academy of Sciences, February 17.—M. Hermite in the chair.—Observations of minor planets made with the great meridian circle and Jardin's meridian circle at the Paris Observatory during the first three months of 1889, by Admiral Mouchez. Comparisons with published ephemerides have been made in the following cases: Victoria (12), Astræa (5), Parthenope (11), Hebe (6), and Eugenia (45).—On the movements of planets, supposing their attraction represented by one of the electro-dynamic laws of Gauss or Weber, by M. F. Tisserand. The author has investigated the motions of Mercury and Venus on the hypothesis that they were not governed by Newton's law of gravitation, but by one of the above named. The change of the longitude of perihelion for a given time would be about twice as great, using Gauss's law, than by using Weber's. Taking the velocity of light as 300,000 kilometres per second, it is found that, on the hypothesis of Weber's law, the major axis of Mercury's orbit would have a direct motion of $14''\cdot 4$ in a century; for Venus the variation would be only $3''\cdot 0$. Using Gauss's law, the value for Mercury becomes $28''\cdot 2$.—Posthumous article on polyhedrons by Descartes; a note by M. de Jonquiemes, in which he shows that Descartes not only knew and employed the relation $S + F = A + 2$, but that he announced it explicitly, and prior to Euler.—On a new reviving plant, by M. Ed. Bureau. Two specimens of a supposed new plant which revived when placed in water, similar to the Rose of Jericho, have been investigated. The change, however, is not simply hydration, as in the latter plant. The specimens, which were found in Arkansas, prove to be the *Polypodium incanum*, Pluck, but the above property does not appear to have been previously observed in it.—On the distribution of pressures and velocities in the interior of liquid sheets issuing from weirs without lateral contraction, by M. Bazin.—On some objections to the theory of deep vertical circulation in the ocean, by M. J. Thoulet. It is concluded that the circulation of water between the equator and the Poles only affects a depth of about a thousand metres. Below this the water is in a state of repose. The conclusion has been arrived at from a consideration of deep-sea sediment and the observations of the density of water at great depths given in the *Challenger* Report.—On the St. Petersburg problem, by M. Seydler. Two solutions are given of this "probability" problem.—On the regular surfaces of which the linear element is reducible to the form of Liouville, by M. Demartres.—On the surfaces of which the linear element is reducible to the form $ds^2 = F(U + V)(du^2 + dv^2)$, by M. A. Petot.—Summary of the observations of the total solar eclipse of December 22, 1889, by M. A. de la Baume Pluvinel.—Note on the calculation of the compressibility of air up to 3000 atmospheres, by M. Ch. Antoine. In the expression $pv = D(\beta + t)$ (the pressure, p, being given in atmospheres, and the volume, v, in litres), for air

$$\beta = 273\cdot 6 - \sqrt{p}.$$

If up to 40 atmospheres $D = 2\cdot 835$,
and beyond 40 atmospheres $D = 2\cdot 835 + 0\cdot 0018 (p - 40)$,

the table given for air at $t = 0°$ is found to agree well with the experimental results of Regnault and Amagat.—Extension of the theorems relative to the conservation of the flux of force and of magnetic induction, by M. Paul Janet.—Upon batteries with

molten electrolytes, and upon the E.M.F. at the surface of contact of a metal and a melted salt, by M. Lucien Poincaré. The author finds the E.M.F.'s in this case to be nearly the same as those found by M. Bouty (*Comptes rendus*, t. xc. p. 217) in the case of saturated solutions.—Electrolysis by igneous fusion of the oxide and fluoride of aluminium, by M. Adolphe Minet. The author presents the result of three years' work on the electrolysis of the fused oxide and fluoride of aluminium, in a table which gives the quantity of metal obtained as a function of the time and of the quantity of electricity used.—Note by MM. P. Hautefeuille and A. Perrey, on the silico-glucinates of soda. In a preceding note, the authors have described a number of silico-glucinates of potash, obtained by heating together mixtures of silica, glucina, and the alkali, with neutral vanadate of potash. They now have applied the same method of mineralization with mixtures containing soda, heating to about 800°. Five forms, of different composition, have been thus obtained. Substituting tungstate for vanadate of soda, two species of crystals have been obtained, corresponding in composition with two of those obtained with vanadate as mineralizing agent.—Upon the *rôle* of foreign bodies in iron and steel; the relation between their atomic volumes and the allotropic transformations of iron, by M. F. Osmond. Prof. W. C. Roberts-Austen, studying the effect of minute percentages of foreign elements upon the mechanical properties of gold, found a relation between the results obtained and the position in the periodic table of the introduced elements, and has predicted a similar phenomenon in the case of iron. Reviewing his former work in the light of this new idea, the author has found the prediction to be verified. Shortly, it may be said that foreign bodies of small atomic volume tend to cause iron to assume or remain in that of its molecular forms in which it has itself the smaller atomic volume, bodies of great atomic volume produce the opposite effect.—M. J. Ville, on dioxyphosphinic and oxyphosphinous acids. In two preceding notes (*Comptes rendus*, t. cvii. p. 659, t. cix. p. 71), and in the present communication, it is shown that by the reaction of aldehydes upon hypophosphorous acid, two new classes of acids have been obtained, with the general formulæ:—

$$(1)\ \mathrm{PO}\begin{cases}\mathrm{(R\,.\,CH\,.\,CH)}\\ \mathrm{(R\,.\,CH\,.\,OH)}\\ \mathrm{OH}\end{cases};\qquad (2)\ \mathrm{PO}\begin{cases}\mathrm{H}\\ \mathrm{(R{-}CH\,.\,OH)}.\\ \mathrm{OH}\end{cases}$$

—Dibromo-carballylic acid, by M. E. Guinochet. This acid has been obtained by the reactions of 4 equivalents of bromine upon one equivalent of aconitic acid in a sealed tube, heated for thirty-six hours to 115°-120°.—Estimation of uric acid in urine by means of a hot solution of hypobromite of soda, by M. Bayrac. The principle of the method consists in separating the uric acid from the urea and creatinin present by alcohol, and the titration of the isolated acid with sodic hypobromite at 90°-100°. Results are said to be as exaet as those obtained by the best known methods, while the process takes much less time.—Researches upon the pathogenic microbes in the filtered waters of the Rhone, by MM. Lortet and Despeignes.—Upon the nutrition of the fungus of the *muguet*, by MM. Georges Linossier and Gabriel Roux. A complete study of the mineral, carbohydrate, and nitrogenous foods required and the substances produced by this fungus is given.—The perception of luminous radiations by the skin, as exemplified by the blind Proteus of the grottos of Carniola, by M. Raphael Dubois. By a number of experiments upon *Proteus anguinis*, the author demonstrates that the sensibility of its skin to light is about half of the sensibility of its rudimentary eyes, and further that this sensibility varies with the colour of the light employed, being greatest for yellow light.—The wax-organs and the secretion of wax in the bee, by M. G. Carlet. The author's researches lead him to conclude: (1) the wax is produced by the 4 last ventral arches of the abdomen; (2) it is secreted by an epithelial membrane and not by the cuticular layer of these arches, nor by the intra-abdominal glands; (3) this secretory membrane lies between the cuticular layer and the lining membrane of the antero-lateral part of the ventral arch; (4) the wax traverses the cuticular layer and accumulates on its outer surface.—Experimental plant cultivation in high altitudes, note by M. Gaston Bonnier. The modifications produced in Alpine plants by the climate have been studied and some general conclusions drawn, among which the most interesting is: "For the same extent of leaf surface, the assimilation is much more considerable in Alpine plants than in those of lower stations, on account of the greater thickness of the palisade tissue and the abundance of chlorophyll."

Berlin.

Physiological Society, January 31.—Prof. du Bois-Reymond, President, in the chair.—Dr. Grabower spoke on root-area of the motor nerves of the laryngeal muscles.—Prof. Munk made a further communication on the subject of the cortical visual areas. His earlier researches on the extirpation of these areas had shown that the retina may be regarded as spatially projected on to the visual area in such a way that its external portion corresponds to the external part of the visual area of the *same* side, while the inner portion corresponds to the inner part of the area of the *opposite* side, and the middle portion to the middle part of the visual area of the opposite side. The upper half of the retina corresponds to the anterior part of the visual area, and the lower half to the posterior. More recently, Prof. Schäfer, of London, has found that, when the visual areas are stimulated electrically, movements result which are confined entirely to the eyes; when the anterior part of the area is stimulated, the eye is turned downwards and towards the opposite side; and when the posterior part is stimulated, the movement is similarly towards the opposite side, but now upwards. When, however, the central part of the area is stimulated, the result is merely a movement towards the opposite side. It was shown by the speaker, as the result of a large number of experiments on dogs which he had performed in conjunction with Dr. Obregici, that these movements are not dependent on the stimulation of any motor centres or upon any ordinary reflex movements, but that they are really movements which accompany visual sensations. They were shown by careful analysis to result in the directing of the eye towards that point in space into which the visual perception is referred whenever any definite point of the retina is stimulated by light, the point stimulated in this case being the corresponding part of the electrically stimulated visual area. Thus when the anterior part of the area is stimulated, the lower portion of the retina is stimulated, the resulting visual image is consequently referred out upwards, and the eyes accordingly also move upwards and towards the opposite side. Similarly for stimulations of other parts of the visual area. These experimental stimulations hence afford an evidence of the detailed spatial projection of the retina on to the visual areas, which is as certain and even more convincing than the evidence obtained from localized extirpations of the areas. They further permitted of a more certain delimitation of the visual areas than had been possible in the earlier experiments. It is impossible to enter here into the many interesting details of these experiments, or to give any account of the lengthy discussion which followed Prof. Munk's communication.

Physical Society, February 7.—Prof. Kundt, President, in the chair.—Dr. Budde spoke on the very rapid rotation of a solid body, possessed of three unequal moments of inertia, about a fixed point. He developed very fully the equations which hold good for this motion, and dealt, at the end of his communication, with the physical experiments which might be performed in order to test the equations.—Dr. Feussner spoke on the methods which are employed at the Government Physico-technical Institute for the measurement of electrical resistances. He exhibited and explained the several instruments used, pointing out that in their arrangement the greatest importance must be attached to the very accurate measurements of temperature. For this purpose the wires are wound upon metallic cylinders in order to provide for the rapid cooling of the wires as they are warmed by the passage of the current: these are then submerged in petroleum, whose temperature is recorded by a thermometer immersed in the liquid, which is itself kept constantly stirred. German-silver wires have shown themselves to be unsuited for the purposes of constructing the standard resistances, since their resistance increases regularly with lapse of time; neither could this increase be done away with by heating the wires until they were quite soft. This tendency was attributed to the occurrence of a gradual crystallization, which depended chiefly upon the zinc in the alloy. On this account an alloy of copper and nickel was employed, which is known commercially as "patent nickel," and examined as to its suitability. Wires made of this alloy possess a very low temperature-coefficient, and were found to be almost absolutely constant after being heated to 100° C. If they are kept for some time after they are made and wound, and are then heated, they may be used as standards for comparison. Several other alloys were also tried, as, for instance, various combinations of copper and manganese. The speaker described the experimental measurements made with these wires, and stated that up to 30 per cent. of manganese, above which amount

it was not possible to draw a wire in this alloy, they have yielded a negative coefficient of temperature. When the alloy contained only a small percentage of manganese, the coefficient was very small, so that such wires would be suitable for the construction of standard coils. In conclusion, he described how the resistances are measured in the Government Institute. The method employed is that of compensation, and measurement of potentials.—Dr. Jäger announced that Dr. de Coudres, in Leipzig, had succeeded in detecting a thermo-electric tension between compressed and uncompressed mercury. The compression was produced either hydraulically or by means of its own weight acting through a column of mercury. It was found possible to determine with certainty the direction of the thermo-electric current, and to measure its intensity for given pressures and temperatures. The investigation is not yet completed, but Dr. de Coudres hopes to be soon in a position to give a full account of his experiments.

IN the report of the meeting of the Berlin Physical Society, January 27 (p. 383), *for* Dr. Lehmann *read* Dr. Leman.

STOCKHOLM.

Royal Academy of Sciences, February 12.—Contributions to the flora of the Hieracia of South-Eastern Sweden, by Herr H. Dahlstedt.—On the remains of a bread-fruit tree from the Cenoman strata of Greenland, by Prof. A. G. Nathorst.—Report on researches in practical pomology and horticulture during a tour in France and Germany, by Herr C. V. Hartman.—On the lichens of the island of Bornholm, by Dr. P. J. Hellbom.—Algæ aquæ dulcis exsiccatæ quas distribuerunt, V. Wittrock et O. Nordstedt, Parts 18-21, exhibited and demonstrated by Prof. Wittrock.—The results of a determination of the rotation of the sun, executed during the years 1887-89 in the Observatory of Lund, by Prof. Dunér.—On the influence of the duration of exposure for a photographic image of a star, by Dr. Charlier.—Experimental determination of the principal elements of a divergent lens, by Dr. C. Mebius.—Derivatives of sulphur urates, by Dr. Hector.—On the $\beta_1 = \alpha_1$ bromium naphthalin sulphon acid, and on the constitution of the acids which are formed by the agency of concentrated sulphuric acid on β-naphthylamin, by S. Forsling.—Experiments on the humidity of the atmosphere, by Dr. K. H. Sohlberg.—Anatomical studies on the floral axes of diclinous Phanerogams, by Herr A. Grevillius.

DIARY OF SOCIETIES.

LONDON.

THURSDAY, FEBRUARY 27.

ROYAL SOCIETY, at 4.30.—The Croonian Lecture—The Relations between Host and Parasite in certain Epidemic Diseases of Plants: Prof. H. Marshall Ward, F.R.S.

SOCIETY OF ARTS, at 5.—The Northern Shan States and the Burma-China Railway: William Sherriff.

INSTITUTION OF ELECTRICAL ENGINEERS, at 8.—The Theory of Armature Reaction in Dynamos and Motors: James Swinburne.—Some Points in Dynamo and Motor Design: W. B. Esson.

ROYAL INSTITUTION, at 3.—The Three Stages of Shakspeare's Art: Rev. Canon Ainger.

FRIDAY, FEBRUARY 28.

AMATEUR SCIENTIFIC SOCIETY, at 8.—Practical Coal-mining: H. S. Streatfeild.

ROYAL INSTITUTION, at 9.—Evolution in Music: Prof. C. Hubert H. Parry.

SATURDAY, MARCH 1.

ESSEX FIELD CLUB, at 7.—Micro-Fungi of Epping Forest; how to Collect, Preserve, and Study Them: Dr. M. C. Cooke.

ROYAL INSTITUTION, at 3.—Electricity and Magnetism: Right Hon. Lord Rayleigh, F.R.S.

SUNDAY, MARCH 2.

SUNDAY LECTURE SOCIETY, at 4.—Apollonius of Tyana; the Story of his Life and Miracles: G. Wotherspoon.

MONDAY, MARCH 3.

SOCIETY OF ARTS, at 8.—Stereotyping: Thomas Bolas.

ARISTOTELIAN SOCIETY, at 8.—The Psychological Development of the Conceptions of Causality and Substance: G. F. Stout.

VICTORIA INSTITUTE, at 8.—Chinese Chronology: Rev. James Legge.

ROYAL INSTITUTION, at 5.—General Monthly Meeting.

TUESDAY, MARCH 4.

ZOOLOGICAL SOCIETY, at 8.30.—On the Classification of Birds: Henry Seebohm.—A Revision of the Genera of Scorpions of the Family Bathidæ, with Descriptions of some New Souh African Species: R. I. Pocock.—On some Galls from Colorado: T. D. A. Cockerell.—Report on the Insect-House for 1889: A. Thomson.

INSTITUTION OF CIVIL ENGINEERS, at 8.—The Hawksbury Bridge, New South Wales: C. O. Burge.—The Erection of the Dufferin Bridge over the Ganges at Benares: F. T. G. Walton.—The New Blackfriars Bridge on the London, Chatham, and Dover Railway: G. E. W. Cruttwell.

UNIVERSITY COLLEGE BIOLOGICAL SOCIETY, at 5.15.—A Peculiar Ferment in Balanoglossus: Dr. Halliburton.—The Weather Plant: Mr. Weiss.

ROYAL INSTITUTION, at 3.—The Post-Darwinian Period: Prof. G. J. Romanes, F.R.S.

WEDNESDAY, MARCH 5.

SOCIETY OF ARTS, at 8.—Recent Progress in British Watch and Clock Making: J. Tripplin.

ENTOMOLOGICAL SOCIETY, at 7.—New Longicornia from Africa: C. J. Gahan.—Notes on the Lepidoptera of the Region of the Straits of Gibraltar: J. J. Walker, R.N.—Some Water Beetles from Ceylon: Dr. D. Sharp.—The Classification of the Pyralidina of the European Fauna: E. Meyrick.—A New Species of Thymara and other Species allied to Himantopterus fuscinervis, Wesm.: Captain H. J. Elwes.—A Catalogue of the Pryralidæ of Sikkim collected by H. J. Elwes and the late Otto Möller: Pieter C. T. Snellen.

THURSDAY MARCH 6.

ROYAL SOCIETY, at 4.30.—The following papers will *probably* be read:—On a Second Case of the Occurrence of Silver in Volcanic Dust—namely, in that thrown out in the Eruption of Tunguragua, in the Andes of Ecuador, January 11, 1886: Prof. J. W. Mallet, F.R.S.—On the Tension of Recently-formed Liquid Surfaces: Lord Rayleigh.—(1) On the Development of the Ciliary or Motor Oculi Ganglion; (2) The Cranial Nerves of the Torpedo (Preliminary Note): Prof. J. C. Ewart.

LINNEAN SOCIETY, at 8.—On the Production of Seed in some Varieties of the Common Sugar-Cane (Saccharum officinarum): D. Morris.—An Investigation into the True Nature of Callus; Part 1, the Vegetable Marrow, and Ballia callitricha: Spencer Moore.

ROYAL INSTITUTION, at 3.—The Early Developments of the Forms of Instrumental Music: Frederick Niecks.

FRIDAY, MARCH 7.

PHYSICAL SOCIETY, at 5.—On Bertrand's Refractometer: Prof. S. P. Thompson.

GEOLOGISTS' ASSOCIATION, at 8.

INSTITUTION OF CIVIL ENGINEERS, at 7.—Telephonic Switching: C. H Wordingham.

ROYAL INSTITUTION, at 9.—Electrical Relations of the Brain and Spinal Cord: Francis Gotch.

SATURDAY, MARCH 8.

ROYAL BOTANIC SOCIETY, at 3.45.

ROYAL INSTITUTION, at 3.—Electricity and Magnetism: Right Hon Lord Rayleigh, F.R.S.

CONTENTS.

THURSDAY, MARCH 6, 1890.

THE SCIENCE COLLECTIONS AT SOUTH KENSINGTON.

IT is satisfactory to learn that the Government has taken the first step towards carrying out the recommendations of the recent Commission on the South Kensington Museum. The Report of the Commissioners was to the effect that the Science Museums contained valuable apparatus which ought to be exhibited; that the buildings in which it is displayed are inadequate; and that the area of the exhibition space ought immediately to be increased by 50 per cent. Between the Natural History Museum in Cromwell Road and the Imperial Institute Road lies the strip of ground on which the new buildings must be erected. It belonged to the Commissioners of the 1851 Exhibition, and they were willing to sell at a price somewhat less than the valuation of the Office of Works, or at ten shillings for every pound of their own estimate.

The question to be decided was, whether the country could afford £100,000 to purchase the land necessary to carry out the Report of one of the strongest Commissions which has ever investigated such a subject, or whether the great group of Museums for which South Kensington is famous was to be cut into two by rows of mansions.

The Government, which certainly did not err through undue haste, felt that a case had been made out, that further delay was useless, that the land ought to be secured before time and labour were spent in discussing the details of the buildings to be erected upon it, and therefore they brought in a supplementary estimate for the sum required.

Then followed a debate of the kind by which the *prestige* of ordinary members of the House of Commons has been reduced to its present level. One member "affirmed that there were empty rooms in South Kensington Museum which might well be used for the display of exhibits," though a body of Commissioners appointed to investigate the state of the collections had reported in a directly opposite sense. Another "could not understand why all these educational collections should be established close to one another at South Kensington." In other words, he could not see that if there is to be at South Kensington a great training school for teachers of science and art, it is desirable that the students should have ready access to the national science and art collections, and that the collections themselves should benefit from the advice of the Professors who are familiar with them. These objections were not, however, raised by men who knew the facts. Approval was expressed from both sides of the House by those who have the interests of education at heart. Sir Lyon Playfair, Sir Henry Roscoe, Mr. Mundella, and Mr. Chamberlain, all spoke in favour of the vote, and Mr. Mundella put clearly what those who are acquainted with the Museum know to be the truth, when he said "this question had been pressing for the last ten years, because for the whole of that period the most valuable national science collections, such as no other country in the world possessed, had been housed in the most disgraceful manner."

The vote was finally carried by 144 to 67, and it is to be hoped, now that the Government have entered upon the path of progress, they will pursue it with determination.

No one would urge precipitancy. Due care ought to be taken that money's value is obtained for money spent; but as the question of principle has been decided after ten years' debate, we have a right to demand that progress shall not be delayed by mere blind obstruction to every proposal which involves outlay, but that those in whose hands the fate of the science collections rests shall make up their minds as to what ought to be done, and shall forthwith do it.

THREE RECENT POPULAR WORKS UPON NATURAL HISTORY.

Glimpses of Animal Life. By W. Jones, F.S.A. (London: Elliot Stock, 1889.)

Toilers in the Sea. By M. C. Cooke, M.A., LL.D. (London: S.P.C.K., 1889.)

Les Industries des Animaux. Par F. Houssay. (Paris: J. B. Baillière et Fils, 1890.)

MR. JONES'S book is a charming little volume of 229 pages, with one illustration forming a frontispiece. There are, in all, seven chapters; dealing, in succession, with "Playfulness of Animals," "Animal Training," "Musical Fishes" (title ill chosen), "Nest-Building and Walking Fishes," "Luminous Animals," "Birds' Nests in Curious Places," and "The Mole." The author has been at immense pains to sift the voluminous literature of his subject (a task which he admits has involved a "somewhat unprofitable course of romance reading"). We find, as might be expected, citations of the old old stories of our youth; the climbing perch, Cowper's hares, and other time-honoured (if perhaps too highly coloured) narratives appear; the luminous centipede is not overlooked; and authorities are appealed to, from Aristotle and the ancient classical writers of the past, down to Lubbock and Romanes ("the Rev. Dr. Romanes" [*sic*], p. 25) of to-day. The work is essentially a compilation; it consists mainly of a collection of lengthy extracts, and the author has left himself little room for originality. There results from this an occasional heaviness of style, which is especially noteworthy in the earlier portions of the volume. Paragraphs too frequently lead off with "Broderip mentions," "Evelyn records," "Humboldt saw," and the like; and not even stories of the gambols between a rhinoceros and an elephant, or of those of a 60-foot whale, serve to relieve the monotony. It is doubtful whether the author has not occasionally erred in the placing of his anecdotes. To take a leading instance; on p. 32 there is recorded the story of a parrot, "which, when a person said to it, 'Laugh, Poll; laugh!' laughed accordingly, and the instant after screamed out, 'What a fool to make me laugh!'" This narrative cannot be said to betray any sense of playfulness on the part of the bird, as would be inferred from its position in the text; it surely should have found a place under "Animal Training." The most serious defect in the book is the absence of an index. The author has brought together a very remarkable series of anecdotes; and if he would give us an

exhaustive index, together with a complete bibliography, his book would befit the more special and advanced student of animal life. Without these it can only appeal to the *dilettanti*; and we shall look for them in a future edition. We would point out, at the same time, that the climbing perch is referred to on p. 151 as *Perca*, and on 157 as *Anabas* (the latter being correct); that "Willmoes" (p. 185) should read Willemoes Suhm; and that Mr. Romanes does not lay claim to the distinction accorded him on p. 25 (*cf. supra*). The author, as he enters into details not usually met with in books of this kind, might advantageously incorporate with his account of the stickleback's nest, the discovery of Möbius and Prince that the thread employed in weaving it is secreted by the animal's kidney. So unique a fact in natural history should not be allowed to pass unnoticed; and that portion of the work which deals with the luminous fishes might well be brought more completely up to date.

Dr. Cooke's treatise is one of 369 pages, with 4 lithographic plates, 70 woodcuts, and an index. It deals with marine invertebrata, in their especial relations to skeleton formation; and the volume is especially designed to make good the shortcomings of the Rev. J. G. Wood's work, entitled "Homes without Hands." The book has its good points; the chapter on "Coral Reefs and Islands," and the "Introduction," are fairly well done. The last-named deals with generalities as affecting life and the conditions of life in the ocean depths; it gives a record of important explorations, from that of Ross in Baffin's Bay, to the *Challenger*; the Bathybius controversy is abstracted, and alternative theories of reef-formation are summarized, both being presented in concise and impartial language. On perusal, however, of the main portion of the book, we meet with a preponderance of antiquated, and often erroneous information. Lengthy citations from the writings of authorities of the last two or three decades are flaunted as if expressive of current knowledge and opinion. The question of sponge affinities is discussed as though settled by Clark and Kent; that of the significance of the yellow bodies of the Radiolarians as though set at rest by the misconceptions of Wallich. We are told that there is no proof that the Millepore is a Hydroid, and so on. Upon the ill-effects which must result from this method of procedure it is needless to enlarge; but in justice to the author it must be admitted that he has made some use of recent literature. He appeals to the *Challenger* volumes. His quotations from these are, however, very capricious, and in some instances inaccurate. It cannot be said that the spines of the Radiolaria are "never tubular," for Haeckel (whose Report the author quotes) has given their tubular character as a diagnosis of his *Phæodaria*. Writing of "sensation in the Radiolaria," the author indulges (p. 103) in a remarkable paragraph, which concludes as follows:—

"Prof. Haeckel considers that the central capsule contains the common central vital principle, which he terms the 'cell-soul,' and that it may be regarded as a simple ganglion cell, comparable to the nervous centre of the higher animals, whilst the pseudopodia are analogous to a peripheral nervous system."

These are not the words of the author cited, and, even if they were, the introduction of such silly stuff into the pages of a book intended for "the large and increasing section of the nature-loving public who indulge in the use of the microscope as a source of instruction and amusement" (p. 3) is intolerable. It is a remarkable fact that, while the author has reproduced the more commonplace statements of the earlier writers in their original form, he should have chosen to give us the above, his own, rendering of the lucubrations of a Haeckel. In doing this he betrays a sad want of sound judgment. The public have a right to expect that a work of this type, intended to serve (p. 3) "as a preliminary to more specific knowledge, the direction of which they will thereafter be better able to choose," shall be up to date; but, to fulfil the useful purpose aimed at, such a work should rest upon a more authoritative foundation than the book now under review. That is amusing as an example of editorial piece-work among a somewhat antiquated literature, and to those familiar with the subjects approached it suggests reflections.

The volume by M. Houssay is one of 312 pages, with 47 woodcuts intercalated in the text (38 only are acknowledged on the title-page). The bulk of the work is divided into six chapters, dealing respectively with modes of capture of prey, of defence, of transport and storage of food, of provision for the young; of constructing or acquiring nests and habitations, and of preservation and protection of the same. The illustrations are, for the most part, admirable; some, which we take to be original, are fit to rank with the famous woodcuts in Brehm's "Thier-Leben," while others are already familiar to us from the pages of that work. In the introduction the author justly asserts that the naturalist of to-day lives more in the laboratory than in the field, that the scalpel and microtome have replaced the pins of the collector, and that the magnifier pales beside the microscope. This is, alas! too true. It cannot be denied that our present systems for the most part take insufficient heed of field-work, and we fully endorse the author's further remarks upon the changed aspect of affairs. The introduction as a whole deals with generalities in direct bearing upon those facts which follow; and by no means its least satisfactory feature is that it clearly sets forth what the author would have his readers understand by the title of his work. The main portion of the book is confined to bare records of observed fact, systematically arranged, and, where necessary, brought into special relationship by cross-references. That "talkee-talkee" so often forced into books of this kind is here withheld. Such comments as are indulged in are either confined to the introduction, or to a few concise paragraphs which make up the author's "conclusion"; and the latter is, as might be expected, devoted to a brief consideration of animal intelligence. In place of an index there is furnished a zoological table, in which the generic names of the animals written about are arranged in classificatory order, each being accompanied by a paged reference and a mention of that particular habit or industry dwelt upon. It is a pity that the author takes no cognizance of animals lower in the scale than the Arthropods; but we nevertheless heartily recommend his book to our readers. It is throughout popular, and written in that peculiarly pleasing, yet didactic, style, so characteristic of the works of the more successful of

French popularizers of science, which has made them masters of their art.

The above-named volumes are three of a number of similar treatises which have lately appeared. The appreciation of the beautiful and generally interesting in Nature must always precede the study of the more useful and special, and it is the highest function of works like the present to awaken this preparatory appreciation. Of such works those are the most valuable whose authors can claim a sound elementary knowledge of the facts with which they deal, and a familiarity with current research. Only on these terms can a popular natural history rise above the level of the too well-known type, in which the scissors supply the knowledge and the paste usurps the place of the co-ordinating intellect. G. B. H.

A GENERAL FORMULA FOR THE FLOW OF WATER.

A General Formula for the Uniform Flow of Water in Rivers and other Channels. By E. Ganguillet and W. R. Kutter. Translated from the German by Rudolph Hering and John C. Trautwine, Jun. (London: Macmillan and Co., 1889.)

THE general formula devised by Messrs. Ganguillet and Kutter for caculating the flow of water in both large and small channels, under varied conditions, was brought under the notice of English-speaking engineers by the publication, in 1876, of a translation by Mr. Jackson of some articles on the subject written by Mr. Kutter, which appeared in the *Journal der Cultur-Ingenieur* in 1870. This translation, however, was not authorized by Mr. Kutter, and contained some incomplete tables inserted by Mr. Kutter in his articles at the request of a friend. The present volume is a translation of the second edition of the treatise on the formula, written by Messrs. Ganguillet and Kutter, engineers in Berne, who have added a preface to the translation. Mr. Kutter died whilst this translation was in progress; and a short memoir of him, with a list of his works, is appended to the translators' preface.

The book commences with an historical sketch of the attempts to arrive at a formula for the flow of water in open channels; and the insufficiency of the earlier formulæ is pointed out. The investigations of Messrs. Darcy and Bazin, and the gaugings of the Mississippi by Messrs. Humphreys and Abbot, are then concisely described, and the formulæ which they deduced from the results of their experiments are given, the history of the subject, in a brief form, being thus brought down to the period at which Messrs. Ganguillet and Kutter commenced their investigations. This forms a sort of introduction to the account of the conception and development of the general formula, of which the various steps are described in detail. The modifications for various amounts of roughness are classified; and, finally, the formula is tested by the comparison of its results with a number of gaugings under very different conditions; and these results indicate, in considerably the greater number of cases, a closer approximation to the actual measurements than those obtained with the formulæ of either Humphreys and Abbot, or Bazin. A supplement gives a more direct derivation of the formula for mathematical readers; and the appendices contain numerous tables giving the flow of water in pipes under pressure, as well as in open channels, for practical use in English measures, derived from the formula, and also a diagram for the graphical determination of the values of the factors in the formula, adapted to English measures by the translators.

Most of the hydraulicians who had investigated the question before Darcy and Bazin, such as De Prony, Dubnat, Eytelwein, D'Aubuisson, Downing, and others, agreed in adopting a formula of the form $V = c\sqrt{RS}$, of which Brahms and Chezy are said to have been the authors in the latter half of the last century, in which V is the velocity, R the hydraulic radius, and S the slope. Different values were assigned to the factor c by the various investigators; but it was always regarded as a constant, applicable to any sized stream in most cases, to any slope, and to any state of the bed. Mr. Darcy was the first who directed attention to the influence the condition of the sides of channels and pipes exercised on the discharge; and he instituted a series of experiments, carried out after his death by Mr. Bazin, by which the flow of water in regular uniform channels, under different conditions of slope, form, and roughness of bed, was measured by careful gaugings and gauge-tubes. A few years previously, Messrs. Humphreys and Abbot had carried out their well-known gaugings of the flow of the Mississippi by means of double floats, and deduced a formula for the results obtained. Messrs. Ganguillet and Kutter found that the formula derived from the Mississippi experiments, relating to a large river with a very slight slope, was not applicable to the small streams with steep slopes of which they measured the flow in Switzerland, and also that Mr. Bazin's formula was not suitable, in its original form, for large rivers with irregular beds. This led Messrs. Ganguillet and Kutter to search for a formula applicable to very different slopes and sizes of channel, and adaptable to various conditions of bed. They took as the basis of their formula the various experimental results obtained in France and America, together with their own independent observations on channels with steep slopes, so as to include the extreme varieties of flow within the range of a single formula. Starting from Mr. Bazin's formula, $V = \sqrt{\dfrac{RS}{a + \dfrac{\beta}{R}}}$, where $c = \sqrt{\dfrac{1}{a + \dfrac{\beta}{R}}}$, they eventually found it expedient to express the value of c in the form $\dfrac{y}{1 + \dfrac{x}{\sqrt{R}}}$, in which, though they at first assumed y and x to be constant for any given state of bed, they finally modified them to expressions varying with the slope. The alterations in the formula were effected by aid of graphical representations of the various sets of gaugings. It was found, in investigating the various experimental results, that the factor c varied generally with the slope; but a somewhat anomalous result was also noted—namely, that whereas in the Mississippi observations c increased with a decrease in the slope, it on the contrary decreased with a decrease of slope in the gaugings of small channels, unless the wetted

perimeter was very rough. This change in the variation of c with relation to the s lope was found to depend upon the hydraulic radius being greater or less than 3·281 feet; so that c becomes independent of the change in slope when R approximates to this value, though the actual value of R at which the modification occurs varies with the degree of roughness of the channel. This result is attributed to the conflicting currents and eddies in large rivers having irregular beds, or in small channels with very rough beds, which are intensified by an increase in the slope; whereas, in small streams flowing in confined channels with smooth beds, an increased velocity tends to dissipate retarding lateral movements. A preliminary form adopted for the value of c was $\dfrac{a + \frac{l}{n}}{1 + \frac{an}{\sqrt{R}}}$, where $a + \frac{l}{n}$ replaces y in the original formula, and $an = x$, or $x = ny - l$, in which a is a constant with value 41·66 in English measures; l is another constant, equal to $\sqrt{R}$ when R has the special value 3·281 referred to above, and therefore 1·811; and n is the coefficient of roughness, varying, according to the state of the channel, from 0·009 to 0·040. The above value of c suffices for the flow in pipes and other small channels with steep slopes, owing to the small influence of a variation of slope on the coefficient c in such cases; but for ordinary channels allowance has to be made for variations in slope, necessitating the introduction of another variable factor into the expression for c. The final shape given to the value of c by Messrs. Ganguillet and Kutter, in their general formula, was $\dfrac{a + \frac{l}{n} + \frac{m}{S}}{1 + \left(a + \frac{m}{S}\right)\frac{n}{\sqrt{R}}}$, where $m = 0·0028075$, for English measures, is a constant of a hyperbola employed in constructing the formula. The general formula, accordingly, became, for English measures—

$$V = \frac{\frac{1·811}{n} + 41·6 + \frac{0·00281}{S}}{1 + \left(41·6 + \frac{0·00281}{S}\right)\frac{n}{\sqrt{R}}}\sqrt{RS},$$

where V is the mean velocity in feet per second, which multiplied by the cross-section would give the discharge in cubic feet per second, and S is the actual slope.

The main interest of the book consists in the clear exposition of the several steps by which the formula was reached; and even if at some future time, by the aid of fresh observations and more accurate experiments, the formula should be superseded by a more comprehensive and exact one, the merit of this work as an elaborate scientific investigation for a general empirical formula must always remain; and the book would deserve to be consulted on this ground alone. The formula depends entirely upon the exactness of the observations upon which it has been based. Mr. Révy has questioned the accuracy of the Mississippi experiments, owing to the use of double floats; and if fresh investigations should establish the inaccuracy of any of the observations made use of, or if further experiments should extend the scope of the inquiry, or bring new facts to light, a modified formula will be required. The authors, however, of the formula do not regard it as final or complete, nor do they claim for it any mathematical precision; they only consider that it agrees more closely than any previous formula with the results of recorded observations. The formula has naturally been objected to on account of its complicated appearance; but the variation due to change of slope renders this inevitable; and it has been seen that a simpler formula may be adopted for pipes, and small channels with steep slopes; and, moreover, graphical methods and tables might simplify the calculations. At the close of last year, Mr. Robert Manning, Engineer to the Board of Works in Dublin, presented a new formula to the Institution of Civil Engineers of Ireland, which, in its general form, is hardly less complicated than that of Messrs. Ganguillet and Kutter. This formula is

$$V = n\sqrt{Sg}\left\{R^{\frac{1}{2}} + \frac{0·22}{m^{\frac{1}{2}}}(R - 0·15m)\right\},$$

where n is the coefficient of roughness, g the force of gravity, and m the height of the barometric column of mercury. Mr. Manning puts it forward as simpler and better than the other, and claims for it, in a simplified form, a closer approximation to the mean of the results of seven of the best known formulæ than any other. Actual observations, however, form a surer basis upon which to establish a general formula than the results of previous formulæ; and it is upon a close concordance with very varied and accurate observations that any general formula must claim acceptance. Whatever position may in the future be assigned to the formula of Messrs. Ganguillet and Kutter, their work marks a notable step in advance, and must rank with the researches of Messrs. Darcy and Bazin, and Messrs. Humphreys and Abbot, as a record of important hydraulic investigations; and the translators have performed a valuable service in placing clearly before English readers the successive steps by which this general formula has been established.

THE COMPASS ON BOARD.

Der Kompass an Bord: Ein Handbuch für Führer von eisernen Schiffen. Herausgegeben von der Direktion der Deutschen Seewarte. (Hamburg: L. Friederichsen and Co., 1889.)

THE important subject of the magnetism of iron ships and the resulting deviations of their compasses, has, during the last fifty years, received marked attention in England from eminent men of science, attended with most valuable results for the safe navigation of our Royal and mercantile navies.

During the last thirteen years this same subject has been one of continuous inquiry at the German Naval Observatory in Hamburg, and papers have been published from time to time in the annual report of that institution, showing what had been accomplished. Combining the results of this work with those obtained from the extensive literature chiefly produced in England, Dr. Neumayer, the Director of the Observatory, has compiled the present work for the use of officers commanding the iron ships of the German mercantile navy.

Of the six chapters into which the work is divided, the first is devoted to information on the magnetism of iron

and steel, terrestrial magnetism, and the means of obtaining the three magnetic elements.

In the second chapter, the various modern forms of the mariner's compass, and instruments for adjusting compasses without sights, are described with illustrations. There is much here which should be of value to commanders of ships anxious to know as much as possible of their best friend in navigation.

It is, however, to be regretted that in some particulars both text and illustrations belong to the past, for in Fig. 38 an imperfect idea is given of Sir W. Thomson's compass. The drawing was correct for 1877, but important improvements were made ten years ago in the substitution of the wire grummet suspension for india-rubber, a change attended with marked success in vessels propelled and severely shaken by powerful engines; also, in 1881, the adoption of a total reflection prism in the azimuth mirror instead of an ordinary piece of looking-glass.

Prominence is given to the Hechelmann compass card, which is intended to combine the principles of the Thomson card (which consist chiefly of a long period of oscillation and great lightness), with a much greater magnetic moment in the Thomson-Hechelmann card, as it may be termed. The chief difference in these cards lies in the arrangement of the needles, Hechelmann's idea being to suspend more powerful needles than Thomson's near the circumference, thus bringing the weight as far as possible from the centre of the card to produce a slow period.

In bringing powerful needles so near the circumference, it is easy to see that something has been lost by Hechelmann when the quadrantal deviation is to be corrected as it should be—a correction so perfectly accomplished by Thomson. The greater weight of the card, too, tends to increase friction at the cap and pivot. Under these considerations the Thomson-Hechelmann card can hardly be considered equal to the modern Thomson.

In the next chapter, which treats of the magnetism of ships and the resulting deviation, it is satisfactory to find that the different kinds of magnetism which careful investigation has shown to exist in modern vessels are specially mentioned. These are—

(1) Permanent magnetism.
(2) Sub-permanent (termed also retentive) magnetism.
(5) Transient magnetism.

These definitions are accompanied by a footnote stating that in the English text-books on deviation no difference is made between permanent and sub-permanent magnetism, but that the two are combined under the expression sub-permanent. This is perhaps rather hard upon some English books, where, by careful reading, it will be found that the distinction is really made, but, it must be confessed, with a want of that clearness of division which is important to sound knowledge. Readers of the papers published by the Royal Society, and more recently by the Royal United Service Institution, will find that the division of a ship's magnetism into the three kinds mentioned above is strongly insisted upon.

A complete analysis of the deviations of any given compass in a ship, and of the changes which take place on a change of latitude, is necessary before a satisfactory compensation of the deviation by magnets and soft iron can be made. In the "Compass on Board," this analysis has a chapter devoted to it, containing information which should be of value both to the captains of ships and compass adjusters. It is illustrated by many examples.

Values of the coefficients v and v', representing the temporary deviation caused by running on a given course for some days, are given for a number of vessels of different types, steam and sailing. They clearly show the navigator of a new ship the need of caution when altering course, and some idea of the amount of change of deviation he may expect; whilst it should be understood that no careful seaman would fail to learn and note the peculiarities of the iron affecting his ship's compasses from personal observation under the varied circumstances experienced during each voyage.

A corrector for the deviation caused by sub-permanent magnetism has yet to be discovered.

Taking a general view of this book, it may be described as calculated to provide good practical information for the officers of the German mercantile navy, as well as a certain amount of a theoretical nature for those inclined to learn something of a ship's magnetism from a higher standpoint.

The maps of the three magnetic elements provided at the end of the book are given for the epoch 1885, and on a larger scale than those usually provided in hand-books. The accompanying map of values of the secular change is somewhat open to criticism as regards the figures recorded in the Red Sea, Bombay, East Indies, and Australia. This, however, will not prove of any detriment to safety in practical navigation.

The difficulties connected with the compass in warships, with their armoured deck, thickly-plated sides, and conning-towers, are not treated of, and their officers must look elsewhere for the special information they require; still, there is much to be found in this book that will serve their purpose.

OUR BOOK SHELF.

Library Reference Atlas of the World. By John Bartholomew, F.R.G.S. (London: Macmillan and Co., 1890.)

THE recognition of the intimate connection that exists between physiography and geography is made very manifest, in all the atlases published during the last few years, by the insertion of maps indicating the physical features of the earth's surface.

We are in an eminently utilitarian age, and a collection of maps, to meet the requirements of the day, must serve more purposes than that of a mere index to the positions of places; it must represent the most permanent features of importance in commercial geography, and the distribution of commodities as explained by the sciences of physics, geology, meteorology, biology, &c., or collectively by physiography. The elegant work before us satisfies all these requirements, it is as complete as it is a trustworthy atlas of modern geography, and will be equally appreciated by the student, the business man, and the general reader.

The atlas contains 84 maps, and amongst them we find plates delineating drainage areas, ocean currents, prevailing winds, rainfall, temperature, climate, and commercial features. A characteristic of the collection is the large number of maps that have been devoted to the British Empire, eighteen plates being given of the United Kingdom alone. India is completed in eight plates, the Dominion of Canada is very completely represented in seven plates, and the mapping of all the British possessions

has been carried out on the same elaborate scale. After the British Empire, special prominence has been given to the United States, whilst all the other countries of the world have been treated in a very comprehensive manner. The general reference index comprises the names of 100,000 places contained in the maps, and for British names it is the most complete ever published. One matter of regret, however, is that the places on some of the maps are not obviously visible because of the bright and superabundant colouring used to indicate the divisions of a country, for, generally speaking, these divisions are better represented by coloured lines. The less masking there is, the more distinct must places appear, and therefore the purpose of an atlas will be the better served. This is, however, but a minor point. The atlas is an excellent one, it is complete and accurate, contains all the results of recent exploration and geographical research, and is issued at a moderate price; its addition to every library therefore is a thing to be desired.

The Bala Volcanic Series of Caernarvonshire and Associated Rocks; being the Sedgwick Prize Essay for 1888. By Alfred Harker, M.A., F.G.S., Fellow of St. John's College, and Demonstrator in Geology (Petrology) in the University of Cambridge. (Cambridge: University Press, 1889.)

IN this useful little work, Mr. Harker has given an admirable *résumé* of the results which have, up to the present time, been arrived at by the study of the ancient igneous rocks of North Wales. Besides summarizing the work of the late John Arthur Phillips and E. B. Tawney, of Prof. Bonney, Mr. Rutley, Mr. Cole, Mr. Teall, Mr. Waller, Miss Raisin, and others who have written on the petrography of the district, he has added many new and often judicious notes on the rocks in question. A number of fresh analyses, and the description of hitherto unrecognized varieties of rocks and minerals, raise the work out of the category of mere compilations; and the excellent classification and arrangement of his materials make the book one eminently useful for purposes of reference. It is unfortunate that it has no index, though the "table of contents," which is very full and carefully paged, causes the want to be less felt than it otherwise would be. Mr. Harker classifies the districts of Caernarvonshire in which volcanic rocks are found as the *Eastern*, *North-Western*, and *Western*, the latter consisting of the Lleyn peninsula. He groups the types of rocks represented under the headings of "rhyolitic lavas," "nodular rhyolites," "acid intrusives," "intermediate rocks," "diabase sills and basalts," and "other basic intrusions." The work concludes with a "review of vulcanicity in Caernarvonshire," in which we find discussions of the relation of the volcanic eruptions to the earth-movements that took place at the period of their occurrence, the succession of lavas in the district, and the evidence in favour of their submarine origin. The book is admirably printed, and is illustrated by six very clearly-drawn sketch-maps. The essay is worthy of the memorial in connection with which it appears, and is creditable to the University under whose auspices it is issued; and higher praise than this it would be difficult to give to any work of the kind.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Inheritance of Acquired Characters.

WITHOUT expressing any opinion upon the question recently discussed in your columns under the above title, I think it may be as well to recall the belief of one whose judgment was not without weight, and to give some of the evidence on which that belief was founded.

In the first chapter of the "Origin of Species" (p. 8 of the sixth edition), Mr. Darwin says, respecting the inherited effects of habit, that "with animals the increased use or disuse of parts has had a more marked influence"; and he gives as instances the changed relative weights of the wing-bones and leg-bones of the wild duck and the domestic duck, and, again, the drooping ears of various domestic animals. Here are other passages taken from subsequent parts of the work:—

"I think there can be no doubt that use in our domestic animals has strengthened and enlarged certain parts, and disuse diminished them; and that such modifications are inherited" (p. 108). And on the following pages he gives five further examples of such effects. "Habit in producing constitutional peculiarities, and use in strengthening and disuse in weakening and diminishing organs, appear in many cases to have been potent in their effects" (p. 131). "When discussing special cases, Mr. Mivart passes over the effects of the increased use and disuse of parts, which I have always maintained to be highly important, and have treated in my 'Variation under Domestication' at greater length than, as I believe, any other writer" (p. 176). "Disuse, on the other hand, will account for the less developed condition of the whole inferior half of the body, including the lateral fins" (p. 188). "I may give another instance of a structure which apparently owes its origin exclusively to use or habit" (p. 188). "It appears probable that disuse has been the main agent in rendering organs rudimentary" (pp. 400-401). "On the whole, we may conclude that habit, or use and disuse, have, in some cases, played a considerable part in the modification of the constitution and structure; but that the effects have often been largely combined with, and sometimes overmastered by, the natural selection of innate variations" (p. 114).

In his subsequent work, "The Variation of Animals and Plants under Domestication," he writes:—

"The want of exercise has apparently modified the proportional length of the limbs in comparison with the body" [in rabbits] (p. 116). "We thus see that the most important and complicated organ [the brain] in the whole organization is subject to the law of decrease in size from disuse" (p. 129). He remarks that in birds of the oceanic islands "not persecuted by any enemies, the reduction of their wings has probably been caused by gradual disuse." After comparing one of these, the water-hen of Tristan D'Acunha, with the European water-hen, and showing that all the bones concerned in flight are smaller, he adds:—"Hence in the skeleton of this natural species nearly the same changes have occurred, only carried a little further, as with our domestic ducks, and in this latter case I presume no one will dispute that they have resulted from the lessened use of the wings and the increased use of the legs" (pp. 286-87). "As with other long-domesticated animals, the instincts of the silk-moth have suffered. The caterpillars, when placed on a mulberry tree, often commit the strange mistake of devouring the base of the leaf on which they are feeding, and consequently fall down; but they are capable, according to M. Robinet, of again crawling up the trunk. Even this capacity sometimes fails, for M. Martins placed some caterpillars on a tree, and those which fell were not able to remount and perished of hunger; they were even incapable of passing from leaf to leaf" (p. 304).

Here are some instances of like meaning from vol. ii.:—

"In many cases there is reason to believe that the lessened use of various organs has affected the corresponding parts in the offspring. But there is no good evidence that this ever follows in the course of a single generation. . . . Our domestic fowls, ducks, and geese have almost lost, not only in the individual but in the race, their power of flight; for we do not see a chicken, when frightened, take flight like a young pheasant. . . . With domestic pigeons, the length of sternum, the prominence of its crest, the length of the scapulæ and furcula, the length of the wings as measured from tip to tip of the radius, are all reduced relatively to the same parts in the wild pigeon." After detailing kindred diminutions in fowls and ducks, Mr. Darwin adds, "The decreased weight and size of the bones, in the foregoing cases, is probably the indirect result of the reaction of the weakened muscles on the bones" (pp. 297-98). "Nathusius has shown that, with the improved races of the pig, the shortened legs and snout, the form of the articular condyles of the occiput, and the position of the jaws with the upper canine teeth projecting in a most anomalous manner in front of the lower canines, may be attributed to these parts not having been fully exercised.

. . . These modifications of structure, which are all strictly inherited, characterize several improved breeds, so that they cannot have been derived from any single domestic or wild stock. With respect to cattle, Prof. Tanner has remarked that the lungs and liver in the improved breeds 'are found to be considerably reduced in size when compared with those possessed by animals having perfect liberty.' . . . The cause of the reduced lungs in highly-bred animals which take little exercise is obvious" (pp. 299–300). And on pp. 301, 302, and 303, he gives facts showing the effects of use and disuse in changing, among domestic animals, the characters of the ears, the lengths of the intestines, and, in various ways, the natures of the instincts.

Clearly the first thing to be done by those who deny the inheritance of acquired characters is to show that the evidence Mr. Darwin has furnished by these numerous instances is all worthless. HERBERT SPENCER.

LET me remind the readers of NATURE that the discussion which has been going on in these columns, between the Duke of Argyll and Mr. Thiselton Dyer, arose out of a reference in Mr. Wallace's book on "Darwinism" to the dislocation of the eyes of flat-fishes. Two views have been expressed as to the origin of this arrangement—the one endeavouring to explain it as a case in which a "sport" or congenital variation, had been selected and intensified; the other attributing it to the direct action of the muscles of ancestral flat-fishes which had pulled the eye out of its normal position, the dislocation thus established being transmitted to offspring, and its amount increased by like action in each succeeding generation. In common with Mr. Wallace and other naturalists, I spoke of this latter hypothesis as one of transmission of an "acquired character." The term "acquired character" was clearly enough defined by this example; it has been used in England for some years, and its equivalent in German (*erworbene Eigenschaften*) has been defined and used for the purpose of indicating the changes in a parent referred to by Lamarck in the following words ("Philosophie Zoologique," tome i. p. 235, édition Savy, 1873):—

"*Première Loi.*—Dans tout animal qui n'a point depassé le terme de ses developpements, l'emploi plus fréquent et soutenu d'un organe quelconque, fortifie peu à peu cet organe, le developpe, l'agrandit, et lui donne une puissance proportionnée à la durée de cet emploi; tandis que le défaut constant d'usage de tel organe, l'affaiblit insensiblement, le détériore, diminue progressivement ses facultés, et finit par le faire disparaître.

"*Deuxième Loi.*—Tout ce que la nature a fait *acquérir* ou perdre aux individus par l'influence des circonstances où leur race se trouve depuis longtemps exposée, et par conséquent par l'influence de l'emploi prédominant de tel organe, ou par celle d'un défaut constant d'usage de telle partie, elle le conserve par la génération aux nouveaux individus qui en proviennent, pourvu que les *changements acquis* soient communs aux deux sexes ou à ceux qui ont produit ces nouveaux individus."

The meaning of the term "acquired characters" is accordingly perfectly familiar to all those who have any qualification for discussing the subject at all. It is used by Lamarck, and has been used since as Lamarck used it. Naturalists are at present interested in the attempt to decide whether Lamarck was justified in his statement that acquired changes are transmitted from the parents so changed to their offspring. Many of us hold that he was not; since, however plausible his laws above quoted may appear, it has not been possible to bring forward a single case in which the acquisition of a character as described by Lamarck and its subsequent transmission to offspring have been conclusively observed. We consider that, until such cases can be produced, it is not legitimate to assume the truth of Lamarck's second law. We admit, of course, the operation of the environment and of use and disuse as productive of "acquired characters"; but we do not find any evidence that these particular characters so acquired are transmitted to offspring. Accordingly it has been held by several naturalists recently (whom I will call the anti-Lamarckians, and among whom I include myself) that it is necessary to eliminate from Mr. Darwin's teachings that small amount of doctrine which is based on the admission of the validity of Lamarck's second law. As everyone knows, Mr. Darwin's own theory of the natural selection of congenital variations in the struggle for existence is entirely distinct from Lamarck's theory, and the latter was only admitted by Darwin as being possibly or probably true in regard to some cases, and of minor importance. Although Darwin expressly states that he was more inclined to attach importance to Lamarck's theory in the later editions of the "Origin of Species," the anti-Lamarckians are convinced that it is conducive to the progress of knowledge to reject that theory altogether until (if ever) it is placed on a solid basis of observed fact; and in the meantime to try if it is possible to explain the cases which seem most favourable to Lamarck's view by the application of Darwin's own theory.

It is essential for those who are not thoroughly familiar with Darwin's writings to note that this does not involve a rejection of the conclusion that the action of external conditions upon a parent may be such as to modify the offspring. That is an important part of Mr. Darwin's own theory, and, as I recently pointed out in NATURE, it is to such action of the environment upon the parent that Mr. Darwin attributed the origin of those congenital variations upon which natural selection acts. This disturbance of the parental body (I compared it to the shaking up of a kaleidoscope), and with it of the germs which it carries, resulting in "sporting" or "variation" in the offspring, is, it should hardly be needful to state, a totally different thing to the definite acquirement of a structural character by a parent as the result of the action upon it of the environment, and the transmission to offspring of that particular acquired structural character. I am not concerned to inquire here whether, or how far, Prof. Weismann's theory of the continuity of the germ-plasm admits of the action of external forces on a parental body in such a way as to disturb the germ-plasm and induce variation. Prof. Weismann can very well defend his own views. All that I am concerned with—and that quite independently of the conclusions of Prof. Weismann—is whether it is or is not reasonable, useful, or indeed legitimate, to assume the truth of Lamarck's second law, in the absence of any direct proof that any such transmission as it postulates takes place. Those who think Lamarck's second law to be true have been urged to state (1) cases in which the transmission of acquired characters is directly demonstrated, or (2) cases in which it seems impossible to explain a given structure except on the assumption of the truth of that law. If they fail to do this, they are asked to admit that Lamarck's second law is unproven and unnecessary.

The response which has been made to this attempt to arrive at facts is beside the mark. Mr. Cope writes to NATURE merely *asserting*, "If whatever is acquired by one generation were not transmitted to the next, no progress in the evolution of a character could possibly occur,"—an opinion peculiar to himself, and certainly one which cannot be taken in place of fact. The Duke of Argyll then "interpolates" (to use his own word) a general statement of his beliefs, and in the last of his letters a statement of "what his position is." We really are not concerned in this matter with beliefs or positions. We want well-ascertained facts and straightforward reasoning from facts. The Duke of Argyll has not assisted us. When on a recent occasion he was asked to cite an instance of what he called "a prophetic germ" in the adult structure of a plant or animal having, in his opinion, such claims to this title as he had ascribed to the electric organ of skates, the Duke was unable to reply. He wrote as a substitute something about embryological phenomena, which had nothing to do with the case. He has not yet ventured to stake his oft-asserted right to offer an opinion upon zoological topics, on the reception which his attempt to deal with the details of a particular case of organic structure would obtain: in this, I think, he is wise.

The Duke similarly tries to evade the appeal to facts when he is pressed by Mr. Dyer to state cases of the transmission of acquired characters. In doing so, however, he has, it must be admitted, revealed an astonishing levity. He answers (par. 9 of his letter) that in all domesticated animals, and especially in dogs, we have constant proof that many acquired characters may become congenital. This is mere assertion; we require details. It is maintained, on the contrary, by anti-Lamarckians that the whole history of artificial selection, and of our domesticated animals, furnishes a mass of evidence against the theory of the transmission of acquired characters, since if such cases occurred they would be on record, and moreover would have been utilized by breeders.

The subsequent proceeding of the Duke is almost incredible. In the following paragraphs of his letter he gives up his contention that acquired characters are transmitted, coupling his retreat with unwarrantable charges against those who have lately raised the question as to whether this is the case or not. He correctly states what is meant by the term "acquired characters," and declares that this meaning has been expressly invented for the purposes of the present discussion by "for-

tuitists," and is "irrational." A more baseless charge was never yet made in controversy, nor a more obvious attempt to alter the terms of discussion so as to give some appearance of plausibility to a lost cause. The Duke, in fact, now at length tells us that *he* does not mean by "acquired characters" what *we* mean. Why then did he "interpolate" his remarks on the subject and make use of the term ?

If the meaning which the phrase has for the scientific world generally be insisted upon, we are now, it appears, to understand that the Duke of Argyll agrees with us: what *we* mean by "acquired characters" are not, he admits, shown to be transmitted.

"Fortuitists," the Duke says, "have invented a new verbal definition of what they mean by 'acquired.'" I have shown at the commencement of this letter that the term "acquired" is used to-day as it was by Lamarck. To the Duke this meaning is "new"—because he has either never read or has forgotten his Lamarck. If this be so, the Duke has been writing very freely about a subject with which his acquaintance is very small. The alternatives are as clear as possible: either the Duke of Argyll knew the significance of the term "acquired characters" as employed by Lamarck, in which case it would have been impossible that he should charge those whom he calls "fortuitists" with having invented a new verbal definition of what they mean by "acquired"; *or* he did not know Lamarck's use of the phrase, and was therefore not qualified to offer an opinion in the discussion, nor to press his "beliefs" and "position" upon public attention.

I have no time and you have no space to devote to a full exposure of the character of other assertions made in the Duke of Argyll's "statement of his position" which are as reckless and demonstrably erroneous as that concerning the meaning of the term "acquired."

Perhaps the most flagrant of these is the assertion that "the theory of Darwin is essentially unphilosophical in so far as it ascribes the phenomena of variation to pure accident or fortuity" (paragraph 4). Of course the Duke cannot be acquainted with the following passage from the "Origin of Species," sixth edition, p. 106; but if he has to plead ignorance of the writings not only of Lamarck, but also of Darwin, what is the value of his opinions and beliefs on Lamarckism and Darwinism? The words of Mr. Darwin referred to are these:—"I have hitherto sometimes spoken as if the variations, so common and multiform with organic beings under domestication, and in a lesser degree with those under nature, were due to chance. This, of course, is a wholly incorrect expression, but it serves to acknowledge plainly our ignorance of the cause of each particular variation."

Whatever meaning the Duke may attach to the word "fortuity," it is mere empty abuse on his part to call the later Darwinians "fortuitists," and still less justifiable to insinuate that their investigations and conclusions are not guided by a simple desire to arrive at truth, but by the intention of propping up a worship of Fortuity. It is natural for the Duke to suppose it impossible to write on Darwinism without some kind of theological bias.

In conclusion, I venture to point out that the Duke of Argyll has (1) failed to cite facts in support of his assertions of belief in "prophetic germs," and "transmission of acquired characters" when challenged to do so; (2) that he displays ignorance of two of the most important passages in the works of Lamarck and of Darwin, whom he nevertheless criticizes, and in consequence of his ignorance completely, though unintentionally, misrepresents; and (3) that he has introduced into these columns a method of treating the opinions of scientific men, viz. by insinuation of motive and by rhetorical abuse, which, though possibly congenial to a politician, are highly objectionable in the arena of scientific discussion.

February 22. E. RAY LANKESTER.

Physical Properties of Water.

As you inform me that my anonymous critic (*ante*, p. 361) does not intend to avail himself of the opportunity I gave him (through you) of correcting his misstatements about my *Challenger* Report, I must ask to be permitted to correct them myself.

(1) There is nothing whatever in my Report to justify the critic's statement that I "*had never heard of* Van der Waals' work . . . till the end of the year 1888." Yet this is made the basis of an elaborate attack on me!

What I did say was to the effect that I was not aware, till Dr. Du Bois told me, that Van der Waals had given numerical estimates of the value of Laplace's K. I had long known, from the papers of Clerk-Maxwell and Clausius, the main features of Van der Waals' investigation. But I also knew that Maxwell had shown it to be theoretically unsound; and that Clausius had taken the liberty of treating its chief formula as a mere empirical expression, by modifying its terms so as to make it better fit Andrews' data. This paper of Clausius is apparently unknown to my critic, as is also my own attempt to establish (on defensible grounds) a formula somewhat similar to that of Van der Waals.

(2) I said nothing whatever about the "Volume of Matter in unit volume of Water." Hence the critic's statement, "Prof. Tait's value is 0·717," is simply without foundation.

I merely said that the empirical formula

$$p(v - a) = \text{constant},$$

if assumed to hold for all pressures, shows that a is the volume when the pressure is infinite. I still believe that to be the case. If not, Algebra must have changed considerably since I learned it.

My critic speaks of a totally different thing (with which I was not concerned), which may be $a/4$ or $a/4\sqrt{2}$, or (as I think is more plausible) $a/8$. But he says that liquids can be *compressed* to 0·2 or 0·3 of their bulk at ordinary temperatures and pressures. I was, and remain, under the impression that this could be done *only at absolute zero*, and then no compression is required.

There are other misrepresentations of my statements, quite as grave as those cited. But it would be tedious to examine them all. I have no objection to a savage review, anonymous or not; on the essential condition, however, that it be *fair*. It is clear from what I have shown that this essential condition is absent.

But my critic, when his statements are accurate, finds fault with the form of my work. I will take two examples of this kind, and examine them.

(3) He blames me for not using C.G.S. units. The *Challenger* Reports are, as a rule, written in terms "understanded of" nautical men. I wonder what such men would have said of me, in their simple but emphatic vernacular, if I had spoken of a pressure of 154,432,200 C.G.S. units, when I meant what they call a "ton"; or, say, of 185,230 C.G.S. units, when I meant a "naut."

(4) I am next blamed for "mixing units."

I should think that if we could find a formula expressing, in terms of a man's age, the average rate at which he can run, say for instance

$$v = \frac{Ax(B - x)}{x^2 + C},$$

even my critic would express A in feet per *second*, and take x as the mere number denoting the age in *years*. Would he, alone in all the world, insist on expressing x as denoting the age in *seconds* in order to prevent what he calls the mixing of units? This is a case precisely parallel to the one in question.

Generally, I would remark that my critic seems to have written much more for the purpose of displaying his own knowledge than of telling the reader what my Report contains. For at least three of the most important things in my Report are not even alluded to:—the compressibility of mercury, the *nature* of Amagat's grand improvement of the *Manomètre Desgoffes*, and (most particularly) the discussion of the wonderful formula for the compressibility of water given in the splendid publications of the *Bureau International*.

P. G. TAIT.

THE last volume of the *Challenger* Reports contains papers on various branches of science. The review which appeared in NATURE was not the work of one writer, and was therefore not signed, but I have no desire to avoid taking full responsibility for the part of which I am the author.

It will be convenient to reply to Prof. Tait in paragraphs numbered to correspond with his own.

(1) Of course I fully accept Prof. Tait's account of his knowledge of Van der Waals' theory at the time when his *Challenger* Report was written, but I entirely dissent from his statement that what he said about it in the Addendum referred to in the review was "to the effect" described above.

It is hardly possible to do justice to my own case without quoting freely, but I will compress as much as possible. He

says (p. 60) that he "was informed" (which implies that he did not previously know) that "one of Van der Waals' papers . . . contains an elaborate study of the molecular pressure in fluids."

Again he says, "I have left the passages . . . which refer to this subject in the form in which they stood before I became acquainted with Van der Waals' work. I have not sufficiently studied his memoir to be able as yet to form a definite opinion whether the difficulty . . . which is raised in Appendix E. can, or cannot, be satisfactorily met by Van der Waals' methods."

Further, he states that he "had been under the impression . . . that Laplace's views had gone entirely out of fashion—having made, perhaps, their final appearance . . . about 1850."

As a matter of fact, Van der Waals adopted Laplace's views in 1873, and his formula differs from the expression $pv = \mathrm{RT}$, only by the introduction of two terms, one of which is obviously an additional pressure such as is deduced from Laplace's theory.

I do not think that any reader could be expected to conclude from these passages in Prof. Tait's Addendum that when writing the paper he had long known the "main features of Van der Waals' investigation." To me they seemed to mean that he had not previously been acquainted with Van der Waals' work, nor with his methods, nor with the facts that he studied molecular pressure and adopted Laplace's ideas.

While, therefore, I willingly submit to Prof. Tait's correction of the phrase that he had "never heard of Van der Waals," I cannot admit that, on the evidence then before me, I did him any substantial injustice.

(2) I very much doubt whether the distinction between the ultimate volume and the molecular volume can be maintained if the equations are treated as empirical; and even if they are not, I doubt whether the ultimate volume, as defined by Prof. Tait, has any real physical meaning. The value of v when $p = \infty$ is independent of the temperature, whether deduced from the theoretical formula to which Prof. Tait refers (p. 48), or from those of Van der Waals or Clausius: hence it must (from this point of view) be the molecular volume. In the case of Prof. Tait's new equation, which was published after his Report was completed, and which is the only one I had not seen when I wrote the review, the results when we put $p = \infty$ or $\mathrm{T} = 0$, are such as to show that its application to these extreme cases is not legitimate. My own view is that such algebraical solutions are worth very little, and I only discuss them because I wish to show that if we admit them at all they justify my treating Prof. Tait's number as an estimate of the molecular volume.

(3) I cannot say that I think that Prof. Tait's reason is adequate. The Royal Naval College at Greenwich has done more for our naval officers than he would have us believe, and, if it were not so, the *Challenger* Reports are not addressed to members of any one profession, nor intended for English-speaking scientific men alone. Their cosmopolitan character is shown by the fact that bound up in the same volume with Prof. Tait's Report is another by a distinguished Belgian geologist.

Foreigners have helped to describe the specimens which our Expedition collected; they will read the Reports which our experts have written. It would have required but a few minutes' work, and a few additional lines of print, to have given the final results in terms which they would have understood at a glance.

(4) The analogy is fallacious. Prof. Tait has devised a formula into which he introduces two quantities (age and speed), which are commonly expressed with reference to different units of time.

I pointed out that he had expressed in the same formula (contrary to common usage) the same quantity (pressure) in terms of two different units, of which one is not ordinarily used by many of those who will make use of his work.

As to the last paragraph, I have only two remarks to make, First, that I think Prof. Tait does himself injustice in regarding a description of apparatus devised by another, and the discovery of a blunder of the Bureau International, as two of the most important things in his Report. Secondly, that I think the imputation of motives should be banished from scientific discussions.

In conclusion, I wish to add that probably I should have left Prof. Tait's defence unanswered if he had not accused me of unfairness. I have no desire for any controversy, and no wish to impugn his knowledge of the theory of gases. But he will forgive my reminding him of the old saying, "*Noblesse oblige.*" A classical research should not be published in a state which leads the reader to the conclusion that the author was only just becoming acquainted with facts which bear upon his work and have been long before the world. As a reviewer, I formed the opinion that the Report under discussion was open to this criticism. As a reviewer, it was my duty to express my opinion in all honesty, and, as I hope, in all courtesy.

ARTHUR W. RÜCKER.

Visualized Images produced by Music.

IN the annexed paper, and in her own words, are related the very curious effects produced on a lady friend by certain musical tones and orchestral combinations. They are so very singular, so entirely outside my experience, and, withal, so inexplicable, that I shall be glad if you will give them a place in your columns, in the hope that some of your readers—physiological or psychological—may be able to throw some light on them.

I should state that the lady is in perfect health, is very intelligent, an accomplished musician, and not at all, in this or any sense, the victim of a disordered imagination. She is quite conscious that these spectral images have only a subjective existence, though visually they have all the vividness of presentment which belongs to realities.

At the first blush it would seem as though these apparitions were in some way a response to stimuli sent through the auditory nerve; but this, if any, is an imperfect explanation, since it will be noticed that occasionally these visualized pictures *slightly precede the instrument they belong to.*

This fact suggests that a state of unconscious expectancy may be a factor in their reproduction, but it fails entirely, I think, to account for their initial appearance. GEO. E. NEWTON.

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"The sound of an oboe brings before me a white pyramid or obelisk, running into a sharp point; the point becoming more acute if the note is acute, blunter if it is grave. The obelisk appears to be sharply defined and solid if the note is loud, and vague and vaporous if it is faint. All the notes of the 'cello, the high notes of the bassoon, trumpet, and trombone, and the low notes of the clarionet and viola, make me see a flat undulating ribbon of strong white fibres.

"The tone of the horn brings before me a succession of white circles of regularly gradated sizes, overlapping one another. These circles and the ribbon float past me horizontally, but the point of the obelisk seems to come at me.

"In an orchestra, when the violins strike up, after the wind band has been prominent for a time, I see often, but not always, a shower of bright white dust or sand, very crisp and glittering. I am taking note of the recurrence of this impression, and think it is becoming more frequent, but it is not invariable like the others.

"I have heard a great deal of orchestral music all my life, but I have only noticed these effects for four or five years. They gained gradually in frequency and clearness, and now the first three are invariable.

"If I know the scoring of a piece well, the various effects *slightly precede* the instrument they belong to; only the objects are vague and faint till the sound begins.

"Sometimes, if an oboe passage has an intense and yearning character, the white point comes so near me, and moves so rapidly, that I think it *must wound me.*

"I am very anxious to make it clear that I am not trying to describe a mental state by symbols, but that *I actually see* the point, the fibres, and the circles. Generally they seem to float half-way between me and the orchestra.

"If only one class of instruments is used, the effect does not extend beyond the opening bars: for instance, in a string quartette I only see the white sand for a moment at the beginning; if, however, wind and stringed instruments are combined, I see the various effects again and again in one piece."

Foreign Substances attached to Crabs.

IN your issue of December 26, 1889 (p. 176), Mr. Pascoe drew attention to the cases of certain crabs which are frequently found covered with sponges, algæ, shells, &c., and brought forward also the well-known case of the Gastropod *Phorus*. He at the same time confessed that he could not see "where protection came in" in any of the cases which he cited. Mr. A. O. Walker, on the other hand (NATURE, January 30, p. 296), regards it as obvious that the attachment of these foreign substances is a useful adaptation for purposes of concealment. Prof. Herdman also (NATURE, February 13, p. 344) bears witness to the

"scarcely recognizable" appearance of the crab *Hyas* when covered with algæ, &c. Indeed, no one who has seen one of these crabs brought up with the dredge, or has found a well-covered *Stenorhynchus* on our own shores, can seriously doubt the usefulness of the habit in rendering the animal inconspicuous. In *Stenorhynchus* and *Inachus* the process of "dressing" with weeds and zoophytes has been described by Bateson (Journ. Mar. Biol. Association, vol. i. 1889, p. 213), and it is seen from his description that, as also in the cases of *Dorippe*, *Pagurus*, *Dromia vulgaris*, &c., the foreign substances or animals become attached to the body not by accident but by the act of the crabs themselves.

Now Mr. Walker, in regarding *all* these cases as instances of adaptation for concealment, has overlooked the fact that in two of our British species of hermit crab (*Pagurus bernhardus* and *P. prideauxii*) it is the habit of the animals to prefer, and often to fight for, shells which are rendered conspicuous by the attachment to them of species of Anemone, in the one case *Adamsia rondeletii* (*Sagartia parasitica*), in the other *Adamsia palliata*. Another British species (*Pagurus cuanensis*) is almost invariably found inhabiting a shell enveloped in the sponge *Suberites domuncula*, which is frequently of a conspicuous orange-red colour Only in the smallest species of *Pagurus* (*e.g.*, *P. lævis*) does the animal depend invariably upon an inconspicuous appearance for its safety.

The value to the crabs of a preference for shells to which Actinians are attached is found in the fact that these gaily-coloured animals are carefully shunned by fishes on account of their stinging powers; and although hermit crabs themselves are very palatable to fishes, their association with Actinians, while rendering them conspicuous as they move about, is at the same time an efficient protection from the persecution of their enemies.

This also explains the habits of the two Mauritian crabs, which, according to Möbius, carry about a sea-anemone in each claw.

The sponge with which *Pagurus cuanensis* is associated is (like all other sponges with which I have experimented) exceedingly obnoxious to fishes on account of its bad smell and taste. I have never succeeded in inducing a fish of any species to swallow a fragment of the sponge; but on the contrary the smell is in most cases quite sufficient to drive the fish away. The association with the sponge is therefore here also an efficient protection, for I know of no fish capable of extracting the crab from its retreat. It is seen from this that the case of *Dromia vulgaris* should probably be removed from the category of adaptations for concealment, and, like the cases of *P. bernhardus*, &c., be included in a special group of *warning* adaptations.

There yet remains the interesting case, adduced by Dr. R. von Lendenfeld, of *Dromia excavata* associated with a Compound Ascidian of the genus *Atopogaster* (Herdman). This, I believe, will be found to belong to the same category of warning adaptations, for after repeated experiments with Compound and other *Tunicata* at the Plymouth Laboratory I can state that these animals are essentially inedible to fishes. The inedibility is in large part due, as in the case of sponges, to the characteristic odour which *Tunicata*, and more especially Compound *Tunicata*, give out, and in no family (excepting perhaps the *Botryllidæ*) is this better marked than in the *Polyclinidæ*, the group to which *Atopogaster* belongs. Bearing in mind also the fact that Composite Ascidians frequently vie with sponges and Actinians in the possession of varied and conspicuous colours, it is rendered practically certain that the case of *Dromia excavata* is another instance of this same type of adventitious warning contrivances.

Thus the edible (the edibility is not yet proved for foreign species) *Crustacea* which attach foreign substances to their bodies may be divided into two groups:—

(α) Those which are rendered inconspicuous in relation to their natural surroundings by the habit; *e.g.*, *Stenorhynchus*, *Hyas*, *Dorippe*, *Pagurus lævis*, and young forms of *Pagurus bernhardus*, &c.

(β) Those which associate themselves with animals, easily recognizable by, and possessing qualities offensive to, their chief enemies; *e.g.*, *Dromia vulgaris* and *excavata*, *Pagurus bernhardus*, *prideauxii*, and *cuanensis*.

WALTER GARSTANG.

Laboratory of the Marine Biological Association, Plymouth, February 21.

P.S.—From facts which Mr. Weldon and Mr. Harmer have communicated to me, it would appear that *Dromia vulgaris* frequently attaches Compound Ascidians (*Leptoclinum maculosum*, *Botrylloides Gasconiæ*) to its back instead of sponges, a variation of habit which is very interesting in connection with the apparently fixed habit of the Australian species.—W. G.

A Key to the Royal Society Catalogue.

"A CATALOGUER" appears to have misunderstood me in two points. In the index that I propose, the heads would not be numbered. Again, in forming an estimate of the size of the work, I made the supposition that the 8 papers of an author could be grouped, not under 8, but under 3 heads.

JAMES C. MCCONNEL.

A Meteor.

LAST night (Monday, the 3rd), as I was crossing the Old Deer Park to Richmond, I witnessed the flight of an exceptionally fine meteor, which shone out with great brilliancy notwithstanding the presence of a bright moon, which was almost at the full.

It appeared to start from the constellation of Leo, and travelled across the sky to the westward, vanishing some 10° or 15° above the horizon.

The night was very quiet at the time, and I heard no report.

T. W. BAKER.

Kew Observatory, Richmond, Surrey, March 4.

THE DISCOVERY OF COAL NEAR DOVER.

THE question of the existence of coal under the newer rocks of Southern England, which has engaged the attention of some of our leading geologists since the year 1855, has found its final answer in the discovery announced last week in the daily press. The story of the discovery is a striking example of the progress of a scientific idea, passing through various phases, and growing more clearly defined through opposition and failure, until ultimately it has been proved to be true, and likely to lead to industrial changes of national importance.

The question was originally started 35 years ago by Mr. Godwin-Austen in a memorable paper brought before the Geological Society of London, in which it was argued, from the character and arrangement of the coal-fields and associated rocks of Somersetshire and South Wales on the west, and of the Belgian and North French coal-fields on the east, that similar coal-fields lie buried beneath the newer strata of the intervening regions. Mr. Godwin-Austen pointed out that the general direction of the exposed coal-fields was ruled by a series of great east and west folds, running parallel to the great line of disturbance—"the axis of Artois,"—from the south of Ireland, through South Wales and Northern Somerset on the west, eastwards through Belgium and Northern France, into the valley of the Rhine, near Düsseldorf. Throughout this area the exposed coal-fields lie in long east and west troughs. This series of folded Carboniferous and older rocks formed also an east and west ridge along the line of the axis of Artois, which gradually sank beneath the waves of the Triassic, Liassic, Oolitic, and Cretaceous seas. Against this the strata of the three first of these rocks gradually thin off, while the coal-measures and other rocks of the ridge have repeatedly been struck in France and Belgium, and are now being worked immediately underneath the Cretaceous strata over a wide area.

The axis of Artois also, where it is concealed by the newer rocks in the south of England, is marked from Somerset eastwards by the anticlinal of the chalk of North Wiltshire, and the line of the North Downs, the general law seeming to be "that when any great folding and dislocation of the earth's crust has taken place, each subsequent disturbance follows the very same lines, and that simply because they are lines of least resistance."

Mr. Godwin-Austen, by combining all these observations, finally concluded that there were coal-fields beneath the Oolitic and Cretaceous rocks of the south of England;

and that they were sufficiently near the surface to allow of their being of great economic value. He further specified the line of the Thames Valley, and the region of the Weald, as possible places where they might be discovered.

These important conclusions were during the next 11 years generally received by geologists, with the exception of Sir Roderick Murchison. The next important step in the direction of their verification was that taken by the Coal Commission of 1866–67, by whom Mr. Godwin-Austen and Sir R. Murchison were examined at length, and the results of the inquiry embodied in the Report by Mr. Prestwich. In the Report, Mr. Godwin-Austen's views are accepted, and fortified by a vast number of details relating both to the coal-fields of Somersetshire and of France and Belgium. Mr. Prestwich also calls special attention to the physical identity of the coals of these two regions, and to the fact that the Carboniferous and older rocks in both are similarly disturbed. He concludes, further, that the coal-fields which now lie buried beneath the newer rocks are probably equal in value and in extent to those which are exposed in Somerset and South Wales on the west, and in Belgium and France on the east.

In 1872 the Coal Commission Report was published, and in the same year the Sub-Wealden Exploration Committee was organized[1] by Mr. Henry Willett to test the question of the existence of coal in the Wealden area by an experimental boring. The site chosen was Netherfield, near Battle, in Sussex, where the lowest rocks of the Wealden formation form the bottom of the valley. It was resolved to go down to the older Palæozoic strata, which were thought to occur at about 1000 feet from the surface, or to carry the bore-hole to 2000 feet if they were not struck before. The work was carried on under considerable difficulties for the next three years, until in 1875 it had to be abandoned at a depth of 1905 feet, because of the breakage of many hundred feet of lining-pipes, coupled with the loss of the boring-tool at the bottom. The section of the strata passed through is as follows:—

Netherfield Section.

	Feet.
Purbeck strata	200
Portland strata	57
Kimmeridge Clay[2]	1073
Corallian rocks[2]	515
Oxford Clay	60
	1905

This section, although it yielded no information as to the Palæozoic rocks, showed that in this particular district they are more than 1900 feet beneath the surface, and revealed the great thickness of the Kimmeridge Clay and Corallian rocks, sufficiently distant from the ridge of coal-measures and older rocks, against which the Oolitic strata thin away to the north, to allow of an accumulation of Oolitic sediments to a thickness of more than 1700 feet. In this respect, therefore, it afforded unmistakable evidence that the search for the ridge in question might be carried on with much greater chance of success further to the north, in the direction of the North Downs. The great and increasing thickness of the successive newer rocks of the Wealden formation, which form the surface of the ground between Netherfield and the North Downs, rendered it undesirable to repeat the experiment within the Wealden area proper. Close to Battle, the Secondary strata were of great thickness, and where the whole series of Wealden rocks were present, they were more than 1000 feet thick.

For the next eleven years the problem remained where it was left by the results of the Netherfield boring; while in the district of London, evidence was being collected in various sinkings for water, which proved the existence of the Palæozoic ridge of rocks, Silurian and old red sandstones, older than the Carboniferous, at about 1000 feet from the surface. Here, too, the Oolitic strata were not more than 87 feet in thickness, at their thickest point in the well at Richmond. The older rocks, moreover, were inclined at a very high angle, as in the case of the similar rocks underlying the coal-fields of Somerset, and of Northern France and Belgium, and this implied the existence of troughs of coal-measures in the synclinal folds, in neighbouring areas.

I come now to the last experiment, which has been so fortunately crowned with success. In 1886, I reported to Sir Edward Watkin that it was desirable, both on scientific and commercial grounds, for a boring to be put down in South-East Kent, in the neighbourhood of Dover, and that the Channel Tunnel works under the Shakespear Cliff would be the best site for the experiment. It was almost within sight of Calais, where the coal-measures had been proved at a depth of 1092 feet. It was also not many miles away from the spot where a large mass of bituminous material—which, according to Mr. Godwin-Austen, was the result of the distillation of coal from the measures beneath—had been discovered in the chalk. Sir Edward Watkin acted with his usual energy on my report, and the work was begun in 1886, and carried on, under my advice, down to the present time. The boring operations have been under the direction of Mr. F. Brady, the chief engineer of the South-Eastern Railway, to whose ability we owe the completion of the work to its present point, under circumstances of great difficulty. The strata passed through may be generalized as follows:—

Section at Shakespear Cliff, Dover.

	Feet.
Lower Grey Chalk, and Chalk-Marl	500.
Glauconitic Marl	
Gault	
Neocomian	
Portlandian	660.
Kimmeridgean	
Corallian	
Oxfordian	
Callovian	
Bathonian	
Coal-measures, sandstones, and shales and clays, with one seam of good blazing coal, struck at 1180 feet from the top of the bore-hole	20.

The coal-measures were struck at a depth of 1160 feet, or 68 feet below the point where the coal-measures were met with in the boring at Calais. It may also be noted as a remarkable confirmation of Mr. Godwin-Austen's views as to the abrupt thinning off of the Wealden strata, that, although along the line of the North Downs the Weald clay strikes towards the French coast, and is seen at low water between Hythe and Folkestone, it and the underlying Wealden strata are not represented in the section at the Shakespear Cliff.

It is too soon as yet to measure the full value of this discovery near Dover, while our work is as yet unfinished. We may, however, remark that the coal-fields of the Continent, which have been proved beneath the newer rocks in Northern France and Belgium, some 60 miles to the west of their eastern outcrops, have now been traced across the Channel, that they are at a workable depth, and that we have now a well-defined base for further researches in Southern England.

W. BOYD DAWKINS.

[1] The Committee consisted of Profs. Ramsay and Phillips, Sir John Lubbock, Sir Philip Egerton, and Messrs. Thomas Hawksley, Warington Smyth, Prestwich, Bristow, Etheridge, Boyd Dawkins, and Topley.

[2] The precise boundary between these two groups is uncertain. If the Kimmeridge Clay series be taken down to the Coralline Oolite, its thickness will be 1512 feet.

THE RELATION BETWEEN THE ATOMIC VOLUMES OF ELEMENTS PRESENT IN IRON AND THEIR INFLUENCE ON ITS MOLECULAR STRUCTURE.

IN a lecture on the Hardening and Tempering of Steel, published in November last (NATURE, vol. xli. pp. 11, 32), an attempt was made to set forth the prominent facts developed in recent researches, more especially those of M. Osmond, which tend to prove that iron, like many other elements, can pass from the normal state to an allotropic one. It was shown that as a mass of iron or steel cools down, there are at least two distinct evolutions of heat, one occurring at a variable temperature not higher than 855° C., the other at a more constant temperature, near 650° C. From a long series of most patient investigations, Osmond argues that there are two kinds of iron, one [hard] β iron, and the other [soft] α iron. The molecular change from β to α iron is indicated by the first evolution of heat in the cooling mass of iron or steel, and at this point the cooling mass of iron regains the magnetic properties which it loses at higher temperatures. The second evolution of heat only occurs in carburized iron or steel, and marks the point at which carbon itself changes from the dissolved or 'hardening-carbon,' to the state of combined or 'carbide-carbon.' In highly carburized steel, the two points at which heat is evolved coincide, and experimental evidence has been given (*loc. cit.* p. 34) as to the abnormal molecular weakness which is exhibited when a very hot bar of such steel cools down to about 660° C. In a recent communication to NATURE (February 20, p. 369), Prof. Carl Barus, of Washington, has pointed out, with reference to this molecular weakness, "that when iron passes through the temperature of recalescence its molecular condition is almost chaotic"; whilst with regard to Osmond's view that α iron passes to β iron when submitted to any stress which produces permanent deformation of the mass, Prof. Barus says that "there is reason to be urged even in favour of the extreme view" that such molecular change may be produced in most metals. In the lecture at Newcastle, I expressed the belief (NATURE, *loc. cit.*) that it would be shown that the influence of small quantities of other elements on masses of iron would be found not to be at variance with the periodic law. I had already given experimental evidence to show that the action of small quantities of impurity on the tenacity of gold was closely in accordance with that law, but in the case of iron it was difficult to say what property of the metal would be most affected by the added matter. It appeared safe, however, to point to the possibility that the direct connection with the periodic law would "be traced by the effect of a given element in retarding or promoting the passage of ordinary iron to the allotropic state," a point of much importance, as the mechanical properties of the metal must depend on the atomic arrangement in the molecules.

I am glad that so eminent an authority and admirable experimenter as M. Osmond has satisfied himself as to the probable accuracy of this view. In two recent papers communicated to the Académie des Sciences, the results of his experiments are given, and the following is a translation of the later of these (*Comptes rendus*, vol. cx. p. 346):—

"Within the last few years and quite recently (*Comptes rendus*, Séances des 26 octobre et 6 décembre 1886, 4 avril 1887, et 3 février 1890), I have had the honour to submit to the Academy facts relating to the allotropic modifications of iron, and to the part played in such changes by foreign bodies alloyed with the mass. Prof. Roberts-Austen, by studying the effect produced on the mechanical properties of gold by the addition of the same weight (about 0·2 per cent.) of seventeen foreign metals, has discovered a curious relation between the results obtained and the position occupied by the added metals in the periodic classification (Phil. Trans. Roy. Soc., vol. clxxix. p. 339, 1888). Prof Roberts-Austen has deduced from this that an analogous relation should exist for iron, but the irons and steels of commerce are such complex products, and the same metal may assume such different aspects, that the relation in question is not readily apparent from a study of their mechanical properties.

"In reviewing my former experiments with these new ideas as guides, it appeared to me that the law of Roberts-Austen was well based, and new experiments undertaken to verify it have only confirmed my first view.

"The foreign elements whose action on the critical points of iron I have studied experimentally with more or less completeness, are ranged as follows in two columns in the order of their atomic volumes:—

I.	Atomic volume.	II.	Atomic volume.
Carbon	3·6	Chromium ...	7·7
Boron	4·1	Tungsten	9·6
Nickel	6·7	Silicon	11·2
Manganese ...	6·9	Arsenic	13·2
Copper	7·1	Phosphorus ...	13·5
		Sulphur	15·7

"The elements in column I., whose atomic volumes are smaller than that of iron (7·2), delay during cooling, *cæteris paribus*, the change of β [hard] iron to α [soft] iron, as well as that of 'hardening-carbon' (*carbone de trempe*) into 'carbide-carbon' (*carbone de recuit*). For these two reasons they tend to increase, with equal rates of cooling, the proportion of β iron that is present in the cooled iron or steel, and consequently the hardness of the metal. Indeed, their presence is equivalent to a more or less energetic hardening.[1]

"On the other hand, the elements of column II., whose atomic volumes are greater than that of iron, tend to raise or at least to maintain near its normal position, during cooling, the temperature at which the change of β to α iron takes place; further, they render the inverse change during heating more or less incomplete, and usually hasten the change of 'hardening-carbon' to 'carbide-carbon.'[2]

"Thus they maintain the iron in the α [soft] state at high temperatures, and must therefore have the same effect in the cooled metal. In this way they would act on iron as annealing does, rendering it soft and malleable, did not their individual properties, or those of their compounds, often intervene and partially mask this natural consequence of their presence.

"The essential part, therefore, played by foreign elements alloyed with iron, is either to hasten or delay the passage of iron, during cooling, to an allotropic state, and to render the change more or less incomplete in one sense or the other, according to whether the atomic volume of the added impurity is greater or less than that of iron. In other words, foreign elements of low atomic volume tend to make iron itself assume or retain the particular molecular form that possesses the lowest atomic volume, whilst elements with large atomic volume produce the inverse effect.

"It should be noted that carbon, whilst obeying the general law, possesses on its own account the property of undergoing, at a certain critical temperature, a change the nature of which is still disputable, although its existence is acknowledged. It is this property which gives carbon a place by itself in the metallurgy of iron."

M. Osmond has shown me the curves which represent the results of his experiments, and these will doubtless

[1] To the elements of column I. hydrogen may be added. As is well known, this element renders electro-deposited iron hard and brittle; perhaps it would be better to say with Graham *hydrogenium*, for hydrogen gas does not appear to have a marked influence on the critical temperature.

[2] Tungsten alone presents certain anomalies.

soon be published. Whatever may ultimately prove to be the true nature of the molecular change which accompanies the thermal treatment of iron and determines its mechanical properties, there is little doubt but that there is a close relation between the action of foreign elements and their atomic volume. Few metallurgial questions are of greater interest at the present time than those which relate to the molecular structure of metals, and the admirable work of M. Osmond has shown it to be very probable that the presence of a small quantity of a foreign metal may cause a mass of another metal to pass into an allotropic state. In relation to iron and steel the problems are of great industrial importance, and it is fortunate that we appear to be nearing the discovery of a law in accordance with which all metallic masses are influenced by "traces."

W. C. ROBERTS-AUSTEN.

SEDGWICK AND MURCHISON: CAMBRIAN AND SILURIAN.[1]

ERRONEOUS impressions have long existed among American geologists with regard to the relations to one another, and to Cambrian and Silurian geology, of Sedgwick and Murchison. The Taconic controversy in this country served, most unreasonably, to intensify feelings respecting these British fellow-workers in geology, and draw out harsh judgments. Now that right views on the American question have been reached, it is desirable that the facts connected with the British question should be understood and justly appreciated.

Sedgwick and Murchison were literally fellow-workers in their earlier investigations. Prof. John Phillips, in a biographical sketch of Sedgwick (NATURE, vol. vii. p. 257), whose intimate friendship through fifty years "he had the happiness of enjoying," speaks thus, in 1873, of their joint work:—

"Communications on Arran and the north of Scotland, including Caithness (1828) and the Moray Firth; others on Gosau and the Eastern Alps (1829-31); and still later, in 1837, a great memoir on the Palæozoic strata of Devonshire and Cornwall, and another on the coeval rocks of Belgium and North Germany, show the labours of these intimate friends in the happiest way—the broad generalizations in which the Cambridge professor delighted, well supported by the indefatigable industry of his zealous companion."

Prof. Phillips then speaks of the Cambrian and Silurian labours "of two of the most truly attached and mutually helpful cultivators of geological science in England."

Of these Cambrian and Silurian labours it is my purpose to give here a brief history derived from the papers they published. They were begun in 1831, without concert—Sedgwick in Wales, Murchison along the Welsh and English borders.

In September of 1831, the summer's excursions ended, Murchison made his first report at the first meeting of the British Association. It was illustrated by a coloured geological map representing the distribution of the "Transition Rocks," the outlying Old Red Sandstone, and the Carboniferous limestone (Murchison, Report of the British Association, i. 91, 1831).

These "Transition Rocks" (of Werner's system), upturned semi-crystalline schists, slates, and other rocks, passing down into uncrystalline, and regarded as mostly non-fossiliferous, the "*agnotozoic*" of the first quarter of the century, were the subject of Sedgwick's and Murchison's investigations—the older of the series, as it turned out, being included in Sedgwick's part.[2] They were early resolved into their constituent formations by Murchison, and later as completely by Sedgwick in his more difficult field.[1]

Already in March and April of 1833, Murchison showed, by his communications to the Geological Society of London, that he had made great progress; for the report says:[2]—He "separated into distinct formations, by the evidence of fossils and the order of superposition, the upper portion of those vast sedimentary accumulations which had hitherto been known only under the common terms of Transition Rocks and Grauwacke." And these "distinct formations" were: (1) the Upper Ludlow rocks; (2) the Wenlock limestone; (3) the Lower Ludlow rocks; (4) Shelley sandstones, "which in Shropshire occupy separate ridges on the south-eastern flanks of the Wrekin and the Caer Caradoc"; (5) the Black Trilobite flagstone whose "prevailing Trilobite is the large *Asaphus Buchii*, which with the associated species," he observed, "is never seen in any of the overlying groups"; and below these, (6) Red Conglomerate sandstone and slaty schist several thousand feet in thickness.

By the following January, 1834, Murchison was ready with a further report,[3] in which he described the "four fossiliferous formations" in detail, and displayed, on a folded table arranged in columns, their stratigraphical order, thickness, subdivisions, localities, and "characteristic organic remains." The subdivisions of the rock-series in the memoir are as follows, commencing above: (I.) Ludlow rocks, 2000 feet; (II.) Wenlock and Dudley rocks, 1800 feet; (III.) Horderley and May Hill rocks (afterward named Caradoc), 2500 feet; (IV.) Builth and Llandeilo flags, characterized by *Asaphus Buchii*, 1200 feet; and, below these, (V.) the Longmynd and Gwastaden rocks, many thousand feet thick, set down as unfossiliferous.

Thus far had Murchison advanced in the development of the Silurian system by the end of his third year. Upper and Lower Silurian strata were comprised in it, but these subdivisions were not yet announced.

During the interval from 1831 to 1834, Sedgwick presented to the British Association in 1832 a verbal communication on the geology of Caernarvonshire, and another brief report of progress in 1833. A few lines for each are all that was published. The difficulties of the region were a reason for slow and cautious work.

In 1834, as first stated in the Journal of the Geological Society for the year 1852, the two geologists took an excursion together over their respective fields. Sedgwick says (Quarterly Journal of the Geological Society, viii. 152, 1852): "I then studied for the first time the Silurian types under the guidance of my fellow-labourer and friend; and I was so struck by the clearness of the natural sections and the perfection of his workmanship, that I received, I might say, with implicit faith everything which he then taught me." And further, "the whole 'Silurian system' was by its author placed *above* the great undulating slate-rocks of South Wales." The geologists next went together over Sedgwick's region, and

[1] Printed from advance sheets kindly supplied by Prof. Dana. The article appears in the current number of the *American Journal of Science*.

[2] Murchison says, in the introductory chapter of his "Silurian System," p. 4, "No one [in Great Britain, before his investigations began] was aware of the existence below the Old Red Sandstone of a regular series of deposits containing peculiar organic remains." "From the days of De Saussure and Werner, to our own, the belief was impressed on the minds of geologists that the great dislocations to which these ancient rocks had been subjected had entirely dissevered them from the fossiliferous strata with which we were acquainted."

[1] The term "Transition" early appeared in American geological writings. Sixty to seventy-five years ago it was applied by Maclure, Dewey, and Eaton, to the rocks of the Taconic region and their continuation; for these were upturned, apparently unfossiliferous, semi-crystalline to uncrystalline, and extended eastward to a region of gneisses. The study of the rocks was commenced; but in 1842, before careful work for the resolution of them had been done—like that in which Murchison and Sedgwick were engaged—they were, unfortunately, put, as a whole, into a "Taconic system" of assumed pre-Potsdam age; at the same time "Transition" was shoved west of the Hudson, over rocks that were horizontal, and already resolved. Owing to this forestalling of investigation, and partly also to inherent difficulties, the right determination of the several formations comprised in this Taconic or "Transition" region was very long delayed.

[2] Murchison, Proceedings of the Geol. Soc. London, i. 470, 474, 1833, in a paper on the sedimentary deposits of Shropshire and Herefordshire.

[3] Murchison, Proc. Geol. Soc., ii. 13, 1834. The subject was also before the British Association: Report for 1834, p. 652.

the sections from the top of the Berwyns to Bala. Murchison concluded, after his brief examination, and told Sedgwick, that the Bala group could not be brought within the limits of his system. He says: "I believed it it to plunge under the true Llandeilo flags with *Asaphus Buchii*, which I had recognized on the east flank of that chain." "Not seeing, on that hurried visit, any of the characteristic Llandeilo Trilobites in the Bala limestone, I did not then identify that rock with the Llandeilo flags, as has since been done by the Government surveyors" (Q. J. G. Soc., viii. 175).

In 1835, the terms "Silurian" and "Cambrian" first appear in geological literature. Murchison named his system the "Silurian" in an article in the *Philosophical Magazine* for July of that year, and at the same time defined the two grand subdivisions of the system: (I.) the Upper Silurian, or the Ludlow and Wenlock beds; and (II.) the Lower Silurian, or the Caradoc and Llandeilo beds (*Phil. Mag.*, vii. 46, July 1835).

During the next month, August, the fourth meeting of the British Association was held at Edinburgh, and in the Report of the meeting (Brit. Assoc., v., August 1835), the two terms, "Silurian" and "Cambrian," are united in the title of a communication "by Prof. Sedgwick and R. I. Murchison," the title reading, "On the Silurian and Cambrian Systems, exhibiting the order in which the older sedimentry strata succeed each other in England and Wales." Murchison, after explaining his several subdivisions, said that "in South Wales" he had "traced many distinct passages from the lowest member of the "Silurian system" into the underlying slaty rocks now named by Prof. Sedgwick the Upper Cambrian." Sedgwick spoke of his "Upper Cambrian group" as including the greater part of the chain of the Berwyns, where, he said, "it is connected with the Llandeilo flags of the Silurian and expanded through a considerable part of South Wales"; the "Middle Cambrian group" as "comprising the higher mountains of Caernarvonshire and Merionethshire"; the "Lower Cambrian group" as occupying the south-west coast of Caernarvonshire, and consisting of chlorite and mica schists, and some serpentine and granular limestone; and finally, he "explained the mode of connecting Mr. Murchison's researches with his own so as to form one general system."

Thus, in four years Murchison had developed the true system in the rocks he was studying; and Sedgwick likewise had reached what appeared to be a natural grouping of the rocks of his complicated area. Further, in a united paper, or papers presented together, they had announced the names Silurian and Cambrian, and expressed their mutual satisfaction with the defined limits. Neither was yet aware of the unfortunate mischief-involving fact that the two were overlapping series.

It is well here to note that *the term "Cambrian" antedates "Taconic" of Emmons by seven years;* and also that Emmons did not know—any more than Sedgwick with regard to the Cambrian—that his system of rocks was in part Lower Silurian, and of Llandeilo and Caradoc age.

In May of 1838, nearly three years later, Sedgwick presented his first detailed memoir on North Wales and the Cambrian rocks to the Geological Society.[1] Without referring to the characteristic fossils, he divides the rocks below the Old Red Sandstone, beginning below, into (I.) the Primary Stratified Groups, including gneiss, mica-schist, and the Skiddaw slates, giving the provisional name of "Protozoic" for the series should it prove to be fossiliferous, and (II.) the Palæozoic Series; the latter including (1) the Lower Cambrian (answering to Middle Cambrian of the paper of 1835), (2) the Upper Cambrian, and (3) the "Silurian," or the series so called by Murchison. Without a report on the fossils, no comparison was possible at that time with Murchison's Silurian series. Yet Sedgwick goes so far as to say that the "Upper Cambrian," which "commences with the fossiliferous beds of Bala, and includes all the higher portions of the Berwyns and all the slate-rocks of South Wales which are below the Silurian System," "appears to pass by insensible gradation into the lower division of the Upper System (the Caradoc Sandstone);" and that "many of the fossils are identical in species with those of the Silurian System."[1] Respecting the Silurian System he refers to the abstracts of Mr. Murchison's papers and "his forthcoming work."

The Protozoic division included the "Highlands of Scotland, the crystalline schists of Anglesea, and the south-west coast of Caernarvonshire." It is added: "The series is generally without organic remains; but should organic remains appear unequivocally in any part of this class they may be described as the Protozoic System."

In the later part of the same year, 1838, Murchison's "Silurian System" was published[2]—a quarto volume of 800 pages, with twenty-seven plates of fossils, and nine folded plates of stratigraphical sections, besides many plates in the text—the outcome of his eight years of work. Five hundred pages are devoted to the Silurian System.

The dedication is as follows:—

"To you, my dear Sedgwick, a large portion of whose life has been devoted to the arduous study of the older British rocks, I dedicate this work.

"Having explored with you many a tract, both at home and abroad, I beg you to accept this offering as a memorial of friendship, and of the high sense I entertain of the value of your labours."

Through Murchison's investigations here recorded, as he remarks in his introduction with reasonable satisfaction, "a complete succession of fossiliferous strata is interpolated between the Old Red Sandstone and the oldest slaty rocks." He observes as follows of Sedgwick:—"In speaking of the labours of my friend, I may truly say, that he not only shed an entirely new light on the crystalline arrangement or slaty cleavage of the North Welsh mountains, but also overcame what to most men would have proved insurmountable difficulties in determining the order and relations of these very ancient strata amid scenes of vast dislocation. He further made several traverses across the region in which I was employed; and, sanctioning the arrangement I had adopted, he not only gave me confidence in its accuracy, but enhanced the value of my work by enabling me to unite it with his own; and thus have our joint exertions led to a general view of the sequence of the older fossiliferous deposits." In accordance with these statements many of the descriptions and the very numerous sections represent the Cambrian rocks lying beneath the Silurian—though necessarily with incorrect details, since neither Murchison nor Sedgwick had then any appreciation of the actual connection between the so-called Cambrian and Silurian.

The Silurian System, as here set forth, is essentially that of Murchison's earlier paper of 1835; and through the work, as each region is taken up, the rocks of the Upper and Lower divisions, and their several subdivisions, are described in order, with a mention of the characteristic fossils. As to the relations of the two grand divisions, he says that, "although two or three species of

[1] An abstract appeared in the Proc. Geol. Soc., ii. 675, 1838. A continuation of the paper appeared in 1841, *ibid.*, iii. 541. See also Q. J. Geol. Soc., viii., 1852.

[1] Of these fossils, he had mentioned "*Bellerophon bilobatus*, *Producta sericea*, and several species of *Orthis*" as occurring in the Bala limestone, "all of which are common to the Lower Silurian System," in a syllabus of his Cambridge lectures, published in 1837.

[2] Murchison's "Silurian System" bears on its title-page the date 1839. He states in the Q. J. Geol. Soc., viii. 177, 1852, that the work was really issued in 1838. The fossil fishes of the volume were described by Agassiz, the Trilobites by Murchison, and the rest of the species by Sowerby.

shells of the Upper Silurian rocks may be detected in the Lower Silurian, *the mass of organic remains in each group is very distinct*." Later he makes the number of identical species larger; but even the newest results do not increase it so far as to set aside Murchison's general statement of 1838.

Sedgwick, with all the light which the fossils of the "Silurian System" were calculated to throw on his Upper Cambrian series, found in the work no encroachments on his field or on his views. They were still side by side in their labours among the hitherto unfathomed British Palæozoic rocks.

In 1840 and 1841, Murchison was in Russia with M. de Verneuil and Count Keyserling, and also in Scandinavia and Bohemia, seeking to extend his knowledge of the older fossiliferous rocks and verify his conclusions; and in 1845 the great work on the "Geology of Russia and the Urals" came out, with a further display of Upper and Lower Silurian life. In his Presidential addresses of 1842 and 1843, reviewing the facts in the light of his new observations, he went so far as to say that the Lower Silurian rocks were the oldest of fossiliferous rocks, and that the fossiliferous series of North Wales seemed to exhibit no vestiges of animal life different from those of the Lower Silurian group.

Still Sedgwick made no protest. He states definitely on this point in his paper of 1852 (Q. J. Geol. Soc., viii. 153, 1852), that from 1834, the time of the excursion with Murchison, until 1842, he had accepted Murchison's conclusions, including the reference of the Meifod beds to the Caradoc or Silurian, without questioning; but that from that time, 1842, he began to lose his confidence in the stability of the *base-line* of the "Silurian System." He adds that in 1842, Mr. Salter, the palæontologist, informed him that the Meifod beds were on the same horizon nearly with the Bala beds; and he accepted this conclusion to its full extent, using the words, "if the Meifod beds were Caradoc, the Bala beds must also be Caradoc or very nearly on its parallel." Thus the inference of Murchison was adopted, and discrepancy between them deferred. And on the following page he acknowledges that all his papers of which there is any notice in the Proceedings or Journal of the Geological Society between 1843 and 1846 admit this view as to the Bala beds and certain consequences of it—"mistakes," as he pronounced them six years later, in 1852 (Q.J. Geol. Soc., viii. 154, 1852).

In 1843, Sedgwick read before the Geological Society in June, a paper entitled "An Outline of the Geological Structure of North Wales," which was published in abstract in the Proceedings (iv. 251); and in November of the same year, one "On the Older Palæozoic (Protozoic) Rocks of North Wales" (from observations by himself in company with Mr. Salter), which appeared, with a map, in the Journal of the Geological Society (i. 1). The abstract in the Proceedings was prepared by Mr. Warburton, the President of the Geological Society, and the paper of the following November makes no allusion to this fact, or any objection to the abstract.

A remarkable feature of the November paper is that it nowhere contains the term "Upper Cambrian" or even "Cambrian," although the rocks are Sedgwick's Upper Cambrian, together with Murchison's Upper Silurian.

A second fact of historical interest is the use of the term "Protozoic," not in the sense in which it was introduced by him in 1838, but in that in which introduced in 1838 by Murchison, on p. 11 of his "Silurian System," where he says:—

"But the Silurian, though ancient, are not, as before stated, *the most* ancient fossiliferous strata. They are, in truth, but the upper portion of a succession of early deposits which it may hereafter be found necessary to describe under one comprehensive name. For this purpose I venture to suggest the term 'Protozoic Rocks' thereby to imply the first or lowest formations in which animals or vegetables appear."

These facts are in accordance with Sedgwick's acknowledgment, already mentioned.

The map accompanying the paper as originally prepared, had colours corresponding to five sets of areas, those of the "Carboniferous Limestone," "Upper Silurian," "Protozoic Rocks," "Mica and Chlorite Slate," "Porphyritic Rocks"; and here again Cambrian, Upper or Lower, does not appear, the term Protozoic being substituted. The map, as it stands in the Journal of the Geological Society, has, in place of simply "Protozoic," the words "Lower Silurian (Protozoic)." Sedgwick complains, in his paper of 1852, pp. 154, 155, of this change from his manuscript, and attributes it to Mr. Warburton, saying that "the map with its explanations of the colours plainly shows that Mr. Warburton did not comprehend the very drift and object of my paper." "I gave one colour to this whole Protozoic series only because I did not know how to draw a clear continuous line on the map between the Upper Protozoic (or Lower Silurian) rocks and the Lower Protozoic (or Lower Cambrian) rocks." "Nor did I ever dream of an incorporation of all the Lower Cambrian rocks in the system of Siluria." Sedgwick also says on the same point: "I used the word 'Protozoic' to prevent wrangling about the words Cambrian and Silurian." But this is language he had no disposition to use in 1843, as the paper of 1843 shows.

Page 155 has a footnote. In it the aspect of the facts is greatly changed. He takes back his charges, saying, "I suspect that, in the explanation of the blank portion of the rough map exhibited in illustration of my paper I had written 'Lower Silurian and Protozoic,' and that Mr. Warburton, erroneously conceiving the two terms identical, changed the words into Lower Silurian (Protozoic)." "I do not by any means accuse Mr. Warburton of any *intentional* injustice—quite the contrary; for I know that he gave his best efforts to the abstract. But he had undertaken a task for which he was not prepared, inasmuch as he had never well studied any series of rocks like those described in my papers." Sedgwick here uses Protozoic in the Sedgwick sense, not, as above, in the Murchison sense. Sedgwick again, in 1854, speaks of "the tampering with the names of my reduced map." But these explanations of his should take the harshness out of the sentence, as it was in 1843 to 1846 out of all his words.

The paper has further interest in its long lists of fossils in two tables: (I.) "Fossils of the Older Palæozoic (Protozoic) Rocks in North Wales, by J. W. Salter and J. de C. Sowerby," showing their distribution; and (2) "Fossils of the Denbigh Flagstone and Sandstone Series."

Thus, until 1846, no serious divergence of views had been noted by Sedgwick. This is manifested in his paper on the "Slate-rocks of Cumberland," read before the Geological Society on January 7 and 21, 1846 (Q. J. Geol. Soc., ii. 106, 122, 1846), which says, on the last page but one: "Taking the whole view of the case, therefore, as I know it, I would divide the older Palæozoic rocks of our island into three great groups—(3) the upper group, *exclusively Upper Silurian;* (2) the middle group, or *Lower Silurian*, including Llandeilo, Caradoc, and perhaps Wenlock; (1) the first group, or *Cambrian*;" differing in this arrangement from Murchison only in the suggestion about the Wenlock. The italics are his own. He adds:—

"This arrangement does no violence to the Silurian system of Sir R. Murchison, but takes it up in its true place; and I think it enables us to classify the old rocks in such a way as to satisfy the conditions both of the fossil and physical as well as mineralogical development."

But before the year 1846 closed, not only the overlapping of their work was recognized, but also the consequences ahead, and divergence of opinion began.

In December a paper was presented by Sedgwick to the Geological Society, on "The Fossiliferous Slates of North Wales, Cumberland, Westmoreland, and Lancashire" (Q. J. Geol. Soc., iii. 133, December 1846), which contains a protest against the downward extension of the Silurian so as to include the Cambrian. It is excellent in spirit and fair in argument. Many new facts are given respecting sections of the rocks in South Wales and North Wales, in some of which occur the Lingula flags, and characteristic fossils are mentioned. In describing some South Wales sections, Sedgwick uses the term "Cambro-Silurian" to include, beginning below: (1) "conglomerates and slates, (2) Lower Llandeilo flags, (3) slates and grits (Caradoc sandstone of Noeth Grug, &c.), (4) Upper Llandeilo flag, passing by insensible gradations into Wenlock shale." The Cambrian series is made to include: (1) the Festiniog or Tremadoc group; (2) roofing-slates, &c., the "Snowdonian group," fossiliferous in Snowdon, &c.; (3) the Bala group; and then (4) "the Cambro-Silurian group," comprising "the lower fossiliferous rocks east of the Berwyns between the Dee and the Severn—the Caradoc sandstone of the typical country of Siluria—and the Llandeilo flags of South Wales, along with certain associated slates, flags, and grits." The extension of the term Silurian down to the Lingula flags, or beyond, is opposed, because the beds below the Llandeilo are not part of the Silurian system; the term Silurian [derived from the Silures of South-East Wales and the adjoining part of England] is not geographically applicable to the Cambrian rocks; and because the only beds in North Wales closely comparable "with the Llandeilo flags are at the top of the whole Cambrian series." This last reason later lost its value when it was proved, as Sedgwick recognized years afterward, that Murchison's Llandeilo flags were really older than Sedgwick's Bala rocks.

Sedgwick's paper was followed, on January 6, with one by Murchison (Q. J. Geol. Soc., iii. 165, January 1847) objecting to this absorption of the Lower Silurian, and reiterating his remark of 1843 that the fossiliferous Cambrian beds were Lower Silurian in their fossils, and arguing, thence, for the absorption of the Cambrian, to this extent, by the Silurian. Having, eight years before, in his great work on the "Silurian System," described the Lower Silurian groups with so much detail, and with limits well defined by sections and by long lists of fossils, over a hundred species in all, many of them figured as well as described, and having thus added a long systematized range of rocks to the lower part of the Palæozoic series, he was naturally unwilling to give up the name of Lower Silurian for that of Upper Cambrian or Cambro-Silurian. Moreover, the term "Silurian," with the two subdivisions of the system, the Upper and Lower, had gone the world over, having been accepted by geologists of all lands as soon as proposed, become affixed to the rocks to which they belonged, and put into use in memoirs, maps, and geological treatises.

In 1852, the controversy, begun by encroachments not intended on either part, reached its height. Sedgwick's earnest presentation of the case (Q. J. Geol. Soc., viii. 152), and appeal before the Geological Society in February of that year—making the latter part of a memoir by him on the "Classification and Nomenclature of the Lower Palæozoic Rocks of England and Wales"—argues, like that of 1846, for the extension of the Cambrian from below upward to include the Bala beds, and thereby also the Llandeilo flags and Caradoc sandstone, although he says, "my friend has published a magnificent series of fossils from the Llandeilo flagstone." Sedgwick also expresses dissatisfaction with Mr. Warburton's abstract of his paper of June 1843, and with the change made in his map of November 1843, but, as shown above, he has no blame for Murchison and little for Mr. Warburton. He also points out some errors in the stratigraphical sections of the "Silurian System"—since the publication of which fourteen years had passed. He closes with the words (p. 168):—

"I affirm that the name 'Silurian,' given to the great Cambrian series below the Caradoc group, is historically unjust. I claim this great series as my own by the undoubted right of conquest; and I continue to give it the name 'Cambrian' on the right of priority, and, moreover, as the only name yet given to the series that does not involve a geographical contradiction. The name 'Silurian' not merely involves a principle of nomenclature that is at war with the rational logic through which every other Palæozoic group of England has gained a permanent name, but it also confers the presumed honour of a conquest over the older rocks of Wales on the part of one who barely touched their outskirts, and mistook his way as soon as he had passed within them.

"I claim the right of naming the Cambrian rocks because I flinched not from their difficulties, made out their general structure, collected their fossils, and first comprehended their respective relations to the groups above them and below them, in the great and complicated Palæozoic sections of North Wales. Nor is this all,—I claim the name Cambrian, in the sense in which I have used it, as a means of establishing a congruous nomenclature between the Welsh and the Cumbrian mountains, and bringing their respective groups into a rigid geological comparison; for the system on which I have for many years been labouring is not partial and one-sided, but general and for all England."

Sedgwick does not seem to have recognized the fact that Murchison had the same right to extend the Silurian system to the base of the Llandeilo beds, whatever its horizon, that he had to continue the Cambrian to the top of the Bala beds.[1]

Murchison's reply was made at the meeting of the Geological Society in June (Q. J. Geol. Soc., viii. 173, 1852). He remarked, with regard to Sedgwick's allusion to the excursion of 1834, that, "if I lost my way in going downward into the region of my friend, it was under his own guidance; I am answerable only for Silurian and Cambrian rocks described and drawn as such within my own region."

In his closing remarks Murchison says:—

"I am now well pleased to find that, with the exception of my old friend, all my geological contemporaries in my own country adhere to the unity of the Silurian System, and thus sustain its general adoption.

"No one more regrets than myself that Cambrian should not have proved, what it was formerly supposed to be, more ancient than the Silurian region, and thus have afforded distinct fossils and a separate system; but as things which are synonymous cannot have separate names, there is no doubt that, according to the laws of scientific literature, the term 'Silurian' must be sustained as applied to all the *fossiliferous* rocks of North Wales.

"Lastly, let me say to those who do not understand the nature of the social union of the members of the Geological Society, that the controversy which has prevailed between the eloquent Woodwardian Professor and myself has not for a moment interrupted our strong personal friendship. I am indeed confident we shall slide down the hill of life with the same mutual regard which animated us formerly when climbing together many a mountain both at home and abroad."

Murchison was right in saying that all British geologists were then with him, even in the extension of the name Silurian to the lower fossiliferous Cambrian rocks; and this was a chief source of irritation to Sedgwick. It was also, with scarcely an exception, true of geologists else-

[1] One important fact is pointed out in this paper in a letter from M'Coy, on p. 143—that the May Hill group, which Murchison had referred to the Caradoc series, really belonged by its fossils to the Upper Silurian. This point was the subject of a paper by Sedgwick in the next volume (vol. ix.) of the Journal of the Geological Society.

where. This state of opinion was partly a consequence of Murchison's early and wonderfully full description of the Silurian rocks and their fossils, which made his work a key to the Lower Palæozoic of all lands. Sedgwick's Cambrian researches and the palæontology of the region were not published in full before the years 1852-55, when appeared his "Synopsis of the Classification of the British Palæozoic Rocks," along with M'Coy's "Descriptions of British Palæozoic Fossils."

But this general acceptance was further due to the fact that the discovered fossils of the Cambrian, from the Lingula flags downward, or the "Primordial," were few, and differed not more from Silurian forms than the Silurian differed among themselves; and also, because the beds were continuous with the Silurian, without a break. Geologists under the weight of the evidence, American as well as European, naturally gravitated in the Murchisonian direction, while applauding the work of Sedgwick.

In 1853, Mr. Salter showed, by a study of the fossils (Q. J. Geol. Soc., x. 62), that the Bala beds from Bala in Merioneth, the original Bala, were included within the period of the Caradoc. Sedgwick subsequently (in the preface to the Catalogue of the Woodwardian Museum by J. W. Salter) divided his Upper Cambrian into (1) the Lower Bala, to include the Llandeilo flags (Upper Llandeilo of the Geological Survey, the Arenig being the Lower); (2) the Middle Bala, corresponding to the Caradoc sandstone, the Bala rocks, and the Coniston limestone (Geological Survey); and the Upper Bala or the Caradoc shales, Hirnant limestone, and the Lower Llandovery (cited from Etheridge, in Phillips's "Geology," ii. 77, 1885).

In 1854, the Cambrian system not having secured the place claimed for it, Sedgwick brought the subject again before the Geological Society. Besides urging his former arguments, he condemned Murchison's work so far as to imply that none of his sections "give a true notion of the geological place of the groups of Caer Caradoc and Llandeilo"; and to speak of the Llandeilo beds, in a note, as "a remarkable fossiliferous group (about the age of the Bala limestone) of which the geological place was entirely mistaken in the published sections of the Silurian System." There were errors in the sections, and that with regard to the May Hill group was a prominent one; but this was sweeping depreciation without new argument; and, in consequence of it, part of the paper was refused publication by the Geological Society.

The paper appeared in the *Philosophical Magazine* for 1854 (fourth series, vol. viii. pp. 301, 359, 481). It contains no bitter word, or personal remark against Murchison. Sedgwick was profoundly disappointed on finding, when closing up his long labours, that the Cambrian system had no place in the geology of the day. He did not see this to be the logical consequence of the facts so far as then understood. It was to him the disparagement and rejection of his faithful work; and this deeply moved him, even to estrangement from the author of the successful Silurian system.

Conclusion.

The ground about which there was reasonably a disputed claim was that of the Bala of Sedgwick's region and the Llandeilo and Caradoc of Murchison's. Respecting this common field, long priority in the describing and defining of the Llandeilo and Caradoc beds, both geologically and palæontologically, leaves no question as to Murchison's title. Below this level lie the rocks studied chiefly by Sedgwick; and if a dividing horizon of sufficient geological value had been found to exist, it should have been made the limit between a Cambrian and a Silurian system.

The claim of a worker to affix a name to a series of rocks first studied and defined by him cannot be disputed. But science may accept, or not, according as the name is, or is not, needed. In the progress of geology, the time finally was reached, when the name Cambrian was believed to be a necessity, and "Cambrian" and "Silurian" derived thence a right to follow one another in the geological record.

"To follow one another;" that is, directly, without a suppression of "Silurian" from the name of the lower subdivision by intruding the term "Ordovician," or any other term. For this is virtually appropriating what is claimed (though not so intended), and does marked injustice to one of the greatest of British geologists. Moreover, such an intruded term commemorates, with harsh emphasis, misjudgments and their consequences, which are better forgotten. Rather let the two names, standing together as in 1835, recall the fifteen years of friendly labours in Cambria and Siluria and the other earlier years of united research. JAMES D. DANA.

THE WEATHER IN JANUARY.

THE month of January, which is generally the coldest month of the year, was so exceptionally warm this year, and in other ways the whole period was so unusual, that a few of the leading features in connection with the weather may not be without interest. The month opened with a short spell of frost, but, after the first few days, mild weather set in, and continued until the close of the month.

The stations used by the Meteorological Office in the compilation of the Daily Weather Report scarcely represent sufficiently the weather at inland stations, but yet they will give an approximate idea of the prevailing conditions. These reports show that the warmest weather was experienced in the south-western parts of the Kingdom, the stations in the north-east of Scotland being about 5° colder than in the south-west of England. On the east coast the mean temperatures of Wick, Aberdeen, Spurn Head, and Yarmouth were each about 41°·0.

The following table gives the mean temperature results for a number of stations in all parts of the British Islands:—

Station.	Mean of max. and min.	Difference from average 15 years, 1871-1885.	Mean maximum.	Difference from average 15 years, 1871-1885.	Mean minimum.	Difference from average 15 years, 1871-1885.	Number of days with 50° and above.	Number of nights with 32° and below.
Wick	40·5	+2·8	45·2	+3·0	35·7	+2·7	4	8
Nairn	41·6	+4·3	47·1	+5·2	36·1	+3·4	13	4
Aberdeen	41·1	+3·2	45·6	+3·2	36·5	+3·2	7	4
Leith	42·2	+3·0	48·2	+3·6	36·2	+2·5	15	9
Shields	42·3	+3·4	47·8	+4·7	36·8	+2·1	14	5
York	41·8	+3·6	47·9	+4·7	35·6	+2·5	15	8
Loughborough	42·2	+4·0	48·4	+4·9	36·0	+3·1	17	6
Ardrossan	43·6	+3·2	47·3	+2·9	39·8	+3·4	6	3
Donaghadee	42·6	+2·2	47·7	+3·3	37·5	+1·2	15	2
Holyhead	44·7	+2·2	48·7	+2·8	40·7	+1·7	18	0
Liverpool	43·2	+3·4	48·5	+4·6	37·8	+2·2	16	4
Parsonstown	42·2	+1·9	48·8	+2·8	35·5	+0·9	16	7
Valencia	45·6	+0·4	51·1	+1·3	40·0	−0·5	21	3
Roche's Point	45·7	+1·9	50·2	+2·3	41·2	+1·5	23	1
Pembroke	46·0	+3·1	49·2	+3·4	42·8	+2·9	17	0
Scilly	48·3	+2·1	51·5	+2·4	45·0	+1·7	25	0
Jersey	46·6	+4·2	50·5	+4·5	42·6	+3·9	24	1
Hurst Castle	45·4	+4·2	49·8	+4·5	40·9	+3·9	23	2
London	43·7	+4·1	49·5	+4·7	37·8	+3·4	20	5
Oxford	42·5	+3·4	48·1	+4·3	36·8	+2·4	15	4
Cambridge...	41·9	+3·6	48·9	+5·0	34·9	+2·3	19	10
Yarmouth	40·8	+2·6	45·6	+3·7	36·0	+1·5	6	7

From this it is seen that the excess of temperature was least at the extreme western stations, the mean at Valencia only exceeding the average for 15 years by 0°·4, whilst the night temperature was even below the average. In nearly every case it is seen that the excess of the day temperatures over the average was larger than that of the night temperatures. A feature of especial interest in the table is the large number of days on which the temperature reached 50° or above.

It is interesting to notice the very great difference between the temperature in January this year, in comparison with that which occurred in January 1881, when the weather was exceptionally cold. At Loughborough, the mean temperature this year exceeded that in 1881 by 17°, which is 4° in excess of the difference between the average temperature for January and May; there were also several stations in nearly all parts of the Kingdom with an excess of 12° and 13°.

At Greenwich Observatory the mean temperature obtained from the mean of the maximum and minimum readings was 43°·4; and with the exception of 43°·5 in 1884 and 43°·6 in 1846, this has not been exceeded in January during the last half-century. The mean of the highest day temperatures was 48°·5, which is higher than any January during the last fifty years, and the only other instances of 48°, or above, were 48°·1 in 1877 and 1851, and 48°·0 in 1846. There were six years with the mean maximum between 47° and 48°, but only eighteen in all above 45°, whilst in January 1879 the mean of the maxima was only 35°·1, or 13°·4 colder than this year, and in 1881 it was only 36°·2. There have been three Januaries during the last half-century with a higher mean night temperature, but in no year was the excess more than 1.° In January this year the mean minimum was 38°·2, and in 1884 it was 39°·2. The Greenwich observations also show that there were in January 17 days with a temperature of 50° or above, whereas in the corresponding period during the last 50 years there has been no similarly high number of days with this temperature. It was reached 14 times in 1877, 1853, and 1846; 13 times in 1873 and 1849; 12 times in 1884; 11 times in 1874, 1869, 1852, and 1851; and in 28 Januaries 50° or above was only attained 5 times or less.

The warm weather was very intimately connected with the heavy wind storms which occurred throughout the month, the storm systems which so frequently arrived on our coasts from off the Atlantic being the natural carriers of warm moist air. Scarcely a day passed during the month without the arrival of some fresh disturbance from the westward, but with one or two exceptions the central areas of the storm systems skirted the western and northern coasts and did not pass directly over our islands. The disturbances, however, passed sufficiently near to us to cause winds of gale force, and there was scarcely a day throughout the month that a gale was not blowing in some part of the United Kingdom. In the North Atlantic the month was exceptionally stormy, and vessels trading between Europe and America experienced unusually heavy weather.

The month was also marked by the prevalence of influenza, and, in addition to this, a general unhealthiness pervaded all classes of the community. The death-rate, from all causes, in London, for the four weeks ending January 25, corresponded to an annual rate of 29·7 per 1000 of the total population, which is excessively high. The rates for the corresponding period in the last four years were 21·7 in 1889, 23·2 in 1888, 22·7 in 1887, and 22·6 in 1886.

CHAS. HARDING.

NOTES.

THE subject of the Bakerian Lecture, which, as we announced last week, is to be delivered by Prof. Schuster on March 20, will be "The Discharge of Electricity through Gases."

THE Academy of Sciences of Berlin has presented the following sums of money: £90 to Dr. Rohde, of Breslau, for a journey to Naples to continue his observations on the central nervous system of sharks and echinoderms at Prof. Dohrn's zoological station; £80 to Prof. Matthiessen, of Rostock, to further his researches on the eyes of whales at the stations of the North Sea fisheries; £25 to Prof. Dr. Winkler, of Breslau, for a journey to St. Petersburg to make researches on the Turkish, Samoyed, and Tungusian languages; £30 to Dr. Schellong, the New Guinea traveller, to publish the results of his anthropological studies.

IT is proposed that the following address shall be presented to Prof. Stuart on the occasion of his resignation of his Professorship at Cambridge:—"We, the undersigned resident members of the Senate, having learned from your letter to the Vice-Chancellor your intention of resigning your Professorship in the University, desire to express our sense of the great public service which you have rendered in connection with the University Extension movement. By yourself first delivering specimen courses of lectures, and afterwards strenuously advocating and ably organizing their wide-spread establishment, you did for the country at large, and for our own and other Universities, work which we regard with sincere respect and admiration. The degree in which Cambridge has, during the last twenty years, come into useful relations with sections of the community which were previously regarded as beyond the sphere of its influence is, we hold, largely attributable to your inspiring initiative, and to the wise principles of administration which, mainly under your guidance, the University laid down."

AMONG the lectures to be delivered at the Royal Institution of Great Britain after Easter we note the following:—On Tuesdays, April 15, 22, 29, three lectures on the place of Oxford University in English history, by the Hon. George C. Brodrick; on Tuesdays, May 27, June 3, 10, three lectures on the natural history of society, by Mr. Andrew Lang; on Thursdays, April 17, 24, May 1, three lectures on the heat of the moon and stars (the Tyndall Lectures), by Mr. C. V. Boys, F.R.S.; on Thursdays, May 8, 15, 22, 29, June 5, 12, six lectures on flame and explosives, by Prof. Dewar, F.R.S.; on Saturdays, April 19, 26, May 3, three lectures on colour and its chemical action, by Captain W. de W. Abney, F.R.S.

THE De Candolle Prize has been awarded to Prof. F. Buchenau, of Bremen, for his monograph of the Juncagineæ.

A CONGRESS for Viticulture will be held in Rome from the 23rd to the 27th of the present month. The principal object of the Congress will be the discussion of remedies for the *Peronospora viticola* and other diseases of the vine caused by vegetable parasites. There will be an International Exhibition of apparatus for the cure of these diseases, and numerous prizes will be awarded.

THE annual general meeting of the members of the German Botanical Society is to be held this year in Bremen late in September.

APPENDIX I. of the *Kew Bulletin*, just issued, contains a list of such hardy herbaceous annual and perennial plants and of such trees and shrubs as matured seeds under cultivation in the Royal Gardens, Kew, during the year 1889. It is explained that these seeds are available for exchange with Colonial, Indian, and Foreign Botanic Gardens, as well as with regular correspondents of Kew. The seeds are for the most part only available in moderate quantity, and are not sold to the general public.

THE Nachtigal Gesellschaft of Berlin, for German research in Africa, has just completed its second year of business. It was announced at the last general meeting that the list of members

had been doubled during the last year. The Society's library contains 200 books on Africa. Herr Schiller-Tietz was elected President of the Society in place of Councillor Engelke.

A CURIOUS phenomenon is reported from Batoum. On January 23, at 4 p.m., during a complete calm, the sea is said to have suddenly receded from the shore, leaving it bare to a depth of ten fathoms. The water of the port rushed out to sea, tearing many of the ships from their anchorage, and causing a great amount of damage. After a short time the sea assumed its usual level.

AN important addition to our knowledge of the meteorology of Central America has been made by the publication of Parts 1–4 of the *Boletin trimestral* of the National Meteorological Institute of San José, Costa Rica, for the year 1888, under the direction of Prof. E. Pittier. The Observatory is situated in latitude 9° 56′ N., longitude 84° 8′ W., and its importance may be judged from the fact that no other station of the first order possessing self-recording instruments is to be found between Mexico, in latitude 19° N., and Rio de Janeiro, in latitude 23° S. The bulletin contains observations made several times daily, and hourly observations of rainfall for five months, also a summary of the observations formerly made in Costa Rica. The older series of observations show that the mean yearly extremes of temperature at San José were 78°·8 and 56°·7, while the mean difference of the monthly means amounted only to about 4°. The daily period of rainfall is very marked. From sunrise to noon scarcely any rain falls, while between noon and 6h. p.m. about 75 per cent. of the whole amount falls. The mean duration of rain on a wet day is 2h. 9m. Only two months of anemometrical observations are given; these show that the maximum velocity at noon is twice as great as the mean velocity during the night. An interesting summary of the observations has been published by Dr. Hann in the *Meteorologische Zeitschrift* for February.

AT a recent meeting of the Paris Geographical Society an interesting lecture was delivered by Dr. Hamy, on the history of scientific missions in France under the old monarchy. He commenced practically with the reign of Francis I., and described many missions abroad, with purely scientific aims, which are now either forgotten, or the results of which have never been published. Thus, the apothecary to Henri IV. went all over the globe in search of the peculiar products of each country, especially medicinal and food plants; still earlier, another explorer went to Brazil to study dyeing woods; and, in the last century, Condamine, Dombey, Bougainville, and La Pérouse went on their well-known expeditions. The President, Comte de Bisemont, mentioned that there were still in the archives of the Ministry of Marine copies of the instructions given to travellers and navigators in past centuries, and that these were "positively models of their kind, which could not be followed too closely now." Prof. Bureau, of the Museum of Natural History in Paris, observed that a botanical collection made by Paul Lucas in the reign of Louis XIV. still existed in the Museum, and he referred especially to Tournefort, of the same period, whom he described as the scientific traveller of former times who perhaps most nearly approached moderns in his methods of observation. He was sent by the King on a botanical expedition to the Levant, with very precise instructions, amongst others, to collect and observe the plants mentioned by the ancients. He did not confine himself to this, but formed a complete herbarium, which is still preserved at the Museum, and is one of its treasures. He was accompanied by an artist named Aubriet, who brought back a large collection of coloured sketches, which forms an important part of the unrivalled collection in the library of the Museum.

A NEW and very simple method of measuring small elongations of a bar under any influence has been devised by Signor Cardani (*Cosmos*). To one end of the bar is attached a metallic wire stretched so as to give a determinate number of vibrations. When the bar expands, the wire becomes less tense, and gives fewer vibrations, and there is a simple relation between the number of vibrations and the elongation of the bar. The author cites a case in which a variation of one hundredth of a millimetre in a bar lessens the double vibrations from 99 to 96·5. Now, a practised ear will appreciate a difference of one vibration per cent.; hence it suffices to ascertain variations of length less than 0·01 millimetre. With other methods of measuring change of vibration, elongations of thousandths of a millimetre may be ascertained.

THE first careful determination of latitude in Tokio (according to the *Japan Weekly Mail*) was made in 1876 by Captain Kimotsuki, at that time Director of the Naval Observatory. In 1888, soon after the transfer of the Naval Observatory to the Imperial University, and its reorganization as the Astronomical Observatory of Tokio, the new Director, Prof. Terao, resolved upon a redetermination of the latitude. The work was entrusted to Mr. Watanabe, a skilled observer, and the result has been published as the first of the "Annales de l'Observatoire Astronomique de Tokio (Université Impériale du Japon, Collège des Sciences)." The determination was made in two distinct ways: first, by observations of the upper and lower transits of the Pole star across the meridian; second, by observations of the zenith distances of 38 different stars, arranged in couples according to Talcott's method. This latter method only was used by Captain Kimotsuki in this earlier determination. The earlier mean value for the latitude was 35° 39′ 17″·492; while the recently obtained mean values were 35° 39′ 15″·05 by the first method, and 35° 39′ 15″·41 by the second method. This discrepancy of fully 2″ is, in the circumstances, too large to be regarded as an accidental error, and must be due to some systematic error in either the earlier or the later determination. More weight will be attached to the new determination, since Mr. Watanabe had much superior instruments at his disposal.

THE stay of some 306 natives from various French colonies, &c., for about six months, in Paris last year, in connection with the Exhibition, was an interesting experiment in acclimatization. Owing to wise hygienic measures (such as vaccination, good water-supply, isolation of closets, and surveillance of food), these Annamites, Tonquinese, Senegalese, &c., seem to have escaped most of the common endemic disease. According to the *Semaine Médicale*, they had no typhoid fever, scarlatina, or measles, though these were in Paris at the time. Some 68 natives were attacked by mumps. The fatigues of a voyage and the change of climate led to a recurrence of intermittent fever, with grave symptoms, in twenty cases. It was thought at first to be typhoid fever of a severe type; but the rapid and durable efficacy of sulphate of quinine, given in doses of 2 to 3 grammes a day, proved the paludine nature of the disorder. It is noteworthy that most illnesses of this population, especially that just noticed, and those from cold, appeared during the first part of the time, when the weather was mild; while in the second period, with unfavourable atmospheric conditions, the illness diminished, whether owing to precautions in the matter of dress and food, or to more complete acclimatization. The negroes of Senegal and the Gaboon seem to have been the greatest sufferers, while the Indo-Chinese race acclimatized the best.

THE first *Bulletin* issued this year by the Académie Royale de Belgique contains a note by M. Van Beneden, on a Ziphius which was stranded in the Mediterranean, and a list of the prize subjects for 1891. The subjects dealt with are architecture, engraving, painting, and music. Four gold medals are given, having values 1000, two 800, and 600 francs respectively. The dissertations may be written in French, Flemish, or Latin, and must be sent before June 1, 1891, to M. J. Liagre, Secretary of the Academy.

A SHORT note on diethylene diamine, $C_2H_4\left\langle\begin{matrix}NH\\NH\end{matrix}\right\rangle C_2H_4$, is contributed to the new number of the *Berichte* of the German Chemical Society by Dr. J. Sieber, of Breslau. It was obtained by the action of ethylene dibromide, $C_2H_4Br_2$, upon ethylene diamine, $C_2H_4\left\langle\begin{matrix}NH_2\\NH_2\end{matrix}\right.$, a liquid boiling at 123° C. Upon treating the product of this reaction with caustic potash, an oily liquid separated, consisting of a mixture of bases. The separated liquid was next dehydrated as completely as possible, and then submitted to fractional distillation, when the portion boiling between 168°–175° was found to consist of diethylene diamine admixed with a little water. The affinity of the base for water is, in fact, so great that it was found impossible to remove the last traces of moisture. Diethylene diamine, however, readily forms salts which can be isolated in a state of purity, and the analyses of which prove the composition of the base itself. The hydrochloride, $C_2H_4\left\langle\begin{matrix}NH.HCl\\NH.HCl\end{matrix}\right\rangle C_2H_4$, crystallizes in beautiful white needles, very soluble in water, but insoluble in alcohol. The platinochloride, $C_4H_{10}N_2(HCl)_2PtCl_4$, forms fine yellow needle-shaped crystals, readily soluble in hot water, but difficultly soluble in boiling alcohol. A very beautiful salt is also formed with mercuric chloride, $C_4H_{10}N_2(HCl)_2HgCl_2$, consisting of star-like aggregates of acicular crystals, also soluble in hot water, but reprecipitated by the addition of alcohol.

DRS. WILL AND PINNOW communicate to the same journal their report upon the analysis of the remarkable meteorite of Carcote, Western Cordilleras, Chili. The great mass of this meteorite, 80 per cent., is found to consist of two silicates. One of them is readily decomposed by hydrochloric acid, and possesses the composition and optical characters of olivine, $(MgFe)_2SiO_4$. The other is unattacked by hydrochloric acid, and exhibits the chemical and crystallographical characters of a member of the diopside group. Interspersed among the silicates are smaller quantities of chrome ironstone, bronze-like sulphide of iron, probably troilite, and light steel-grey nickeliferous iron. The latter is not only found in minute particles, but also frequently in small plates which show the Widmannstadt figures in the form of an extremely fine rectangular network. Here and there are found silver-white crystals of rhabdite, one of the forms of nickeliferous iron. By far, however, the most interesting substance contained in the meteorite, is a form of crystalline elementary carbon, dull black in appearance and of extreme hardness, at least 9. It is, in fact, a variety of black diamond, and its presence in the meteorite affords considerable ground for speculation. Carbon is further present in the form of organic substances soluble in ether, and these substances carbonize upon heating, evolving the usual odour of burning organic matter. Hence this meteorite is an extremely interesting one, and forms another addition to the fast-accumulating list of those in which carbon forms a not insignificant ingredient.

OUR ASTRONOMICAL COLUMN.

OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at Greenwich at 10 p.m. on March 6 = 8h. 58m. 19s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G.C. 1713	—	—	8 45 49	+33 50
(2) 120 Schj.	6·5	Reddish-yellow.	9 4 6	+31 26
(3) α Hydræ	3	Yellow.	9 22 12	− 8 11
(4) α Cancri	4	Yellowish-white.	8 52 30	+12 17
(5) 124 Schj.	5·4	Reddish-yellow.	9 45 59	+22 36
(6) T Monocerotis ...	Var.	Yellow.	6 19 20	+ 7 8·7

Remarks.

(1) This bright oval nebula is now in a very convenient position for observation. I am not aware that the spectrum has been recorded. It is about 8′ long, and 3′ broad, and is thus described in the General Catalogue: "Very bright, very large, very much elongated 40°·9, gradually much brighter in the middle." The description is very suggestive of the Great Nebula in Andromeda, and if, as in that case, the spectrum at first appears continuous, closer scrutiny may reveal irregularities. The brighter parts, assuming that they exist, should be compared with the spectrum of carbon.

(2) According to the observations of D'Arrest, Secchi, and Vogel, this is a fine example of the stars of Group II. Dunér states that all the bands 1 to 10 inclusive are excessively wide and dark, and that the spectrum is totally discontinuous. The star, therefore, affords a good opportunity for further observations of the bright carbon flutings with the object of establishing the cometary character of the stars of this group. It may be remarked that the citron band of carbon need not enter into this comparison, as it will be masked by the dark fluting of manganese (band 4).

(3) A star of the solar type (Konkoly). The usual differential observations are required.

(4) A star of Group IV. (Vogel). The usual observations are required.

(5) This star has a "very fine" spectrum of the Group VI. type, notwithstanding its low altitude in our latitude (Dunér). The principal bands, 6, 9, and 10, are very dark, and the secondary bands, 4 and 5, are also well seen. Further observations, with special reference to line or other absorptions, are suggested.

(6) A maximum of this short-period variable will occur on March 8. Gore gives the period as 26·76 days, and the magnitudes at maximum and minimum as 6·2 and 7·6 respectively. There is still a little doubt with regard to its spectrum. In his spectroscopic catalogue, Vogel writes it II.*a*? III.*a*, giving the magnitude at the time of observation as 7·3. In all probability the spectrum is intermediate between Group II. and Group III., perhaps something like Aldebaran. A. FOWLER.

THE TOTAL SOLAR ECLIPSE OF DECEMBER 22, 1889.—M. A. De La Baume Pluvinel, who was located in Royal Island, about 30 miles north of Cayenne, during this eclipse, communicated his results to the Paris Academy on the 17th ult. (*Comptes rendus*, No. 7, 1890). An examination of the photographs of the corona which were obtained leads to the conclusions that—

(1) The corona presented the same general aspect as on January 1, 1889.

(2) The extension of the corona was small, being about 18′ at the solar equator, and about 6′ at the poles, and in this respect resembled the coronæ of 1867 and 1878, thus confirming the intimate relation that exists between the intensity of extra-solar phenomena and the frequency of sun-spots.

(3) The aspect of the luminous aigrettes which constitute the corona, and notably the curved form of the aigrettes in the neighbourhood of the poles, seem to prove the existence of streams of matter submitted to two forces—a force of projection normal to the solar sphere, and a centrifugal force developed by the sun's rotation.

COMETS AND ASTEROIDS DISCOVERED IN 1889.—

Comet a 1889.—Discovered on January 15, a little before dawn, by Mr. W. Brooks at Geneva, N.Y., U.S.A. The comet was moving rapidly from east to west, and was not afterwards observed.

Comet b 1889.—Discovered by Mr. Barnard, of the Lick Observatory, on March 31; it was then very feeble and difficult to see. After perihelion passage, the comet was observed at Ann Arbor on July 22, near the position assigned to it by M. E. Millosevich.

Comet c 1889.—Also discovered by Mr. Barnard, on June 23, as a faint nebulosity without condensation or tail. Not observed after August 6. Dr. Berberich determined the elements of this comet on the hypothesis of an elliptic orbit, and found that its period was 128 years.

Comet d 1889.—This comet, the most interesting of those observed last year, was discovered by Mr. Brooks, of Geneva, U.S., on July 6. It is periodic, the time of revolution being 7·04 years. On August 1, Mr. Barnard found that the principal comet was accompanied by four companions. Mr. Chandler

has found that in 1886 this comet must have approached near to Jupiter, and his investigations seem to show that it is identical with the lost comet of Lexell.

Comet e 1889.—Discovered by Mr. Davidson at Branscombe, Mackay (Queensland), on July 22, and visible to the naked eye at first as a star of the fourth magnitude. It moved rapidly towards the north, and at the same time diminished in brightness, remaining visible, however, up to November.

Comet f 1889.—Discovered by Mr. Lewis Swift at Rochester, U.S., on November 17. From observations extending over twenty days, Dr. Zelbr was led to conclude that the comet was periodic, the time of revolution being 6·91 years.

Comet g 1889.—Discovered by M. Borrelly at Marseille, on December 12. It was then feeble, but rapidly increased in brightness. Although the declination of this comet on discovery was + 48° 55′, it moved so quickly towards the south, that it was lost to our latitudes about January 10, 1890. The first observations fixed the perihelion passage at January 26, 1890.

Six asteroids were discovered in 1889, viz.:—

(282) Discovered by M. Charlois at Nice on January 28.
(283) ,, ,, ,, ,, February 8.
(284) ,, ,, ,, ,, May 29.
(285) ,, ,, ,, ,, August 3.
(286) ,, ,, M. J. Palisa at Vienna on August 3.
(287) ,, ,, Dr. Peters at Clinton, U.S., October 13.

MASS OF SATURN.—The Transactions of the Astronomical Observatory of Yale University, vol. i. part ii., contains some researches with the heliometer by Mr. Asaph Hall, for the determination of the orbit of Titan and the mass of Saturn.

From observations made at the oppositions of 1885–86, 1886–87, the mean value of the semi-major axis of Titan's orbit was determined as—

$$176''\cdot570 \pm 0''\cdot0243\,;$$

and the mass of Saturn—

$$\frac{1}{3500\cdot5 \pm 1\cdot44},$$

the sun being unity.

Struve showed that the value found by Bessel from Titan should be 3502·5, while the values found by Struve himself from Iapetus and Titan are respectively 3500·2 ± 0·82 and 3495·7 ± 1·43. Prof. Hall, with the great Washington refractor, found from Iapetus by means of differences of right ascension and declination, the mass 3481·2 ± 0·65, and by distances and position-angles 3481·4 ± 0·97; from Titan the values corresponding to the same methods are 3496·3 ± 1·84, and 3469·9 ± 1·49, but there seem to be grounds for questioning these results, so discordant with those found by Struve, and at Yale College.

THE OPENING OF THE FORTH BRIDGE.

MUCH interest was excited all over the country by the opening of the Forth Bridge on Tuesday. The ceremony was simple, and all the arrangements were carried out successfully. There was no rain, and although the wind blew stiffly, it was "comparatively mild." The special train conveying the directors and invited guests left the Waverley Station, Edinburgh, in two portions, the first at 10.45, the second, to which the Royal carriages were attached, ten minutes later. At the Forth Bridge Station Sir John Fowler, Mr. Benjamin Baker, Mr. William Arrol, Mr. Phillips, and other gentlemen connected with the building of the bridge, awaited the arrival of the Royal party from Dalmeny. By the special desire of the Prince of Wales, who wished to have an opportunity of examining some details of the structure, the Royal train steamed very slowly across the bridge. As seen from the shore, the long train of large saloon carriages is said to have looked like "a mere toy as it passed through the stupendous framework of tubes and girders at Inverkeithing." From the North Queensferry Pier the steam launch *Dolphin* conveyed the Royal party and the directors over the Firth, so that the bridge might be seen from the sea; and another vessel followed, containing the rest of the company. Both vessels steamed out to the middle of the Firth; and, according to the *Times*, the view was much enjoyed "as each cantilever was passed in succession, the junction of the girder bridges with the cantilever arms being specially noted." Afterwards, the bridge was recrossed, and in the middle of the north connecting girder the train stopped to allow the Prince of Wales to perform the ceremony of driving the last rivet. "A temporary wooden staging," says the *Times*, "had been erected there, and upon it His Royal Highness stepped, along with Lord Tweeddale, Lord Rosebery, and Mr. Arrol. The hydraulic rivetter was swung from one of the booms, the pressure being supplied from an accumulator at Inchgarvie. Two men were placed on the boom below to manipulate the machine. The gilded rivet having been placed in the bolt-hole, and the silver key having been handed to His Royal Highness by Lord Tweeddale, the Prince, with Mr. Arrol's assistance, finished the work in a few seconds, amid cheers. The rivet is in the outside of the boom, and holds together three plates. Around its gilded top there is an inscription stating that it is the 'last rivet, driven in by His Royal Highness the Prince of Wales, 4th March, 1890.' The train stopped a second time at the south great cantilever pier, where another platform had been erected, upon which several ladies were standing. Here the Prince again left the train, at half-past 1 o'clock, to make the formal declaration of the opening of the bridge. As the wind was blowing a perfect gale, so that His Royal Highness had difficulty in retaining a steady foothold, it was impossible to make a speech. He therefore simply said:-'Ladies and Gentlemen, I now declare the Forth Bridge open.' Hearty cheers greeted the announcement, and, the Prince having returned to his carriage, the train moved slowly along to the Forth Bridge Station."

At 2 o'clock a banquet was given in the model-room at the bridge works, the chair being occupied by Mr. M. W. Thompson. The Prince of Wales, responding to the toast of "The Prince of Wales and other members of the Royal Family," spoke as follows:—

"I feel very grateful for the kind words which have fallen from the chairman in proposing the toast, and I thank you all most heartily for the cordial way in which you have received it. The day has been a most interesting day to all of us, and especially so to me, and I feel very grateful that I have been asked to take part in so interesting and important a ceremony as the one at which we have all assisted. I had the advantage, nearly five and a half years ago, of seeing the Forth Bridge at its very commencement, and I always looked forward to the day when I should witness its successful accomplishment. I may perhaps say that in opening bridges I am an old hand. At the request of the Canadian Government I performed the opening ceremony 30 years ago of opening the Victoria Bridge over the St. Lawrence at Montreal, putting in the last rivet, the total of rivets being one million. To-day I have performed a similar ceremony for the Forth Bridge, but on this occasion the rivets number nearly eight millions instead of one million. The construction of the bridge has been on the cantilever principle, which has been known to the Chinese for ages, and specimens of it may be seen likewise in Japan, Tibet, and the North-West Provinces of India. Work of this description has hitherto been carried out on small dimensions, but in this case the engineers have had to construct a bridge in 30 fathoms of water, at the height of 150 feet above high water mark, and crossing two channels, each one-third of a mile in width. Had it not been for the intervening island of Inchgarvie the project would have been impracticable It may perhaps interest you if I mention a few figures in connection with the construction of the bridge. Its extreme length, including the approach viaduct, is 2765 yards, one and one-fifth of a mile, and the actual length of the cantilever portion of the bridge is one mile and 20 yards. The weight of steel in it amounts to 51,000 tons, and the extreme height of the steel structure above mean water-level is over 370 feet; above the bottom of the deepest foundation 452 feet, while the rail-level above high water is $156\frac{1}{2}$ feet. Allowance has been made for contraction and expansion and for changes of temperature to the extent of one inch per 100 feet over the whole bridge. The wind-pressure provided for is 56 lb. on each square foot of area, amounting in the aggregate to about 7700 tons of lateral pressure on the cantilever portion of the bridge. About 25 acres of surface will have to be painted with three coats of paint. As I have said, about eight millions of rivets have been used in the bridge, and 42 miles of bent plates used in the tubes, about the distance between Edinburgh and Glasgow. Two million pounds have been spent on the site in building the foundations and piers; in the erection of the superstructure; on labour in the preparation of steel, granite, masonry, timber, and concrete; on tools, cranes, drills, and other machines required as plant; while about two

and a half millions has been the entire cost of the structure, of which £800,000 (nearly one-third of this amount) has been expended on plant and general charges. These figures will give you some idea of the magnitude of the work, and will assist you to realize the labour and anxiety which all those connected with it must have undergone. The works were commenced in April 1883, and it is highly to the credit of everyone engaged in the operation that a structure so stupendous and so exceptional in its character should have been completed within seven years. The opening of the bridge must necessarily produce important results and changes in the railway service of the east coast of Scotland, and it will, above all, place the valuable manufacturing and mineral-producing district of Fife in immediate communication with the south side of the Firth of Forth. When the Glenfarg line, now nearly completed, is opened for traffic, the distance between Edinburgh and Perth will be reduced from 69 to 47 miles, and instead of the journey occupying, as at present, two hours and 20 minutes, an express will be able to do it in an hour. Dundee, likewise, will be brought to within 59 miles of Edinburgh, and Aberdeen 130 miles, and no sea ferries will have to be crossed. The construction of the bridge is due to the enterprise of four important railway companies—(1) North British (the bridge is in its district), (2) North-Eastern, (3) Midland, and (4) Great Northern—and the design is that of two most eminent engineers, Sir John Fowler and Mr. Benjamin Baker. The contractor was Mr. William Arrol, and the present Tay Bridge, and the bridge which I have inaugurated to-day, will be lasting monuments of his skill, resources, and energy. I have much pleasure in stating that, on the recommendation of the Prime Minister, the Queen has been pleased to create Mr. Matthew William Thompson, Chairman of the Forth Bridge Company and of the Midland Railway Company, and Sir John Fowler, engineer-in-chief of the Forth Bridge, baronets of the United Kingdom. The Queen has also created, or intends to create, Mr. Benjamin Baker, Sir John Fowler's colleague, a Knight Commander of the Order of St. Michael and St. George, and to confer on Mr. William Arrol, the contractor, the honour of a knighthood. I must not allow this opportunity to pass without mentioning the valuable assistance which has been rendered to the companies by Mr. Wieland, their able and indefatigable secretary, who deserves especial praise for the admirable way in which he has carried out the important financial arrangements essential in a scheme of such magnitude. Before concluding I must express my pleasure at seeing here Major-General Hutchinson and Major Marindin, two of the nspecting officers of the Board of Trade. Although in this country great undertakings of the kind which we are celebrating this day are wisely wholly left to the enterprise and genius of private individuals without aid or favour from the State; yet, in connection with these particular works, Parliament, I am informed, for the first time associated officers of the Board of Trade with those practically engaged in the construction of this magnificent bridge from its commencement by requiring the Board of Trade to make quarterly reports to be laid before Parliament as to the nature and progress of the works. This most important and delicate duty has been performed by Major-General Hutchinson and Major Marindin; and I now congratulate them on the completion of their responsible duties, which they have carried out in a way that redounds credit to themselves and to the department which they so ably serve. Allow me again, gentlemen, in thanking you for the kind way in which you have received this toast, to assure you of the great pleasure and gratification it has been to me to have been present on this occasion to inaugurate this great success of the skill of engineering."

Sir John Fowler, in acknowledging the toast of the Forth Bridge, said he begged to return his most grateful thanks to His Royal Highness the Prince of Wales for the flattering manner in which he had spoken of their work. It was now seven years ago since the foundations of the bridge were commenced, but up to two years ago they had to endure not only the legitimate anxieties of their duties, but the attacks and evil predictions which were always directed against those who undertook engineering work of novelty or exceptional magnitude. It was very curious to watch the manner of retreat of these prophets of failure. The results had proved them to be mistaken. But he could tell some very curious stories connected with the bridge. He pointed out how, from the nature of the materials which had been used in the construction of the bridge, and from the nationality of the men who had been engaged in that construction, the bridge possessed an international character. He also predicted that the bridge would last for many, many years, and he cordially acknowledged the workmanship and ability of all who had assisted in its erection. As to the workmen themselves, he said they had done admirable work, and had never knowingly scamped a rivet.

Mr. Arrol also acknowledged the toast, and Mr. Baker, in response to calls from the audience, made a few remarks.

Mr. John Dent, Deputy-Chairman of the Forth Bridge Railway Company, in proposing the toast of "The Guests," congratulated the recipients of the special honours bestowed by the Queen, and he spoke of the universal reputation which had become attached to the bridge, which stood as a monument of industry, of genius, and of ability.

After a clever speech from Lord Rosebery, Herr Mehrtens, of the Prussian Railway Department, replied for himself and in the name of his companions from Saxony, Austria, and Hungary. He expressed their feelings of thankfulness that they had been permitted to be present on so interesting an occasion, and their admiration at all the wonderful things they had seen that day. That day, he said, marked the commencement of a new era in iron bridge building. He congratulated Great Britain, which had led the way in iron bridge building, on now having the largest span bridge and the strongest bridge in the world.

M. Picot, on behalf of the railway engineers of France, also replied in a speech in which he eulogized the bridge and its engineers and contractors.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—The General Board of Studies announce that they will this term appoint an additional Lecturer in Botany for three years, from the beginning of the Easter term 1890. The stipend is £100 a year. Names of candidates are to be sent to the Vice-Chancellor on or before March 8.

The Syndics of the Press propose that a gift of books published by them shall be made to the Library of the University of Toronto, lately destroyed by fire.

The discussion by the Senate of the proposal to accept the Newall telescope was on the whole favourable to the proposal, though the difficulty of finding the funds required for its adequate maintenance and use has not yet been made. From remarks made by members of the Observatory Syndicate, it appears that it regards the purchase of a large reflecting telescope as the first claim on the Sheepshanks Fund; and it is unwilling to deplete the fund until this purchase can be effected. Prof. Liveing referred to the recent development of astronomical physics, and said the University was bound to further it. The Newall telescope was specially suited for physical researches, and to reject it as a "white elephant" would damage the University by discouraging other benefactors. The matter is to be referred to the Financial Board.

At the meeting of the Philosophical Society on March 10, the following papers are promised:—W. Gardiner, on the germination of *Acacia sphærocephala*; M. C. Potter, the thickening of the stem in Cucurbitaceæ; Dr. Lea and W. L. Dickinson, note on the action of rennin and fibrin-ferment; W. Bateson, on some skulls of Egyptian mummified cats.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, February 20.—"A Comparative Study of Natural and Artificial Digestions" (Preliminary Account). By A. Sheridan Lea, Sc.D., Fellow of Gonville and Caius College, Cambridge, University Lecturer in Physiology. Communicated by Prof. Michael Foster, Sec. R.S.

The objects of the investigation were (i.) to obtain in artificial digestions some closer approximation to the general conditions under which natural digestion is carried on in the body, and (ii.) to apply the improved methods of carrying on artificial digestions to the elucidation of some special differences, which so far have appeared to exist between the natural and artificial processes.

An apparatus was described by means of which digestions can be carried on in a dialyzer in such a way as to provide for the constant motion of the digesting mixture and the removal of digestive products: by this method a partial reproduction of two of the most important factors in natural digestion is provided.

So far the method has been employed for

I. *The salivary digestion of starch.* Experiments conducted under otherwise similar conditions in the dialyzing digester and a flask, showed that—(i.) The *rate* of digestion in the former is always greater than in a flask, and at the same time the tendency to the development of bacteria is greatly lessened. (ii.) The amount of starch converted into sugar is always greatest in the dialyzer. (iii.) The total sugar formed and small residue (4·29 per cent.) of dextrin left during an active and prolonged digestion in the dialyzer justify the assumption that, under the more favourable conditions existing in the body, the whole of the starch taken is converted into sugar before absorption.

The above results afford an explanation of the existing discordant statements as to the nature and amount of products formed during starch digestion.

II. *The tryptic digestion of proteids.* The experiments made dealt chiefly with the formation of leucin and tyrosin, and were undertaken, initially, in order to find out why these crystalline products are formed in large amount during an artificial digestion, while they have so far been described as occurring in mere traces during natural digestion. The results of the experiments made it probable that leucin and tyrosin should be formed during natural digestion. Examination of the contents of the small intestine during proteid digestion showed that, contrary to existing statements, leucin and tyrosin are formed in not inconsiderable quantities during the natural process.

The last part of the communication dealt with the probable physiological importance of the formation of amidated bodies during tryptic digestion, and a view was put forward as to the possible and probable importance of amides in the chemical cycle of animal metabolism.

The experiments are being extended to the pancreatic digestion of starch.

Linnean Society, February 20.—W. Carruthers, F.R.S., President, in the chair.—Mr. G. C. Druce exhibited specimens of *Agrostis canina*, var. *Scotica*, and a small collection of flowering plants dried after treatment with sulphurous acid and alcohol, and showing a partial preservation of the natural colours of the flowers.—Mr. F. P. Pascoe exhibited a series of Coleopterous and Lepidopterous insects to show the great diversity between insects of the same family.—The Right Hon. Sir John Lubbock, Bart., M.P., P.C., then gave an abstract of four memoirs which he had prepared: (1) on the fruit and seed of the Juglandiæ; (2) on the shape of the oak-leaf; (3) on the leaves of Viburnum; and (4) on the presence and functions of stipules. An interesting discussion followed, in which Mr. J. G. Baker, Mr. John Fraser, Mr. D. Morris, and Prof. Marshall Ward took part.

EDINBURGH.

Royal Society, February 17.—Sir W. Thomson, President, in the chair.—Prof. Crum Brown communicated a paper, by Mr. Tolver Preston, on Descartes' idea of space and Sir W. Thomson's theory of extended matter.—The following communications from the chemical laboratory of the University were read:—(*a*) Prof. Crum Brown, on a new synthesis of dibasic organic acids. The method proposed was the electrolysis of potassium ethyl salts of lower dibasic acids which would take place according to the scheme

$$2EtOOC.R''.COOK = EtOOC.R''.R''.COOEt + 2CO_2 + 2K,$$

thus giving the diethyl ether of a higher acid of the same series. (*b*) Prof. Crum Brown and Dr. James Walker, on the electrolysis of potassium ethyl malonate, and potassium ethyl succinate. The reaction actually takes place in great measure in the above indicated sense, the yields of pure succinic ether and of adipic ether respectively being from 20 to 30 per cent. of the theoretically obtainable quantities. The method is thus proved to be of practical as well as of theoretical importance. (*c*) Dr. John Gibson, on the action of bromine and carbonate of soda in solutions of cobalt and nickel salts.—Mr. W. Calderwood read a paper on the swimming bladder and flying powers of *Dactylopterus volitans*.

PARIS.

Academy of Sciences, February 24.—M. Hermite in the chair.—The proofs of the separation of the south-east extremity of the Asiatic continent during recent times, by M. Émile Blanchard. The author advances proofs from the resemblance of animal and vegetable life in Further India, on the peninsula of Malacca, and Sunda Islands.—The *Dryopithecus*, by M. Albert Gaudry. The relation of *Dryopithecus* to the ape and to man has been investigated.—A contribution to the chemical study of the truffle, by M. Ad. Chatin. The researches have been directed to the quantitative determination of the organic and other matter in truffles.—Scrotal pneumocèles, by M. Verneuil.—On the anatomy and the physiological pathology of the retention of urine, by M. F. Guyon.—Transformations in kinematic geometry, by M. A. Mannheim.—On the constitution of the line spectra of elements, by M. J. R. Rydberg. This is a note on the periodic recurrence of doubles and triplets in the spectrum of an element. It is shown how this periodicity enables the spectrum of an element to be found by interpolation when the spectra of elements of the same group are known, the case of gallium being given as an example of the verification of the principle.—Electrical oscillations in rarefied air, without electrodes; demonstration of the non-conductivity of the vacuum, by M. James Moser. It is well known that vacuum-tubes become luminous when near an induction coil in action. The author, by enveloping one vacuum-tube with another, in which the rarefaction could be varied, finds that the excitation may take place without any electrode. If the pressure in the outer tube be equal to 760 mm., the inner tube, under the influence of the coil, becomes luminous and of a clear blue colour; if, however, the pressure be diminished to 1 mm. of mercury, the air in the outer tube becomes luminous and of a pronounced red colour, thus reversing the phenomena.—Upon the variation, with the temperature, of the bi-refractions of quartz, barytes, and kyanite, by MM. Er. Mallard and H. Le Chatelier. This variation has been studied by the aid of a photographic spectroscopic method: with quartz a singular point is detected at 570°, at which temperature the law of variation suddenly changes; a similar phenomenon is indicated as occurring in the case of kyanite somewhere between 300° and 600°.—The vapour-pressure of acetic acid solutions, by MM. F. M. Raoult and A. Recoura. It has been previously shown by one of the authors (*Comptes rendus*, May 23, 1887; *Annales de Chimie et de Physique*, 6th series, t. xv., 1888) that, if f represents the vapour-tension of a solvent for a certain temperature, f' the vapour-tension under similar conditions when a non-volatile body is in solution, P the weight of substance dissolved in 100 grms. of the solvent, M the molecular weight of the dissolved body, and M' the molecular weight of the solvent, then for dilute solutions—

$$K = \frac{100(f - f')}{f'P} \cdot \frac{M}{M'},$$

K being a constant generally near to unity. Employing the dynamical method, the mean value of K for acetic acid is found to be 1·61, taking 60 as the molecular weight of acetic acid; but if the molecular weight of a liquid be the same as that of the saturated vapour, the apparent anomaly disappears, for with molecular weight 97 (deduced from density of saturated acetic acid vapour at 118°, viz. 3·35), the above formula gives K = 1.—The action, in the dry way, of various arseniates of potassium and sodium upon the oxides of the magnesia series, by M. C. Lefèvre.—Note on the volumetric estimation of copper, by MM. A. Etard and P. Lebeau. A method of titration is given by the authors, for which they claim a rapidity and accuracy comparable to the permanganate method for iron; it is based upon the formation of a characteristic violet coloration on the addition of concentrated hydrobromic acid to a solution of the copper salt, and the subsequent decoloration of the solution by standardized stannous chloride solution containing much hydrochloric acid; thus—

$$\underbrace{2CuBr_2 + nHBr}_{\text{Coloured.}} + SnBr_2 = SnBr_4 + \underbrace{Cu_2Br_2 + nHBr.}_{\text{Uncoloured.}}$$

—Preparation of hydroxycamphocarbonic acid from camphocarbonic acid, by MM. A. Haller and Minguin.—Upon the organization of left-handed Prosobranchiate Gastropoda (*Neptunea contraria*, Linnæus), by MM. P. Fischer and E. L. Bouvier.—

Upon the initial cells of the ovary in fresh-water Hydræ, by M. Joannes Chatin.—Note on a new putrefaction ptomaine, obtained by the culture of *Bacterium allii*, by Mr. A. B. Griffiths. The author gives analyses of an alkaloid, produced by the decomposition of albuminoids by this organism, showing it to belong to the hydropyridine series, and to possess the formula of hydrocoridine, $C_{10}H_{17}N$.—On the chromogenous functions of the pyocyanic bacillus, by M. C. Gessard.—Fossil Radiolarians inclosed in albite crystals, by M. A. Issel. The author concludes from the data given—(1) that a sedimentary fossiliferous rock has become crystalline and rich in plagioclastic crystals, without the stratification being sensibly altered; (2) that this change has been produced in a Tertiary formation; (3) that a hydrothermal action is indicated.—A contribution to the history of chrome-iron, by M. Stanislas Meunier.

DIARY OF SOCIETIES.

London.

THURSDAY, March 6.

Royal Society, at 4.30.—On a Second Case of the Occurrence of Silver in Volcanic Dust—namely, in that thrown out in the Eruption of Tunguragua, in the Andes of Ecuador, January 11, 1886: Prof. J. W. Mallet, F.R.S.—On the Tension of Recently-formed Liquid Surfaces: Lord Rayleigh, Sec.R.S.—(1) On the Development of the Ciliary or Motor Oculi Ganglion; (2) The Cranial Nerves of the Torpedo (Preliminary Note): Prof. J. C. Ewart.

Linnean Society, at 8.—On the Production of Seed in some Varieties of the Common Sugar-Cane (Saccharum officinarum): D. Morris.—An Investigation into the True Nature of Callus; Part 1, the Vegetable Marrow, and Ballia callitricha: Spencer Moore.

Royal Institution, at 3.—The Early Developments of the Forms of Instrumental Music: Frederick Niecks.

FRIDAY, March 7.

Physical Society, at 5.—On Bertrand's Refractometer: Prof. S. P. Thompson.

Geologists' Association, at 8.—On some Pleistocene (non-Marine) Mollusca of the London District: B. B. Woodward.—Notes on some Pleistocene Sections, in and near London: W. J. Lewis Abbott.—Note on a Curious Appearance produced by the Natural Bisection of some Spherical Concretions in a Yoredale Stone Quarry near Leek: Dr. Wheelton Hind.

Institution of Civil Engineers, at 7.30.—Telephonic Switching: C. H. Wordingham.

Royal Institution, at 9.—Electrical Relations of the Brain and Spinal Cord: Francis Gotch.

SATURDAY, March 8.

Royal Botanic Society, at 3.45.

Royal Institution, at 3.—Electricity and Magnetism: Right Hon Lord Rayleigh, F.R.S.

SUNDAY, March 9.

Sunday Lecture Society, at 4.—Pasteur, and his Discoveries (with Oxyhydrogen Lantern Illustrations): Sir Henry E. Roscoe, M.P., F.R.S.

MONDAY, March 10.

Royal Geographical Society, at 8.30.—On Lieut. H. B. Vaughan's Recent Journey in Eastern Persia: Major-General Sir Frederic J. Goldsmid, K.C.S.I.

Victoria Institute, at 8.—On the Monism, Pantheism, and Dualism of Brahmanical and Zoroastrian Philosophers: Sir M. Monier-Williams, K.C.I.E.

TUESDAY, March 11.

Society of Arts, at 8.—The Claims of the British School of Painting to a Thorough Representation in the National Gallery: James Orrock.

Anthropological Institute, at 8.30.—Exhibition of the Skull of a Carib, from a Cave in Jamaica: Prof. Flower, C.B., F.R.S.—Manners, Customs, Superstitions, and Religions of South African Tribes: Rev. James Macdonald.

Institution of Civil Engineers, at 8.—The Hawksbury Bridge, New South Wales: C. O. Burge.—The Erection of the Dufferin Bridge over the Ganges at Benares: F. T. G. Walton.—The New Blackfriars Bridge on the London, Chatham, and Dover Railway: G. E. W. Cruttwell. (Discussion.)

Royal Institution, at 3.—The Post-Darwinian Period: Prof. G. J. Romanes, F.R.S.

WEDNESDAY, March 12.

Geological Society, at 8.—On a Deep Channel of Drift in the Valley of the Cam, Essex: W. Whitaker, F.R.S.—On the Monian and Basal Cambrian Rocks of Shropshire: Prof. J. F. Blake.—On a Crocodilian Jaw from the Oxford Clay of Peterborough: R. Lydekker.—On Two New Species of Labyrinthodonts: R. Lydekker.

Society of Arts, at 8.—The Chemin de Fer Glissant, or Sliding Railway: Sir Douglas Galton, K.C.B., F.R.S.

THURSDAY, March 13.

Royal Society, at 4.30.

Mathematical Society, at 8.—Some Groups of Circles connected with Three given Circles: R. Lachlan.—Perfect Numbers: Major P. A. MacMahon, R.A.

Society of Arts, at 5.—Agriculture and the State in India: W. R. Robertson.

Institution of Electrical Engineers, at 8.—The Theory of Armature Reactions in Dynamos and Motors: James Swinburne.—Some Points in Dynamo and Motor Design: W. B. Esson. (Discussion.)

Royal Institution, at 3.—The Early Development of the Forms of Instrumental Music (with Musical Illustrations): Frederick Niecks.

FRIDAY, March 14.

Royal Astronomical Society, at 8.

Royal Institution, at 9.—The Glow of Phosphorus: Prof. T. E. Thorpe, F.R.S.

SATURDAY, March 15.

Society of Arts, at 3.—The Atmosphere: Prof. Vivian Lewes.

Royal Institution, at 3.—Electricity and Magnetism: Right Hon. Lord Rayleigh, F.R.S.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Prodromus Faunæ Mediterraneæ, Part 2: J. V. Carus (Stuttgart, Koch).—The Elements of Laboratory Work: A. G. Earl (Longmans).—History of Botany (1530–1860): J. von Sachs; translated by H. E. F. Garnsey; revised by I. B. Balfour (Clarendon Press).—Traité Encyclopédique de Photographie, neuv. fasc.: C. Fabre (Paris, Gauthier-Villars).—A Syllabus of Elementary Dynamics: Prof. W. N. Stocker (Macmillan).—Synoptical Tables of Inorganic and Organic Chemistry: C. J. Leaper (Gill).—The Growth of Capital: R. Giffen (Bell).—Coal Gas as a Fuel: T. Fletcher (Warrington, Mackie).—The Zoological Record for 1888 (Gurney and Jackson).—An Elementary Treatise on Light and Heat, 2nd edition: Rev. F. W. Aveling (Relfe).—Demoids: J. B. Sutton (Baillière).—The Railways of Scotland: W. M. Ackworth (Murray).—Electrical Engineering: W. Slingo and A. Brooker (Longmans).—Un Viaggio a Nías: E. Modigliani (Milano, Fratelli Treves).—Transactions of the Astronomical Observatory of Yale University, vol. i. Part 2 (New Haven).—Cycles of Drought and Good Seasons in South Africa: D. E. Hutchins (Wesley).—How to Know Grapes by the Leaves: A. N. M'Alpine (Edinburgh, Douglas).—Boilers, Marine and Land, 2nd edition: T. W. Traill (Griffin).—Four-Figure Mathematical Tables, 2nd edition: J. T. Bottomley (Macmillan).—The Cultivated Oranges and Lemons, &c., of India and Ceylon, text and plates: Dr. E. Bonavia (Allen).—Elementary Manual of Magnetism and Electricity, Part 2: Prof. Jamieson (Griffin).—Quarterly Journal of Microscopical Science, February (Churchill).—Zeitschrift für Wissenschaftliche Zoologie, 49 Band, 3 Heft (Williams and Norgate).—Journal of the Royal Microscopical Society, 1889, Part 6*a*, 1890, Part 1 (Williams and Norgate).—Studies from the Biological Laboratory, Johns Hopkins University, vol. 4, No. 6 (Baltimore).—Transactions and Proceedings of the Botanical Society, vol. xvii. Part 3 (Edinburgh).—Annual Report of the Canadian Institute, Session 1888–89 (Toronto).

CONTENTS.

THURSDAY, MARCH 13, 1890.

GERMAN CONTRIBUTIONS TO ETHNOLOGY.

Ethnographische Beiträge zur Kenntniss des Karolinen Archipels. Von J. S. Kubary. 1 Heft, mit 15 Tafeln. (Leyden: P. W. M. Trap, 1889.)

SINCE 1868, when Herr Kubary first entered upon a course of inquiry among the Polynesians, which he had undertaken for the Godeffroy Museum in Hamburg, to which institution he was then officially attached, he has made the archipelago of the Carolines the chief seat and object of his observations. These islands, lying between 5° and 10° N. lat., midway between the Ladrones and New Guinea, and stretching from 138°–160° E. long., have been visited by few white men excepting the traders who occasionally touch there for purposes of barter, or with the object of securing workmen for some more or less remote labour-market on terms of hire which are usually misunderstood by the natives themselves. To this drain on the numbers of able-bodied men, and to continual tribal wars among the different members of the group, the rapid diminution of the population of the Carolines is probably mainly due. In some of the islands the author found that the once numerous families of the kings or chiefs had either wholly died out in recent years, or were only represented by a single male descendant, who, in the absence of any other woman of pure native race, would have to take a half-sister for his wife, if he would avoid the alternative of making a prohibited exogamic marriage.

The probably imminent extermination of these Northern Polynesians gives more than common interest to Herr Kubary's narrative of his long sojourn in the island Yap, and in the Pelew group, or Western Carolines, where he had the good fortune to obtain previously-unknown information regarding the various indigenous moneys in use, and thus to establish the hitherto unsuspected fact that among these people a carefully-adjusted and rigidly-prescribed monetary system has been long in force. Thus in the island of Yap he found that each distinct kind of money could only be used for specially-defined purposes, the form known as *gau*, which consists of strings of equally-sized polished disks of the spondylus, constituting what we may term the gold of the district. This is not current among the general public, but is carefully accumulated by the chiefs, who keep it in reserve to be exchanged with other chiefs for canoes or weapons of all kinds, to be used when they are preparing to make, or to resist, a hostile attack. This spondylus currency has considerable ethnological interest, for we find that the shell can only be procured to the east or the north of Yap, and that it is traditionally the most ancient form of money in use in that and some of the neighbouring islands, while its discovery in old graves of chiefs in the Ladrones seems to point to a common origin of the natives of the latter group and those of the Carolines. Next in value is the *palan*, which consists of round disks of arragonite of various degrees of thickness, which is obtained by the people of Yap at considerable risk and with much labour from certain islands of limestone-formation in the Pelew group. The supply of this money in Yap is mainly dependent on the enterprise of the young men of the villages, who, from time to time combine together to procure a canoe, in which, with the consent of their chief, they repair to the arragonite rocks to extract as much of the stone as their boat will hold. On returning to their native village, they are bound to present their chief with all the larger blocks, after which they dispose of the remainder to the villagers at the rate of the market value of the stone, which is estimated according to its width. Thus, while a fragment measuring an inch or two in diameter is the recognized price of a basket of *taro*, consisting of a definite number of roots, the scale of values rises gradually until it requires a mass six feet in width to purchase a good-sized canoe, or a *gau*-belt adorned with two whale's teeth, which ranks in the eyes of a Yap dandy as the most precious of all personal ornaments. The arrival of a cargo in which there are several of these exceptionally large blocks, is generally soon followed by the breaking out of hostilities between the village chief and his neighbours, as the former seldom loses a chance of making speedy use of these sinews of war; and hence perhaps *palan* is popularly known as "men's money." Next in value to it comes *yar*, which consists of small threaded nacreous shells that serve as small change, and are known as "women's money."

In the Pelew Islands, another form of money, known as *audouth*, is current, whose origin and history are unknown, although the traditions regarding it suggest that it may have been obtained through early trading relations between these islands and remote eastern and western nations. *Audouth* is divided into numerous groups, consisting of coloured or enamelled beads or disks, some of which present a vitreous or earthy character, recalling objects of Chinese or Japanese art; while others, to judge by the coloured illustrations in Herr Kubary's work, are almost identical with the glass beads still largely manufactured in Venice. Each variety of bead has a fixed place on the scale of values, which, beginning from the *taro*-basket unit, gradually rises, until it finally reaches so large an amount that each of the still existing forty or fifty beads, which rank as the highest in the series, and which are all accumulated in the hands of one or two of the kings, actually represents a sum equal to ten or twelve pounds sterling. The extremely limited number of the *audouth*-beads, and the obligation of making payments with only specially prescribed forms of these coins, have led to the establishment of a regularly organized system of loans. By the rules of this system, a man who requires to make a payment in a coin of which he is not possessed, and who has to borrow it from his chief, or some neighbour, is compelled to give in pledge certain definite objects, only redeemable by repayments at fixed periods and rates of interest, while he is, moreover, obliged to refund his debt in the same coin which he originally borrowed.

In his comments on the singular fact that the unclothed, tattooed natives of a remote Polynesian archipelago should possess well-organized systems, based on fixed principles, not only for regulating loans, but also for conducting exchange and barter on equitable terms, Herr Kubary adduces apparently good grounds for assuming that the people have derived these methods

together with the principal features of their political and social institutions, through their early acquaintance with the higher civilization of the great Malayan States, with whose inhabitants they probably share one common origin. Like these races, the people of the Carolines attach an extraordinary importance to money, which is made the pivot on which everything in the State turns. Thus, the sole penalty for all crimes and misdemeanours is a fixed payment in some definite form of money; and, as among our own northern ancestors, every injury done to man or beast has its recognized price, while every act or event in a man's life from his birth to his death, and beyond it, is charged with a definite payment. Similarly, the favour of the gods in sickness, and the good-will of a chief, would seem to be regarded as only attainable by money offerings to priests or rulers. Strangely enough, however, the chiefs themselves are compelled to make certain prescribed payments in their various transactions with the people, by which means an excessive accumulation of money in the hands of a few is prevented, and a free circulation of the various coins insured; and thus, these uncivilized Polynesians have attempted, after their own fashion, to solve a problem involved in the question of capital and labour.

The author's copiously illustrated descriptions of the dwellings and other buildings erected by the islanders show how closely they approximate in structure and ornamentation to the Malayan type. The arrangements of the interior, however, where the quiet and solitude of the owner of a house are provided for by various portions of the building being tabooed to all strangers, and at certain times to the women and children of the family, afford strong evidence that in their social usages the people have been strongly influenced, probably in recent ages, by intercourse with Polynesians occupying the remoter eastern archipelagoes. This is shown by the uniformity in various practices followed both by the natives of some of the Carolines, and those of other far distant groups.

Nothing, however, is more remarkable than the diversity presented by contiguous islands, for while in the one we find some form of textile art or some method of elaborate tattooing, characteristic of the inhabitants of a far distant archipelago, not a trace of either is to be met with in the neighbouring islands. Even more inexplicable are the differences in stature, appearance, and general physical character among the natives of one island, or one group; and hence it is impossible to arrive at any firmly-based conclusions as to the true ethnic history of the present occupants of the Caroline archipelago.

Herr Kubary has devoted much attention to the study of the various maladies from which the natives suffer, with a view of determining how far these are indigenous or imported; and, while he highly commends the patience under suffering of these gentle, unsophisticated natives, he shows that various specific forms of disease, which are usually malignant among civilized communities, here present a benign character. His remarks on this subject are full of interest, as are also his descriptions of the various local remedies employed, among which it would appear that some possess such well-marked specific properties as to merit the careful attention of our own pharmacologists.

The present volume, which is to be followed by a further series of Herr Kubary's contributions, is edited by Dr. Schmeltz, on behalf of the directors of the Imperial Museum of Ethnology in Berlin, where the most valuable of the author's collections are deposited.

ENGLISH AND SCOTTISH RAILWAYS.

The Railways of England. By W. M. Acworth. Second Edition. (London: John Murray, 1889.)

The Railways of Scotland. By W. M. Acworth. (London: John Murray, 1890.)

BEYOND the comparatively small railway circle, there are many persons who take great interest in the railway system of this country. Any particularly fast train is carefully noted, and compared detail for detail with its predecessor; and its particular virtues are pointed out. To such persons the works before us will be most welcome. To railway men we need only say that not to read these books will be a great loss and a mistake. Mr. Acworth has evidently had excellent opportunities for observation, and he has not failed to make good use of the chances thus obtained for careful study of the many different phases of railway life. The author confesses to have written anonymously not a few criticisms on the management of certain English railways, which were meant to be particularly scathing. In the present books we can find nothing of the kind; in fact, in most cases the author uses language of almost unvarying panegyric, even the hunting-ground of the "Flying Watkin Express" coming in for nothing but praise. This is certainly as it should be, for those who know anything of the subject are aware that the English railway system taken as a whole is second to none in the world, either in management, rolling-stock, or permanent way.

The volume on the railways of England deals principally with the railways terminating in London. An historical sketch of the early railways is given, and we find, besides much useful matter, many amusing anecdotes. The author deals at length with the change wrought by the introduction of railways in the various trades affected by the withdrawal of the stage-coach, and the consequent loss of trade to many towns and villages on the old turnpike roads, as well as the birth of new trades and occupations caused by the advance of the railway system.

The London and North-Western Railway is the first one noticed, in Chapter II. The territory of this railway extends from London in the south to Carlisle in the north, and from Cambridge in the east to Swansea and Holyhead in the west. The description naturally begins at Crewe, for at this station are the main locomotive and other works of the Company, employing about 6000 men. Here also are the head-quarters of the locomotive staff, under Mr. F. W. Webb, the able mechanical superintendent. The author gives an excellent description of the works, and the many special manufactures carried on. The illustration of the Webb transverse steel sleeper shows how a steel sleeper can be designed to suit the English mongrel-sectioned rail known as the "Bullhead." It is a pity some enterprising railway manager in England does not give the Indian all-steel permanent way a trial

viz. a Vignoles or flanged rail with a transverse steel sleeper formed out of a ribbed plate, with lugs or clips formed out of the solid to take the rail flange, and fastened with a steel key. In this system there is nothing that can get loose, and excellent results are obtained in India, where several millions are now in use.

In Chapter IV. we find the Midland Railway thoroughly discussed. The growth of this enterprising and pushing Company is carefully and vividly delineated. This large system, like most others, is the result of the amalgamation of many small companies, and, under an enlightened management, it has long been considered the most progressive railway in this country. The author gives a capital description of this large system, and many interesting statistics. Among the many special details, perhaps the Lickey incline on the Birmingham and Gloucester section is of most interest. On this incline, having a gradient of 1 in 37, the traffic has always been worked by locomotives, even in the days when stationary engines were used to haul the trains out of Euston Station and Lime Street Station at Liverpool; and further, in these early days (1839), the English-built locomotive was unable to be of much use on this incline, and some American locomotives were imported and succeeded in working the traffic. Derby is the "Crewe" of the Midland. Here the Company builds the locomotives, carriages, and most of the waggons. The travelling public owe much to the Midland Company. On this line the author tells us most of the new departures in rolling-stock and details were originally tried, the Pullman car and many other equally important novelties, down to the diminutive but most useful apparatus, the sand-blast, for sanding the rails under the treads of the driving-wheels of the locomotive. The effects of this apparatus are very interesting, and its use is becoming universal. So much does it add to the effectiveness of a single-wheeled locomotive that it is possible to use it on trains in place of the four-coupled engine, a saving evident to those familiar with the subject. The single-wheeled engines, running at high speeds, are more free; which means less wear and tear to the engine itself, and probably the permanent way. With an express train the sand-blast apparatus uses about nine ounces of sand per mile, giving a continuous supply to the driving-wheels; and, be the rails ever so greasy, the wheels seldom slip half a turn. The testing of the materials used at Derby Works appears to be very efficient; the steel, particularly for plates, axles, tyres, &c., being thoroughly tested by tensile and bending tests, and by chemical analysis.

Chapter V. deals with the Great Northern, North-Eastern, and Manchester, Sheffield, and Lincolnshire Railways. In any description of the Great Northern system it would be impossible to pass over the splendid running of the Company's express trains. Some of these are, without doubt, the fastest in the world. The 105¼ miles between Grantham and London are continuously "done" in 117 minutes, or at the rate of 54 miles per hour; and both up and down trains are known to get over 60 consecutive miles in as many minutes. On one occasion, the author states, the 105¼ miles were "reeled off" in 112 minutes—a result worthy of Mr. Stirling's splendid locomotives. The description of driving the "Flying Scot" is very true, and we are glad to observe that the author combats the nonsense written to the daily press concerning the drivers and firemen of the Scotch expresses "being paralyzed with fear at the awful speeds." No two men are prouder of their positions, nor would they exchange into any other link. Their position is, in fact, the blue ribbon of the foot-plate.

In dealing with the North-Eastern Railway, the author gives much useful information on the subject of the compound locomotive. The locomotive superintendent of that railway, Mr. T. W. Worsdell, uses probably the best arrangement of cylinders, &c., possible to fulfil the many conditions under which a satisfactory locomotive must be constructed, and the results obtained appear to point to a great saving in fuel. We would commend to our readers the description of the snow-block on this railway in the year 1886; it is well written.

With reference to the electric lighting of trains on the Glasgow underground section of the North British Railway, it should be noted that the current is taken off the third insulated rail, not by a brush, as stated by the author, but by means of a wheel in a swing frame under each coach. This wheel runs on the central elevated and insulated rail, and each coach is electrically independent of any other. The system appears to work very well. To the Manchester, Sheffield, and Lincolnshire Railway the author gives little attention, for reasons stated on p. 193. Probably no line in this country is more handicapped by heavy gradients on its main line, and the locomotive stock has had to be designed to satisfy the conditions, more especially on the section between Manchester and Sheffield. The late Mr. Charles Sacré, the eminent engineer and locomotive superintendent of that railway, designed some particularly fine four-coupled bogie engines for the passenger service, and his goods engines did good work on the heavy sections.

The Great Western Railway loses nothing by the description given in Chapter VI. This historical line is well described, and the "battle of the gauges" thoroughly gone into. It is to be regretted that some compromise was not made between the rival gauges; for it is now evident that the four feet eight and a half inches gauge—the standard one in this country—is not wide enough. Locomotives and rolling stock have grown so much that locomotive engineers are in difficulties when trying to design more powerful engines. Take, for instance, the Indian or the Irish broad gauge; in these cases the engines are not limited in width so much, and can have ample bearing surfaces; as well as, for inside cylinder engines, crank axles not tied down by considerations of cylinder centres and the like. A ride on the "Dutchman" express locomotive is well enough described to make many young locomotive engineers long to have shared with the author that thoroughly enjoyable experience. The Severn Tunnel is well treated in this chapter. Chapter VII. deals with the South-Western Railway, and the following one gives much useful information of that model of all southern railways—the London, Brighton, and South Coast Railway. In noticing the latter we cannot but express our regret for the loss that Company and locomotive engineering generally have sustained by the recent death of Mr. William Stroudley. Without doubt one of our ablest railway engineers, he brought the designing of locomotives and

rolling-stock to the highest pitch; his engines are patterns to be used with advantage, and their coal consumption is the lowest on record. Chapter IX. describes the South-Eastern and Chatham Railways; and the volume concludes with Chapter X., on the Great Eastern Railway. These last chapters lack none of the interest to be found in the earlier ones in the book.

The second volume, on Scottish railways, is merely a continuation of the first, and is written in the same lucid style. Its most interesting part is a description of the Forth Bridge. Mr. Acworth gives a good account of the bridge and the earlier schemes proposed for crossing the Forth.

Mr. Acworth has written two most interesting books, which will be of great use to all in any way connected with, or interested in, the British railway system.

N. J. L.

DISEASES OF PLANTS.

Diseases of Plants. By Prof. H. Marshall Ward, F.R.S., M.A. (London: Society for Promoting Christian Knowledge.)

THIS little book is an excellent popular introduction to the study of the diseases of plants, in so far as they are due to the attacks of parasitic Fungi or similar organisms. The author, who has made this field of research especially his own, succeeds in being intelligible and interesting to ordinary readers, without in any degree sacrificing the scientific character of his work.

The book is illustrated by fifty-three woodcuts, which have been very well selected, many of them from the author's own papers. In certain cases, however, the engraving leaves something to be desired, and scarcely does justice to the original figures.

An introductory chapter explains what is here meant by disease in plants, namely "those disturbances of the structure and functions of the plant, which actually threaten the life of the plants, or at least their existence as useful objects of culture." The two factors of disease, the external cause on the one hand, and the condition of the patient on the other, are clearly distinguished.

The second chapter gives a general account of Fungi as saprophytes and parasites. Mucor is described as an example of the former, and vine-mildew (*Peronospora viticola*) of the latter group.

The succeeding nine chapters, forming the bulk of the book, are occupied with the consideration of special diseases.

First comes the "damping-off" of seedlings, a disease only too well known to gardeners, due to the attacks of various species of Pythium. The whole life-history of the parasite is described. In Fig. 9 it is a pity that the point of attachment of the antheridium is not more clearly shown.

Next, we have an account of the very interesting disease of cabbages and other Crucifers, known as "fingers and toes," "club-root," &c. Here the cause of the mischief is a Myxomycete, and this is the only case of a non-fungoid disease described in the book. Happily, a satisfactory cure can here be prescribed.

Chap. v. is on the potato-disease. An account of the normal mode of nutrition of the plant in health is introduced in order to show the exact nature of the deadly injury which is wrought by the Phytophthora. As a preventive measure, the selection of resistant varieties of the potato is especially recommended. Chap. vi. is devoted to the "smut" of corn. The cause of the frequent failure of protective dressings applied to the ripe grain is discussed. If, however, as Jensen believes, the ovule may be infected at the time of flowering, an altogether new light is thrown on this question.

After a chapter on the disease known as "bladder-plums," caused by the yeast-like Fungus Exoascus, we come to the lily-disease. The Fungus which is here responsible has been shown by Prof. Ward to afford an excellent example of a saprophyte which can become a parasite on occasion.

The next three chapters describe the ergot of rye, the mildew of hop (Podosphæra), and the rust of wheat. In the case of the hop-disease, a figure of the conidia might have been added with advantage. The now familiar but always interesting story of the heterœcism of rust is well told.

With a caution which in the case of a popular work cannot be too highly commended, the author avoids expressing any opinion on the subjects of fertilization in Podosphæra, and of the function of the spermogonia in Æcidium.

In the concluding chapter, Prof. Ward endeavours to interest his readers in the wider questions of mycology, so fascinating to the botanist, such as the phylogenetic origin and relationships of the Fungi.

The book should have a wide circulation among the numerous classes interested in the important group of diseases of plants with which it deals.

D. H. S.

OUR BOOK SHELF.

The Physician as Naturalist. Addresses and Memoirs bearing on the History of and Progress of Medicine chiefly during the last hundred years. By W. T. Gairdner, M.D. (Glasgow: Maclehose and Sons, 1889.)

A SUCCESSFUL physician, during a long and busy life, is frequently called upon to preside and deliver addresses at meetings at which he is expected to treat his subjects in a more or less popular manner.

Dr. Gairdner has brought together a most interesting series of such addresses, which fall into two main groups. First, those in which he has contrasted the treatment of the present day with that in vogue among our predecessors of more or less remote times; and in which he has attempted to present the answer to that ever-interesting question, "Is the treatment of disease adopted at the present day superior to that in vogue formerly? And if so, in what does its superiority consist?" Second, those in which he lays down the lines on which he considers the medical education of the future should be conducted, in order to lead to still greater advances.

The dependence of modern treatment upon the discussion of accumulations of facts, and not solely upon theory, and the necessity of making experience and

not authority the arbiter in cases of doubt, are the conclusions which the author inculcates throughout.

A century ago it was considered a fundamental principle that venesection was essential in most, if not all, serious illnesses; and, to such an extent was this carried, that 200 ounces of blood were sometimes drawn off during a week, and even half that amount in 24 hours. Next came a reaction, and the theory that fever patients required stimulation, rather than venesection, led to the administration of enormous quantities of alcohol, especially at the hands of Dr. Todd, who at times administered more than four gallons of brandy to young girls during an illness. Finally, to Dr. Gairdner himself is due much of the credit of the modern treatment; for in 1864 he showed that in fevers, especially typhus, the mortality is far less when the patients are supported with milk and not with alcohol. Quackery and humbug meet with but little mercy at the author's hands, and the hollowness of the pretensions of homœopathy is well brought out in an essay contributed thirty years ago, which is reprinted in this collection.

The volume should meet with a large circle of readers outside the medical profession, as it is eminently readable and touches upon many points in the past history of medicine as well as in modern practice, which are of interest to all.

Materials for a Flora of the Malayan Peninsula. Part I. By Dr. George King, F.R.S., Calcutta. Pp. 50. (Reprinted from the *Journal of the Asiatic Society of Bengal*, 1889, No. 4.)

SIR J. D. HOOKER'S "Flora of British India," of which five volumes out of seven are now printed, marks an era in tropical botany, inasmuch as it will probably contain descriptions, with their synonymy, of half the tropical plants of the Old World. It furnishes, therefore, a broad platform for his successors to build upon. It is not likely that within the bounds of India proper many new plants still remain to be described; but it is not so in the wonderfully rich flora of the Malay peninsula. During the last ten years large collections have been accumulated at Calcutta from this region, gathered mainly by Scortechini and other collectors who have been sent out by the authorities of the Calcutta Botanic Garden. In the present pamphlet, which is reprinted from the Journal of the Asiatic Society of Bengal, Dr. King, the Director of the Calcutta Garden, begins a synopsis of the plants which are indigenous to the British provinces of the Malay peninsula, including the islands of Singapore, Penang, and the Nicobar and Andaman groups.

In this present paper he deals with the orders Ranunculaceæ, Dilleniaceæ, Magnoliaceæ, Menispermaceæ, Nymphæaceæ, Capparideæ, and Violaceæ, leaving over the intricate and largely represented order Anonaceæ for another time. In these seven orders there are 35 Malayan genera and 90 species, of which 32 are here described for the first time. Amongst the novelties are included a Magnolia, a Manglietia, 3 Talaumas, an Illicium, 4 species of Capparis, and no less then 11 new Alsodeias. Besides the species here described for the first time, there are several others, known previously in Java and China, which are new to British India. It will be seen that the work will add materially to our knowledge of Indian plants, and it is to be hoped that Dr. King, in the midst of his multifarious official duties, may be able to go on with it quickly and steadily. It is hardly worth while, we think, in a series of papers of this kind, to take up space and time by recapitulating in detail the characters of the orders and genera, as, from the nature of the case, it is essentially a supplement to Hooker's "Flora of British India," in which they are already fully worked out.

J. G. B.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Panmixia.

SEEING that the whole structure of Prof. Weismann's theory is founded—both logically and historically—upon the doctrine of "panmixia," and seeing that in some important respects his statement of the doctrine appears to me demonstrably erroneous, I propose to supply a paper on the subject.

It will be remembered that the principal evidence on which Mr. Darwin relied to prove the inheritance of acquired characters was that which he derived from the apparently inherited effects of use and disuse—especially as regards the bones of our domesticated animals when compared with the corresponding bones of ancestral stocks in a state of nature. Now, in all his investigations regarding this matter, the increase or decrease of a part was estimated, not by directly comparing, say, the wing bones of a domesticated duck with the wing-bones of a wild duck, but by comparing the *ratio* between the wing and leg bones of a tame duck with the *ratio* between the wing and leg bones of a wild duck. Consequently, if there be any reason to doubt the supposition that a really inherited diminution of a part thus estimated is due to the inherited effects of diminished use, such a doubt will also require to extend to the evidence of a really inherited augmentation of a part being due to the inherited effects of augmented use. Now, there is the gravest possible doubt lying against the supposition that any really inherited decrease is due to the inherited effects of disuse. For it may be—and, at any rate to a large extent, must be—due to another principle which it is remarkably strange that Mr. Darwin should have overlooked. This is the principle of what Prof. Weismann has called panmixia. If any structure which was originally built up by natural selection on account of its use, ceases any longer to be of so much use, in whatever degree it so ceases to be of use, in that degree will the premium before set upon it by natural selection be withdrawn. And the consequence of this withdrawal of selection as regards that particular part will be to allow the part in a corresponding measure to degenerate through successive generations. Weismann calls this principle panmixia, because, by such withdrawal of natural selection from any particular part, promiscuous breeding ensues with regard to that part. And it is easy to see that this principle must be one of great importance in nature, inasmuch as it must necessarily come into operation in all cases where a structure or an instinct has ceased to be useful. It is likewise easy to see that its effects—viz. of inducing degeneration—must be precisely the same as those which were attributed by Mr. Darwin to the inherited effects of disuse; and, therefore, that most of the evidence on which he relied to prove the inherited effects both of use and of disuse is vitiated by the fact that the idea of panmixia never happened to occur to him. In this connection, however, it requires to be stated that the idea first of all occurred to myself, unfortunately just after the appearance of his last edition of the "Origin of Species." I then published in these columns a somewhat detailed exposition of the subject (see NATURE, vol. ix. pp. 361, 440, vol. x. p. 164). I called the principle the cessation of selection—which still seems to me a better, because a more descriptive, term than panmixia—and at first it appeared to me, as it now appears to Weismann, entirely to supersede the necessity of supposing that the effects of use and of disuse are ever inherited in any degree at all. Thus it obviously raised the whole question touching the admissibility of the Lamarckian principles in any case, or the question which is now being so much discussed concerning the possible inheritance of acquired as distinguished from congenital characters. But Mr. Darwin satisfied me that this larger question could not be raised. That is to say, although he fully accepted the principle of panmixia, and as fully acknowledged its obvious importance, he left no doubt in my mind that there was independent evidence for the transmission of acquired characters sufficient in amount to leave the general structure of his previous theory unaffected by what he nevertheless recognized as a necessarily additional factor in it. And forasmuch as no further facts bearing upon the subject have been forthcoming since that time, I see no reason to change the judgment that was then formed.

There is, however, one respect in which Prof. Weismann's statement of the principle of panmixia differs from that which was considered by Mr. Darwin; and it is this difference of statement—which amounts to an important difference of theory—that I now wish to discuss.

The difference in question is, that while Prof. Weismann believes the cessation of selection to be capable of inducing degeneration down to the almost complete disappearance of a rudimentary organ, I have argued that, *unless assisted by some other principle*, it can at most only reduce the degenerating organ to considerably above one-half its original size—or probably not through so much as one-quarter. The ground of this argument (which is given in detail in the NATURE articles before alluded to) is, that panmixia depends for its action upon fortuitous variations round an ever-diminishing average—the average thus diminishing because it is no longer *sustained* by natural selection. But although no longer sustained by *natural selection*, it does continue to be sustained by *heredity*; and therefore, as long as the force of heredity persists unimpaired, fortuitous variations alone—or variation which is no longer controlled by natural selection—cannot reduce the dwindling organ to so much as one-half of its original size; indeed, as above foreshadowed, the balance between the positive force of heredity and the negative effects of promiscuous variability will probably be arrived at considerably above the middle line thus indicated. Only if for any reason the force of heredity begins to fail, can the average round which the cessation of selection works become a progressively diminishing average. In other words, so long as the original force of heredity as regards the useless organ remains unimpaired, the mere withdrawal of selection cannot reduce the organ much below the level of efficiency above which it was previously *maintained* by the *presence* of selection. If we take this level to be 70 per cent. of the original size, cessation of selection will reduce the organ through the 30 per cent, and there leave it fluctuating about this average, unless for any reason the force of heredity begins to fail—in which case, of course, the average will progressively fall in proportion to the progressive weakening of this force.

Now, according to my views, the force of heredity under such circumstances is always bound to fail, and this for two reasons. In the first place, it must usually happen that when an organ becomes useless, natural selection as regards that organ will not only *cease*, but become *reversed*. For the organ is now absorbing nutriment, causing weight, occupying space, and so on, *uselessly*. Hence, even if it be not also a source of actual danger, "economy of growth" will determine a reversal of selection against an organ which is now not merely useless, but deleterious. And this degenerating influence of the reversal of selection will throughout be assisted by the cessation of selection, which will now be always acting round a continuously sinking average. Nevertheless, a point of balance will eventually be reached in this case, just as it was in the previous case where the cessation of selection was supposed to be working alone. For, where the reversal of selection has reduced the diminishing organ to so minute a size that its presence is no longer a source of detriment to the organism, the cessation of selection will carry the reduction a small degree further; and then the organ will remain as a "rudiment." And so it will remain permanently, unless there be some further reason why the still remaining force of heredity should be abolished. This further reason I found in the consideration that, however enduring we may suppose the force of heredity to be, it would be unreasonable to suppose that it is actually everlasting; and, therefore, that we may reasonably attribute the eventual disappearance of rudimentary organs to the eventual failure of heredity itself. In support of this view there is the fact that rudimentary organs, although very persistent, are not everlasting. That they should be very persistent is what we should expect, if the hold which heredity has upon them is great in proportion to the time during which they were originally useful, and so firmly stamped upon the organization by natural selection causing them to be strongly inherited in the first instance. Thus, for example, we might expect that it would be more difficult finally to eradicate the rudiment of a wing than the rudiment of a feather; and accordingly we find it a general rule that long-enduring rudiments are rudiments of organs distinctive of the higher taxonomic divisions—*i.e.* of organs which were longest in building up in the first place, and longest sustained in a state of working efficiency in the second place. Again, that rudimentary organs, although in such cases very persistent, should not be everlasting, is also what we should expect, unless (like Weismann) we have some argumentative reason to sustain the doctrine that the force of heredity is inexhaustible, so that never in any case can it become enfeebled by a mere lapse of time—a doctrine the validity of which in the present connection I will consider later on.

Thus, upon the whole, my view of the facts of degeneration remains the same as it was when first published in these columns sixteen years ago, and may be summarized as follows.

The cessation of selection when working alone (as it probably does work in our domesticated animals, and during the first centuries of its working upon structures or colours which do not entail any danger to, or perceptible drain upon the nutritive resources of, the organism) cannot cause degeneration below, probably, some 20 to 30 per cent. But if from the first the cessation of selection has been assisted by the *reversal* of selection (on account of the degenerating structure having originally been of a size sufficient to entail a perceptible drain on the nutritive resources of the organism, having now become a source of danger, and so forth), the two principles acting together will continue to reduce the ever-diminishing structure down to the point at which its presence is no longer a perceptible disadvantage to the species. When that point is reached, the reversal of selection will terminate, and the cessation of selection will not then be able of itself to reduce the organ through more than at most a very few further percentages of its original size. But, after this point has been reached, the now total absence of selection, either for or against the organ, will sooner or later entail this further and most important consequence—viz. a failure of heredity as regards the organ. So long as the organ was of use, its efficiency was constantly *maintained* by the *presence* of selection—which is merely another way of saying that selection was constantly maintaining the force of heredity as regards that organ. But as soon as the organ ceased to be of use, selection ceased to maintain the force of heredity; and thus, sooner or later, that force began to waver or fade. Now it is this wavering or fading of the force of heredity, thus originally due to the cessation of selection, that in turn *co-operates* with the still continued cessation of selection (panmixia) in reducing the structure below the level where its reduction was left by the actual reversal of selection. So that from that level downwards the cessation of selection and the consequent failing of heredity act and react in their common work of causing obsolescence. In the case of newly acquired characters the force of heredity will be less than in that of more anciently acquired characters; and thus we can understand the long endurance of "vestiges" characteristic of the higher taxonomic divisions, as compared with those characteristic of the lower. But in all cases, if time enough be allowed, under the cessation of selection the force of heredity will eventually fall to zero, when the hitherto obsolescent structure will finally become obsolete.[1]

Let us now turn to Weismann's view of degeneration. First of all, he has omitted to perceive that "panmixia" alone (if unassisted either by reversed selection or an inherent diminishing of the force of heredity) cannot reduce a functionless organ to the condition of a *rudiment*. Therefore he everywhere represents panmixia (or the mere *cessation* of selection) as of itself sufficient to cause degeneration, say from 100 to 5, instead of from 100 to 80 or 70, which, for the reasons above given, appeared (and still appears) to me about the most that this principle alone can accomplish, so long as the original force of heredity continues unimpaired. No doubt we have here what must be regarded as a mere oversight on the part of Prof. Weismann; but the oversight is rendered remarkable by the fact that he *does* invoke the aid of reversed selection *in order to explain the final disappearance of a rudiment*. Yet it is self-evident that the reversal of selection must be much more active during the initial than during the final stages of degeneration, seeing that, *ex hypothesi*, the greater the degree of reduction which has been attained the less must be the detriment arising from any useless expenditure of nutrition, &c.

And this leads me to a second oversight in Prof. Weismann's statement, which is of more importance than the first. For the

[1] It may not be needless to add that in the case of newly acquired and comparatively trivial characters, with regard to which reversal of selection is not likely to take place (*e.g.* slight differences of colour between allied species), cessation of selection is likely to be very soon assisted by a failure in the force of heredity; seeing that such newly acquired characters will not be so strongly inherited as are the more ancient characters distinctive of higher taxonomic groups.

place at which he does invoke the assistance of reversed selection is exactly the place at which reversed selection must necessarily have ceased to act. This place, as already explained, is where an obsolescent organ has become rudimentary, or, as above supposed, reduced to 5 per cent. of its original size; and the reason why he invokes the aid of reversed selection at this place is in order to save his doctrine of "the stability of germ-plasm." That the force of heredity should finally become exhausted if no longer *maintained* by the *presence* of selection, is what Darwin's theory of perishable gemmules would expect to be the case, while such a fact would be fatal to Weismann's theory of an imperishable germ-plasm. Therefore he seeks to explain the eventual failure of heredity (which is certainly a fact) by supposing that after the point at which the cessation of selection alone can no longer act (and which his first oversight has placed some 70 per cent. too low), the reversal of selection will begin to act directly against the force of heredity as regards the diminishing organ, until such direct action of reversed selection will have removed the organ altogether. Or, in his own words, "The complete disappearance of a rudimentary organ can only take place by the operation of natural selection; this principle will lead to its diminution, inasmuch as the disappearing structure takes the place and the nutriment of other useful and important organs." That is to say, the rudimentary organ finally disappears, not because the force of heredity is finally exhausted, but because natural selection has begun to utilize this force against the continuance of the organ—always picking out those congenital variations of the organ which are of smallest size, and thus, by its now *reversed* action, *reversing* the force of heredity as regards the organ.

Now, the oversight here is that the smaller the disappearing structure becomes, the less hold must "this principle" of reversed selection retain upon it. As above observed, during the earlier stages of reduction (or while co-operating with the cessation of selection) the reversal of selection will be at its *maximum* of efficiency; but, as the process of diminution continues, a point must eventually be reached at which the reversal of selection can no longer act. Take the original mass of a now obsolescent organ in relation to that of the entire organism of which it then formed a part to be represented by the ratio 1 : 100. For the sake of argument we may assume that the mass of the organism has throughout remained constant, and that by "mass" in both cases is meant capacity for absorbing nutriment, causing weight, occupying space, and so forth. Now, we may further assume that when the mass of the organ stood to that of its organism in the ratio of 1 : 100, natural selection was strongly reversed with respect to the organ. But when this ratio fell to 1 : 1000, the activity of such reversal must have become enormously diminished, even if it still continued to exercise any influence at all. For we must remember, on the one hand, that the reversal of selection can only act so long as the presence of a diminishing organ continues to be so injurious that variations in its size are matters of life and death in the struggle for existence; and, on the other hand, that natural selection in the case of the diminishing organ does not have reference to the presence and the absence of the organ, but only to such variations in its mass as any given generation may supply. Now, the process of reduction does not end even at 1 : 1000. It goes on to 1 : 10,000, and eventually 1 : ∞. Consequently, however great our faith in natural selection may be, a point must eventually come for all of us at which we can no longer believe that the reduction of an obsolescent organ is due to this cause. And I cannot doubt that if Prof. Weismann had sufficiently considered the matter, he would not have committed himself to the statement that "the complete disappearance of a rudimentary organ can only take place by the operation of natural selection."

According to my view of the matter, the complete disappearance of a rudimentary organ can only take place by the *cessation* of natural selection, which permits the eventual exhaustion of heredity, when heredity is thus simply left to itself. During all the earlier stages of reduction, the cessation of positive selection was assisted in its work by the activity of negative or reversed selection; but when the rudiment became too small for such assistance any longer to be supplied, the rudiment persisted in that greatly reduced condition until the force of heredity with regard to it was eventually worn out. This appears to me, as it appeared to me in 1874, the only reasonable conclusion that can be drawn from the facts. And it is because this conclusion is fatal to Prof. Weismann's doctrine of the permanent "stability" of germ-plasm, while quite in accordance with all theories which belong to the family of pangenesis, that I deem the facts of degeneration of great importance as tests between these rival interpretations of the facts of heredity. It is on this account that I have occupied so much space with the foregoing discussion; and I shall be glad to ascertain whether any of the followers of Prof. Weismann are able to controvert the views which I have thus re-published.

London, February 4. GEORGE J. ROMANES.

P.S.—Since the above article was sent in, Prof. Weismann has published in these columns (February 6) his reply to a criticism by Prof. Vines (October 24, 1889). In this reply he appears to have considerably modified his views on the theory of degeneration; for while in his essays he says (as in the passage above quoted) that "the complete disappearance of a rudimentary organ can only take place by the operation of natural selection"—*i.e.* only by the *reversal* of selection,—in his reply to Prof. Vines he says, "I believe that I have proved that organs no longer in use become rudimentary, and must finally disappear, solely by 'panmixia'; not through the direct action of disuse, but because natural selection no longer sustains their standard structure"—*i.e.* solely by the *cessation* of selection. Obviously, there is here a flat contradiction. If Prof. Weismann now believes that a rudimentary organ "must finally disappear *solely*" through the *withdrawal* of selection, he has abandoned his previous belief that "the complete disappearance of a rudimentary organ can *only* take place by the *operation* of selection." And this change of belief on his part is a matter of the highest importance to his system of theories as a whole, since it betokens a surrender of his doctrine of the "stability" of germ-plasm—or of the virtually everlasting persistence of the force of heredity, and the consequent necessity for a reversal of this force itself (by natural selection placing its premium on *minus* instead of on *plus* variations) in order that a rudimentary organ should finally disappear. In other words, it now seems he no longer believes that the force of heredity in one direction (that of sustaining a rudimentary organ) can only be abolished by the active influence of natural selection determining this force in the opposite direction (that of removing a rudimentary organ). It seems he now believes that the force of heredity, if merely left to itself by the withdrawal of natural selection altogether, will sooner or later become exhausted through the mere lapse of time. This, of course, is in all respects my own theory of the matter as originally published in these columns; but I do not see how it is to be reconciled with Prof. Weismann's doctrine of so high a degree of stability on the part of germ-plasm, that we must look to the Protozoa and the Protophyta for the original source of congenital variations as now exhibited by the Metazoa and Metaphyta. Nevertheless, and so far as the philosophy of degeneration is concerned, I shall be very glad if (as it now appears) Prof. Weismann's more recent contemplation has brought his principle of panmixia into exact coincidence with that of my cessation of selection.—G. J. R.

Newton in Perspective.

THE interesting modern science termed by the Germans *Geometrie der Lage*, and by the French and other Latin peoples *géométrie de position*, may be traced in germ to that part of Newton's "Principia" which deals with the construction of curves of the second order, and to what has survived in tradition of Pascal's lost manuscript entitled "Traité complet des Coniques." The more recent developments of this important subject cast much new light upon Newton's propositions, many of which we are now enabled to solve by easier and more direct methods. A noteworthy example is here fully worked out, in order to show how problems which Newton solved by indirect and circuitous processes may be solved more simply by the aid of modern graphics.

PROBLEM.—*Given the four tangents EA, AB, BC, CD* (*Fig.* 1), *as well as a point of contact; to construct the conic.*—First it will be necessary to give some faint idea of Newton's solution of this problem, without entering upon details which can be found in the Latin edition of the "Principia" edited by Sir William Thomson and Prof. H. Blackburn. Having expounded at great length a general theorem for the transformation of curves, Newton transforms the quadrilateral figure formed by the four tangents into a parallelogram. Then he joins the given point of contact *y*, transformed according to the same principle as the given four tangents, to the centre O of the parallelogram

—which is also the centre of the conic—and producing the line yO to y', so that Oy' may be equal to Oy, he determines a second point of contact y' on the conic, by which means the problem is reduced to the case dealt with in the preceding proposition, showing how to construct the curve when three tangents and two points are given. Having in this way found five points on the transformed conic, Newton next proceeds to retransform the whole of the figure to its original shape, in order to apply his well-known method of constructing a conic of which five points are known.

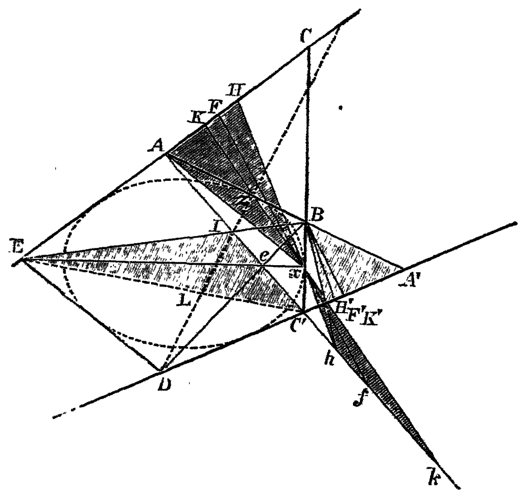


FIG. 1.

Now all these transformations and retransformations of lines and quadrangles involve very tedious and laborious operations, which can be avoided by borrowing a few simple principles of modern geometry. The following two original solutions of the above problem will serve to illustrate this statement.

SOLUTION.—*Case* I. *When the given point of contact x lies on one of the given four tangents.*—Assume the given point of contact x and the neighbouring apex B of the quadrangle as centres of projection, and the given tangent lines EA and C′D as punctuated lines. The meaning of the term "punctuated line," familiar to students of modern geometry, will appear in the sequel.

It will be seen that the fourth tangent AB cuts the first punctuated line EA in A and the second punctuated line C′D in A′. Now, according to a proposition of modern geometry, if the points A and A′, in which the tangent AB intersects the two punctuated tangents EA and C′D, be projected by rays xA and BA′ issuing from their respective centres of projection x and B, those rays will meet in a point A, situate on what is termed the perspective line of the pencils x and B.

Next imagine the tangent AB to revolve upon the curve so as gradually to approach the limiting position BC. In that case A will approach C, B will fall upon C′, and the intersection of the projecting rays xC and BC′ will coincide with C′, which is therefore a second point on AC′, the required perspective line of the pencils x and B. Wherefore, in order to find a fifth or any number of tangents to the curve, choose any point E on the punctuated line EA, and project this point from x, the corresponding centre of projection, upon the perspective line AC′ in e; and then project e from the second centre of projection B upon the corresponding punctuated line C′D in D. The line ED is a fifth tangent to the conic, and any number of tangents can be drawn in precisely the same way. Then, let F be any other point on EA. Join and produce Fx, intersecting the perspective line AC′ in f; and from the centre B project f upon the punctuated tangent C′D in F′. Then the line FF′ will be a sixth tangent to the conic.

COR. I.—Since the lines AC′, BD, and xE all meet in the same point e, it follows that, in any pentagon ABC′DE circumscribed to a conic, the opposite diagonals AC′ and BD and the line joining the fifth point E to the opposite point of contact x all meet in the same point.

Case II. *When the given point of contact z lies outside of the four tangents AEDC′B.*—By the corollary, Case I., if AB be the fifth tangent, it must pass through the given point of contact z in such a direction that the diagonals C′A and EB may intersect in a point I situate on a given line Dz.

Now let AB revolve about the fixed point of contact z as a fulcrum, whilst A and B describe the lines EC and CC′ (Figs. 1 and 2). Then, necessarily, z will be the centre of perspectivity of the punctuated lines EC and CC′, whose centres of projection are respectively C′ and E. But, by a well-known proposition of geometry of position, when the points of two converging punctuated lines, such as EC and CC′, are projected from opposite centres in this fashion, the locus of the successive intersections of the rays C′A and EB, or in other words the variable position of the point I, will describe a conic, which in the present instance is a hyperbola. But the problem is how to find the point I on the transversal Lz without constructing the hyperbola, four points on which are already known. For it will be observed that, when A coincides with E, the point B will lie on the prolongation of Ez, and the corresponding projecting rays Ez and C′E will meet in E, a point on the hyperbola. Similarly C′ is a second point on the hyperbola. Again, as AB continues to revolve about the fixed centre of perspectivity z, its intersections A and B with the punctuated lines EC and CC′ will ultimately coalesce in the point C, common to both those lines. Hence, since in that case the rays projecting the double point C from the centres E and C′ meet in C, this point must lie on the hyperbola.

Fourthly, if the line Cz be produced to intersect the line EC′ in N, it can be easily shown that i, the third point in the harmonic ratio GziN, is a fourth point on the hyperbola. A fifth point can be found by simply drawing AB in any direction traversing z and intersecting EC in A′ and CC′ in B′, and then projecting A′ and B′ from the centres C′ and E respectively by rays C′A′ and EB′ which will meet in a fifth point upon the hyperbola.

Thus, given these or in fact any five points EDiTH (Fig. 2)

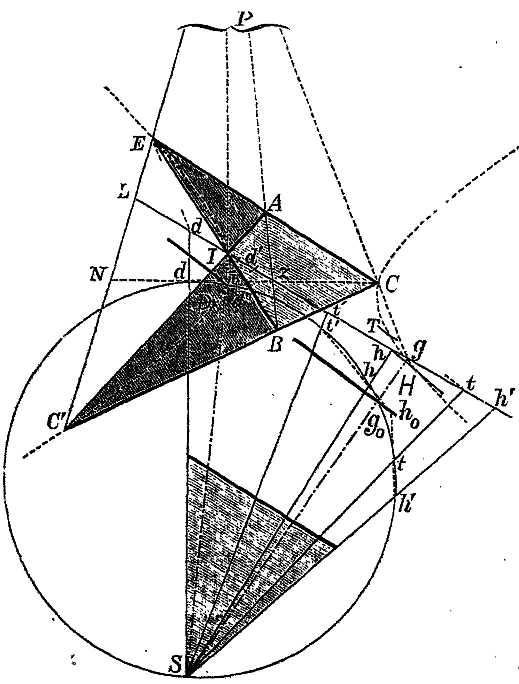


FIG. 2.

on the hyperbola, it is possible to find the point of intersection of the given transversal Lz with the hyperbola without constructing the curve. First describe any circle in the plane of the five points, choosing two of these, such as E and i, as centres of projection from which to project the remaining three points DHT upon the given transversal Lz in the points d,

and $d'h't'$ respectively. Then, from any point S on the circumference of the circle, reproject the six points dht, $d'h't'$, upon the same circumference in the points similarly lettered.

By means of this double projection from the centres E and i the points DHT have been transferred in duplicate from the hyperbola to the circle, or from one conic to another of a different species; and it is proved in treatises on modern geometry that points so transferred lose none of their projective properties. Hence the points dht and $d'h't'$ on the circumference of the circle are allied projective systems. Therefore, in order to find the perspective line common to both systems, choose one point t of the first set as the centre of projection of the second system; and make t', the correlative point of the second set, the centre of projection of the system dht.

From t project the points d' and h' by rays td' and th', and from t' project the correlative points d and h by rays $t'd$ and $t'h$. Then the correlative rays td' and $t'd$ will intersect in a point d_0 on the required perspective line; and the correlative rays th' and $t'h$ will meet in h_0, a second point on the same line. This perspective line d_0h_0 will intersect the circumference in two points i_0 and g_0 which, being joined to S and produced, will determine the double points I and g common to the hyperbola and transversal Lz. The complete quadrangle EC'IC shows that the harmonic ratios CziN and gzIL are segments of the same harmonic pencil P.

The lines Ez and C'z are tangents to the curve at E and C' respectively; and z is the pole of the polar EC' with respect to the hyperbola. The proofs of these last two deductions may be found in any good text-book on geometry of position.

ROBERT H. GRAHAM.

Thought and Breathing.

PROF. MAX MÜLLER'S article on thought and breathing, in your issue of February 6 (p. 317) has just come into my hands. In it he states that the power of retaining the breath is practised largely by Hindus as a means towards a higher object, viz. the abstraction of the organs of the human body from their natural functions. The same custom prevails amongst a certain sect of Mahometans also—the so-called Softas.

In 1878, when in the Central Provinces of India, I came across a native Christian—Softa Ali, as he was called—who had a history. His father had been a Cazi—or religious judge—and a wealthy man, who through scruples of conscience fell into disgrace with a certain native ruler, lost his all, and was banished. His son was, or became, a Softa, and after some years embraced Christianity from conviction, and at great cost to himself—for his wife and children would no longer consort with him. When describing to me the practices formerly enjoined upon him by his religion, this man stated that a Softa is required to draw in and retain his breath and respire it again in various manners. He did not give full details as to how this should be effected, but said that the object of this procedure was to worship with every organ of one's body—heart, lungs, &c., in turn. He added that this practice was a fruitful source of heart-disease.

The following year, when staying at Futtehpore Sikri, near Agra, I saw and heard a Mahometan, unknown to himself, make his evening devotions near the tomb of Suleem Chisti in the way above described; his movements, and the sounds he uttered, were most peculiar.

It has been often related, from well-attested evidence, that in the case of those who have been recovered from drowning, or of those who have been hung and cut down before life was extinct, a kind of automatic consciousness seems to be extraordinarily active in them at the time of their peril. It would appear that, as regards Hindu and Mahometan devotees, and the drowning or partially hung man, a kind of asphyxia is the result, and that, when sensation is almost gone, the intelligence acquires increased activity. In our ordinary life, if our minds are intently fixed upon a subject, we instinctively and involuntarily retain the breath.

When in Rajputana, and again when on the frontier of Chinese Tibet, I saw in each place a man who, to all appearance, seemed to have attained the power of perfect abstraction. In the former case, the villagers asserted that the devotee rose only once a week from his most uncomfortable and constrained position; in the second instance, the man—a most singular-looking person—remained absolutely immovable the whole day. Both seemed to be in a kind of cataleptic trance.

HARRIET G. M. MURRAY-AYNSLEY.

Former Glacial Periods.

I HAVE long felt convinced that geologists are being misled in reference to former glacial epochs by failing to give due thought to a consideration referred to on former occasions,[1] viz. that when the present surface of the globe has been disintegrated, washed into the sea, and transformed into rock, there will undoubtedly then be about as little evidence that there had been a glacial epoch during post-Tertiary times as there is at present that there was one during Miocene, Eocene, Permian, and other periods.

JAMES CROLL.

Perth, March 6.

AUSTRALASIAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

THE formation of this Association, mainly by the efforts of Prof. Liversidge, of Sydney University, and its first meeting in Sydney in August 1888, were noticed at the time in NATURE (vol. xxxviii. pp. 437, 623). One of the chief rules of the Association is that it shall meet in turn in the capital cities of the various colonies; and Melbourne was agreed upon as the second meeting-place. It was found inconvenient, however, to hold the Melbourne meeting during 1889, as should have happened in due course, for it is only after Christmas that all the Universities are simultaneously in vacation; and accordingly it was commenced on the 7th of January in the present year, and was continued through the following week. Some anxiety was felt as to the result of this choice of date, for there is always a risk in January of such continuous heat as would hinder the work and destroy the pleasure of the meeting; but the Association proved to be specially favoured in the matter of weather.

The following are the names of the officers of the Association and of the Sections. With regard to the latter, the rule obtains that Presidents are chosen from other colonies, while Vice-Presidents and Secretaries are chosen from the colony in which the meeting is held.

President, Baron von Mueller, K.C.M.G., F.R S.

Local Treasurer, R. L. J. Ellery, C.M.G., F.R.S.

General Secretaries: Prof. Archd. Liversidge, F.R.S., Permanent Hon. Secretary; Prof. W. Baldwin Spencer, Hon. Sec. for Victoria.

Assistant Secretary for Victoria, J. Steele Robertson.

Sectional Officers:—Section A (Astronomy, Mathematics, Physics, and Mechanics)—President, Prof. Threlfall, Sydney University. Vice-President, Prof. Lyle, Melbourne University. Secretaries: W. Sutherland, E. F. J. Love.

Section B (Chemistry and Mineralogy)—President, Prof. Rennie, Adelaide University. Vice-President, C. R. Blackett, Government Analyst, Melbourne. Secretary, Prof. Orme Masson, Melbourne University.

Section C (Geology and Palæontology—President, Prof. Hutton, Canterbury College, New Zealand. Vice-President, Prof. McCoy, C.M.G., F.R.S., Melbourne University. Secretary, James Sterling,

Section D (Biology)—President, Prof. A. P. Thomas, Auckland. Vice-Presidents: J. Bracebridge Wilson; P. H. MacGillivray. Secretaries: C. A. Topp, Arthur Dendy.

Section E (Geography)—President, W. H. Miskin, President of the Queensland Branch of the Royal Geographical Society of Australasia. Vice-Presidents: Commander Crawford Pasco, R.N.; A. C. Macdonald. Secretary, G. S. Griffiths.

Section F (Economic and Social Science and Statistics—President, R. M. Johnson, Registrar-General, Hobart. Vice-President, Prof. Elkington, Melbourne University. Secretaries: A. Sutherland, H. K. Rusden.

Section G (Anthropology)—President, Hon. J. Forrest, C.M.G., Commissioner for Crown Lands, Western

[1] Quart. Journ. Geol. Soc. for May 1889; "Climate and Time," p. 266.

Australia. Vice-President, A. W. Howitt, Secretary for Mines, Melbourne. Secretary, Rev. Lorimer Fison.

Section H (Sanitary Science and Hygiene)—President, Dr. J. Ashburton Thompson, Sydney. Vice-Presidents: A. P. Akehurst, President of the Central Board of Health, Melbourne; G. Gordon. Secretary, G. A. Syme.

Section I (Literature and Fine Arts)—President, Hon. J. W. Agnew, Hobart. Vice-Presidents: Prof. Tucker, Melbourne University (Literature Sub-Section); J. Hamilton Clarke (Music Sub-Section). Secretaries: Dr. Louis Henry (Music Sub-Section); Tennyson Smith (Literature Sub-Section).

Section J (Architecture and Engineering)—President, Prof. Warren, Sydney University. Vice-Presidents: A. Purchas, H. C. Mais. Secretary, A. O. Sachse.

All arrangements for the meeting were made by the Local Committee, of which Mr. R. L. J. Ellery, the Government Astronomer, was chairman, and Prof. W. Baldwin Spencer secretary. The greater share of the work devolved on Prof. Spencer, and to his indefatigable energy is mainly due the undoubted success of the meeting. The buildings and grounds of the University were placed at the service of the Association, and nothing could have been better than the accommodation thus afforded. A lecture theatre was set apart for each of the ten Sections; and, as these theatres are situated in different parts of the grounds, and some distance apart, they were all connected by telephone, so that the advent of each paper in any Section could be signalled in every other. The large Wilson Hall was used as a reception-room; and a luncheon-hall, smoking-rooms, reading- and writing-rooms, a press-room, &c., were also provided, as also a special post- and telegraph-office. An official journal of the proceedings was published each morning, and every member was supplied with a copy of a special hand-book compiled for the occasion, and containing the following chapters:—

(1) "History of Victoria," by Alexander Sutherland.
(2) "Geology of Melbourne," by G. S. Griffiths.
(3) "Aborigines of Victoria," by Lorimer Fison.
(4) "Zoology, Vertebrata," by A. H. S. Lucas.
(5) "Zoology, Invertebrata," by A. Dendy.
(6) "Entomology," by C. French, Government Entomologist.
(7) "Botany," by C. A. Topp.
(8) "Commerce and Manufactures," by W. H. Thodey.
(9) "Climate," by R. L. J. Ellery, C.M.G., F.R.S., Government Astronomer.

Over six hundred members, representing all parts of Australasia, were in actual attendance, the total membership roll numbering more than a thousand. Some hundred and fifty papers in all were set down for reading in the various Sections. All these figures show a large increase since the first meeting, and give gratifying evidence of the growing interest taken in science throughout the colonies; further proofs of which are to be found in the facts that the Government of Victoria voted the liberal sum of £1000 towards defraying the expenses of the meeting, and that the entertainments provided by the hospitality of prominent citizens were numerous and on a most sumptuous scale. Many visits to places of scientific interest were also arranged for—short afternoon excursions for those who might not care for continuous Sectional work, and longer excursions at the conclusion of the meeting, under special leaders, to the Australian Alps, the Black Spur and Marysville, Gippsland Lakes, Ferntree Gully, Ballarat, and Sandhurst, all of which proved highly successful.

At the opening meeting in the Town Hall—presided over by His Excellency the Governor, the Earl of Hopetoun—the President, Baron Sir Ferdinand von Mueller, delivered his address, after being introduced by his predecessor in office, Mr. Russell, the Government Astronomer of New South Wales. Baron von Mueller undoubtedly stands at the head of the scientific workers in Australia. He has been a colonist since 1848, and since 1852 has held the position of Government Botanist in Victoria. His fame, which is based not only on the immense amount of work he has done in his special subject, the botany of Australia, but on his early achievements as an explorer, may be indicated in the words used by Mr. Russell:—"In 1861 he was made a Fellow of the Royal Society; he received from Her Majesty the Queen the Knight Companionship of St. Michael and St. George; was made a Commander of the Orders of St. Iago of Portugal, of Isabella of Spain, and of Philip of Hesse; was created hereditary Baron by the King of Würtemberg in 1871; and is honorary or corresponding member of a hundred and fifty learned societies." To this enumeration may be added what is, perhaps, the most honourable award of all—that of a Royal Medal by the Royal Society at the end of 1888. Throughout the colonies "the Baron" is known: a unique personality, not always wholly understood, but always recognized as a proud possession. His address, therefore, was listened to with peculiar interest, and perhaps all the more so that he did not confine himself to any special branch, but dealt generally with the past and future of Australasian science.

The Presidents of Sections also, in many cases, chose for their addresses subjects of particular interest in Australia. Prof. Rennie spoke of the work that has been done in the investigation of the chemistry of native plants and minerals, and made suggestions as to how this work may in future be encouraged and facilitated. Prof. Thomas discussed the problems here awaiting the biologist, and the local desiderata in scientific education. Mr. Miskin spoke principally of exploration in Australia and New Guinea, and of the importance to the colonies of Antarctic exploration; but he also discussed the chief geographical work now being done in other parts of the world. Mr. Forrest's address dealt with the present condition of the Australian aboriginal races. Dr. Ashburton Thompson discussed the sanitary organizations of Victoria and New South Wales, and the modes of obtaining and interpreting health statistics. Prof. Warren spoke of the education of engineers, with special reference to the local conditions and requirements. Dr. Agnew reviewed the literature and art of Australia. In the other Sections the Presidents chose subjects that do not owe their interest to local colour. Prof. Threlfall gave an account of the present state of electrical knowledge; Prof. Hutton's address was on the oscillations of the earth's surface; and Mr. Johnston spoke generally of current social and economic problems. A large proportion of the papers read by members in the various Sections were also Australian in their character. This was specially the case in the Sections of Geology and Anthropology; where, perhaps, the most valuable original work was communicated. As the Transactions will soon be published, the individual papers need not now be noticed; but reference may be made to the work done in the form of reports from Committees appointed at the previous meeting. The most bulky and perhaps the most valuable of these reports is that by a Committee which undertook, with Prof. Liversidge as its secretary, to prepare a census of the known minerals of the Australasian colonies. It disposes of New South Wales (only such information being given as was required to supplement Prof. Liversidge's published work), Queensland, and New Zealand. The portions dealing with Victoria and Tasmania are in process of completion; and, the Committee having been re-appointed, it is hoped that by next year the whole census will be complete. The publication will probably be delayed till then, and it will if possible take the form of a separate volume. A very important recommendation was made by another Committee (Prof. Haswell, of Sydney, secretary), which when

it is carried out will do much for biological research, viz. that steps be taken to establish and endow a central biological station at Port Jackson. Among the other reports may be mentioned one on the Polynesian races and Polynesian bibliography.

At the final meeting of the General Committee of the Association new special Committees were appointed to investigate and report on the following subjects: wheat rust, the manner of laying out towns, the preparation of geological maps, the arrangement of museums, the fertilization of the fig, Australian tides, and the present state of knowledge with regard to Australasian palæontology. A Committee was also appointed to formulate a scheme for obtaining practical assistance from the various Colonial Governments in the collection of material for research—chemical, geological, or biological. Other special Committees were appointed for the publication of the Transactions and for the revision of the laws of the Association.

The next meeting is to be held in Christchurch, New Zealand, probably in January 1891; and Sir James Hector has been elected President, and Prof. Hutton, Secretary. It has also been decided to hold the fourth meeting in Hobart, Tasmania, so that the Association will not again meet on the mainland for three years. To adventure so far as Christchurch is somewhat bold in so young an Association; but the success of the Melbourne meeting has demonstrated its usefulness and popularity, and warrants the belief that many will cross the water next year. There is even a strong hope felt by some that the occasion and the place may tempt a few of the members of the parent British Association to make the longer voyage from home, and see for themselves what is being done and what waits to be done for science at the antipodes.

ORME MASSON.

METEOROLOGICAL REPORT OF THE "CHALLENGER" EXPEDITION.[1]

PREVIOUS to 1872, discussions of the fundamental problems of meteorology relating to diurnal changes in atmospheric pressure, temperature, humidity, wind, and other phenomena, may be regarded as restricted to observations made on land. It had then, however, become evident that data from observations made on land only, which occupies about a fourth part of the earth's surface, were quite inadequate to a right conception and explanation of meteorologieal phenomena; and hence, when the *Challenger* Expedition was fitted out, arrangements were made for taking, during the cruise, hourly or two-hourly observations. These observations were published in detail in the "Narrative of the Cruise," Vol. II. pp. 305–74, and are still by far the most complete yet made on the meteorology of the ocean.

Elaborate observations were likewise made on deep-sea temperatures, which were at once recognized as leading to results of the first importance in terrestrial physics, and opening for discussion the broad question of oceanic circulation, on a sound basis of authentic facts. Preliminary, however, to any such inquiry, a full discussion of atmospheric phenomena was essential, requiring for its proper handling maps showing the mean temperature, mean pressure, and prevailing winds of the globe for each month of the year, with tables giving the data from which the maps are constructed. In other words, what was required was an exhaustive revision and ratification of Dove's isothermals, 1852; Buchan's isobars and prevailing winds, 1869; and Coffin's winds of the globe, 1875.

[1] "Report of the Scientific Results of the Voyage of H.M.S. *Challenger* during the Years 1873–76." Prepared under the superintendence of John Murray, LL.D. "Physics and Chemistry," Vol. II., Part V. "Report on Atmospheric Circulation." By Alexander Buchan, M.A., LL.D.

The work was entrusted to Mr. Buchan, of the Scottish Meteorological Society, in 1883, and was published in the beginning of this year. In addition to the tables of the appendices, giving the results of the *Challenger* observations, the more important are those giving the mean diurnal variation of atmospheric pressure at 147 stations in all parts of the world; the mean monthly and annual pressure at 1366 stations; a similar table of temperatures at 1620 stations; and the mean monthly and annual direction of the wind at 746 stations. It is believed that these tables include all the information at present existing that is required in the discussion of the broad questions raised in the Report, which includes, with the exception of the rainfall, all the important elements of the climates of the globe.

The Report itself is divided into two parts, the first dealing with diurnal, and the second with monthly, annual, and recurring phenomena. This is the first attempt yet made to deal with the diurnal phenomena of meteorology over the ocean—the temperature, pressure, and movements of the atmosphere, together with such phenomena as squalls, precipitation, lightning, and thunderstorms.

In equatorial and subtropical regions, the mean temperature of the surface of the sea falls to the daily minimum from 4 to 6 a.m., and rises to the maximum from 2 to 4 p.m., the amount of the diurnal variation being only 0°·9 F. In the higher latitudes of the Antarctic Ocean, the diurnal variation was only 0°·2. Of the four great oceans, the greatest variation was 1°·0 in the North Pacific, and the least 0°·8 in the Atlantic. This small daily variation of the temperature of the surface of the sea, shown by the *Challenger* observations, is an important contribution to physical science, being in fact one of the prime factors in meteorology, particularly in its bearings on the daily variations of atmospheric pressure and winds. The diurnal phases of the temperature of the air over the open sea occur at the same times as those of the temperature of the surface, but the amount of the variation is about 3°·0, and when near land the amount rises to 4°·4. The greater variation of the temperature of the air, as compared with that of the surface of the sea on which it rests, is a point of much interest from the important bearings of the subject on the relations of the air, and its aqueous vapour in its gaseous, liquid, and solid states, and the particles of dust everywhere present, to solar and terrestrial radiation. Thus the air rises daily to a higher and falls to a lower temperature than does the surface of the sea on which it rests.

The diurnal variation in the elastic force of vapour in the air is seen in its amplest form over the open sea, the results giving a curve closely coincident with the diurnal curve of temperature. But near land, the elastic force instead of rising towards, and to, the daily maximum at noon and 2 p.m., shows a well-marked depression at these hours, and indicates no longer merely a single, but a double maxima and minima. In other words, the curve now assumes the characteristics of this vapour curve as observed at all land stations, or where during the warmest hours of the day ascending currents rise from the earth's surface, and down-currents of drier air take their place. An important point specially to be noted here is that over the open sea, hygrometric observations disprove the existence of any ascending current from the surface of the sea during the hours when temperature is highest. On the other hand, the curve of relative humidity is simply inverse to that of the temperature, falling to the minimum at 2 p.m. and rising to the maximum early in the morning.

As regards the diurnal variation of the barometer, it is shown that the special forms of the monthly curves are, in their relations to the sun, direct and not cumulative as is the case with most of the monthly mean results of

meteorology. The movement of the daily barometric oscillations from east to west is only quasi-tidal, being quite different from the manner in which the tides of the ocean are propagated from place to place over the earth's surface; these oscillations being, undoubtedly, directly generated by solar and terrestrial radiation in the regions where they occur, and it is thus only that the striking variations in the curves of restricted districts comparatively near each other are to be explained. These peculiarities do not occur over the open sea.

As illustrating these variations, reference is made to the retardation of the time of occurrence of the morning maximum, which is delayed as the year advances, the latest retardation being in June; and the curves of 14 stations are given, these stations being situated in the middle and higher latitudes, and in localities which, while strongly insular in character, are at the same time not far from extensive tracts of land to eastward or south-eastward. These barometric curves for June present a graduated series, the two extremes being Culloden, where the morning maximum occurs at 7 a.m., and Sitka, where the same phase of pressure is delayed till 3 p.m., there being thus eight hours between them. Another set of curves is given from lower latitudes, showing the diurnal variation in mid-ocean from the *Challenger* observations, together with a series of land stations representing the influence of a land surface in increasing the amount of the variation, which reaches the maximum in the driest climates. Latitude for latitude, the maximum daily variation occurs in such arid climates as Jacobabad on the Indus, and the minimum over the anticyclonic regions of the great oceans. At Jacobabad the variation from the morning maximum to the afternoon minimum reaches 0·187 inch, whereas in the South Pacific it is 0·036 inch, and in the North Atlantic only 0·014 inch.

The following are some of the other types of barometric curves discussed—the curves at high-level stations on true peaks, and down the sides of the mountain; the curves in deep contracted valleys; those in high latitudes in the interior of continents where the morning minimum disappears; and those in high latitudes over the ocean where the afternoon minimum disappears. In the two last cases, the curve is reduced to a single maximum and minimum, which as regards the times of occurrence are the reverse of each other.

The atmosphere over the open sea rests on a floor or surface, subject to a diurnal range of temperature so small as to render that temperature practically constant both night and day; but notwithstanding this, the diurnal oscillations of the barometer occur over the open sea, equally as over the land surfaces of the globe. Hence the vitally important conclusion is drawn that the diurnal oscillations of the barometer are not caused by the heating and cooling of the earth's surface by solar and terrestrial radiation and by the effects following these diurnal changes in the temperature of the surface, but that they are primarily caused by the direct heating by solar radiation and cooling by terrestrial radiation of the molecules of the air and of its aqueous vapour, and the changes consequent on that cooling. It follows that these changes of temperature are instantly communicated through the whole atmosphere, from its lowermost stratum resting on the surface to the extreme limit of the atmosphere. There are important modifications of the barometric curves affecting the amplitude and times of occurrence of the principal phases of the phenomena, over land surfaces, for example, which are superheated during the day and cooled during the night according to the amount of aqueous vapour present in the atmosphere. But it is particularly insisted on that the barometric oscillations themselves are independent of any change in the temperature of the floor of the earth's surface on which the atmosphere rests. It scarcely requires to be added that these results of observation will necessitate the revision of all theories of the diurnal oscillations of the barometer that have assumed a diurnal change of the temperature of the surface on which the atmosphere rests as a necessary cause of these oscillations. The theory of the diurnal oscillations of the barometer submitted by Mr. Buchan may be thus stated: Assuming that aqueous vapour, in its purely gaseous state, is as diathermanous as the dry air of the atmosphere, it is considered that the *morning minimum* of pressure is due to a reduction of tension brought about by a comparatively sudden lowering of the temperature of the air itself by terrestrial radiation through all its height, and by a change of state of a portion of the aqueous vapour from the gaseous to the liquid state by its deposition on the dust particles of the air. The morning minimum is thus due, not to any removal of the mass of air overhead, but to a reduction of the tension by a lowering of the temperature and change of state of a portion of the aqueous vapour.

As the heating of the air proceeds with the ascent of the sun, evaporation takes place from the moist surfaces of the dust particles, and tension is increased by the simple change from the fluid to the gaseous state; and as the dust particles in the sun's rays rise in temperature above that of the air-films in contact with them, the temperature of the air is thereby increased, and with it the tension. Under these conditions the barometer steadily rises with the increasing tension to the *morning maximum;* and it is to be noted that the rise of the barometer is not occasioned by any accessions to the mass of air overhead, but only to increasing temperature of the air itself and change of state of a portion of its aqueous vapour.

By and by an ascending current of the warm air sets in, and pressure gradually falls as the mass of air overhead is reduced by the ascending current flowing back as an upper current to eastward—in other words, over the section of the atmosphere to eastward whose temperature has now fallen considerably lower than that of the region from which the ascending current is rising; and this continues till pressure falls to the *afternoon minimum.*

The back flow to eastward of the current, which has ascended from the longitudes where pressure at the time is at the minimum, increases pressure over the longitudes where temperature is now rapidly falling, and this atmospheric quasi-tidal movement brings about the *evening maximum* of pressure, which occurs from 9 p.m. to midnight according to latitude and geographical position. As the early hours of morning advance these contributions through the upper currents become less and less, and finally cease, and the effects of terrestrial radiation now going forward again introduce the morning minimum as already described. It is during the evening maximum that the diurnal maximum of periods of lightning without thunder and of the aurora take place, it being during this phase of the pressure that the atmospheric conditions result in an abundant increase of ice spicules in the upper regions of the atmosphere, which thus serve as a screen for the better presentation of any magneto-electric discharges that may occur.

It is interesting to note, in this connection, that the amount of the diurnal barometric tide falls conspicuously to the minimum, latitude for latitude, within the anticyclonic regions of the great oceans, where, owing to the descending currents which there prevail, deposition from the aqueous vapour is less abundant on the dust particles.

From a discussion of the whole of the two-hourly observations of the wind made during the cruise, sorted into those made over the open sea and those made near land, it is shown that the velocity of the wind is greater over the open sea than at or near land, the difference being from 4 to 5 miles per hour. The most important result is that there is practically no diurnal variation in the wind's velocity over the open sea. But as respects

the winds observed near land, the velocity at the different hours of the day gives a curve as clearly and decidedly marked as that of the temperature, the minimum occurring from 2 to 4 a.m., and the maximum from noon to 4 p.m., the absolute maximum being at 2 p.m. The difference between the hour of least and that of greatest velocity is for the Southern Ocean 6½ miles; South Pacific, 4½ miles; South Atlantic, 3¼ miles; and North and South Atlantic, each 3 miles. It is also to be noted that even the maximum of the day near land in the case of none of the oceans attains to the velocity observed over the open sea. The curve near land is substantially the same as the curves characteristic of stations on land. Thus, over the sea, where surface temperature is practically a constant day and night, the velocity of the wind shows no diurnal variation; whereas over land, and also near it, where the temperature of the surface is subject to a diurnal variation, the wind's velocity is also subject to an equally well-marked diurnal variation. On the other hand, at high-level observatories situated on true peaks, the maximum velocity occurs during the night, and the minimum during the day. In deep valleys in mountainous regions, an abnormally high barometer obtains during the night, which is the result of cold currents from the adjoining slopes that the cooling effects of terrestrial radiation set in motion. Now since these down-flowing winds must be fed from higher levels than those of the mountain itself, the winds prevailing on their tops are really the winds of a higher level, and blow therefore with the increased velocity due to that greater height. On the other hand, during the warmer hours of the day, the barometric pressure in deep valleys is abnormally low, owing to the superheating of these valleys as contrasted with the temperature of the surrounding region, thus giving rise to a warm wind blowing up the valleys, and an ascending current close to the sides of the mountain up to the summit. Now, since no inconsiderable portion of this ascending current, whose horizontal velocity is necessarily much retarded, mingles with the air-current proper to the level of the peak, the wind on the peak is retarded, and falls to the minimum of the day when the temperature is highest.

The results of the averaging of the squalls over the open sea entered in the *Challenger's* log show a strongly marked diurnal maximum early in the morning, when the effects of terrestrial radiation are at the maximum. But over land the diurnal curves for whirlwinds, tornadoes, and allied phenomena, show the minimum at these hours, and the maximum at the hours when insolation is strongest. It is probable that the daily maximum occurs in each case at those hours when temperature decreases with height at a greatly more rapid rate than the normal.

The distribution during the day of thunderstorms, and of lightning without thunder, is very remarkable. During the cruise 26 thunderstorms occurred over the open sea, of which 22 occurred during the 10 hours from 10 p.m. to 8 a.m., and only 4 during the other 14 hours of the day. Hence, over the open sea, the diurnal curve of thunderstorms is precisely the reverse of what obtains on land. Of the 209 reported cases of lightning without thunder, 188 occurred during the 10 hours from 6 p.m. to 4 a.m., and only 21 during the other 14 hours of the day. The following are the hours of the maxima of these phenomena in the warmer months over land and the open sea respectively. Thunderstorms over land, 2 to 6 p.m.; lightning over land, 8 p.m. to midnight; lightning over the open sea, 8 p.m. to 4 a.m.; and thunderstorms over the open sea, 10 p.m. to 8 a.m. These facts are a valuable contribution to the science, from their intimate connection with the ascending and descending currents of the atmosphere.

The second part of the Report, dealing with the monthly and annual phenomena, aims at giving a comparative view of the climatologies of the globe to a degree of completeness not previously attempted. The distribution of the temperature and pressure of the atmosphere and prevailing winds is illustrated by 52 newly constructed maps, of which 26 show by isothermals the mean monthly and annual temperature on hypsobathymetric maps, first on Gall's projection, and second on north circumpolar maps on equal surface projection; and 26 show, by isobars, for each month and for the year, the mean pressure of the atmosphere, with the gravity correction to lat. 45° applied, and by arrows the prevailing winds of the globe. Two other maps are given in the text, one showing for July the geographical distribution of the amount of the barometric oscillation from the morning maximum to the afternoon minimum; and the other, the annual range of the mean monthly pressure, which, in a sense, may be regarded as indicating the relative stability of the atmospheric pressure in different regions of the earth.

For the details of this discussion, we must refer to the Report itself, the broad results of which Mr. Buchan thus summarizes:—

"The isobaric maps show, in the clearest and most conclusive manner, that the distribution of the pressure of the earth's atmosphere is determined by the geographical distribution of land and water in their relations to the varying heat of the sun through the months of the year; and since the relative pressure determines the direction and force of the prevailing winds, and these in their turn the temperature, moisture, rainfall, and in a very great degree the surface currents of the ocean, it is evident that there is here a principle applicable not merely to the present state of the earth, but also to different distributions of land and water in past times. In truth, it is only by the aid of this principle that any rational attempt, based on causes having a purely terrestrial origin, can be made in explanation of those glacial and warm geological epochs through which the climates of Great Britain and other countries have passed. Hence the geologist must familiarize himself with the nature of those climatic changes which necessarily result from different distributions of land and water, especially those changes which influence most powerfully the life of the globe."

It is evident from what has been said that many of the results of the diurnal and seasonal phenomena of ocean meteorology are equally novel and important, and, when combined with the analogous results obtained from land observations, enable us to take a more intelligent and comprehensive grasp of atmospheric phenomena in their relations to the terraqueous globe taken as a whole than has hitherto been possible.

THE BOTANICAL LABORATORY IN THE ROYAL GARDENS, PERADENIYA, CEYLON.

THE attention of the readers of NATURE has been drawn more than once (vol. xxxi. p. 460, vol. xxxiv. p. 127) to the opportunities which are before botanists for the study of plants other than those of our own flora. But since the latter of these articles appeared, a step has been taken which will justify a return once more to this important subject.

It is certainly one of the most healthy signs of the present time that our younger botanists desire not merely to pore over minute details of microscopical structure in the laboratory at home, but to become personally acquainted with plants in the open. When the somewhat sudden reversion occurred some fifteen years ago, from taxonomy as an academic study, to the more detailed examination of the tissues of plants in the laboratory, and the study of their functions, those who took a large view of the progress of the science must have seen with regret that the change, however valuable in itself, brought with it a new danger. Those who as students were first introduced to plants as subjects of microscopic study ran

the risk of failing to appreciate the importance of external form: they acquired a knowledge of the minute structural details of certain plants, but did not acquire a strong grasp of the external characters of plants as a whole. But the pendulum which thus swung rapidly over to an extreme position is now returning to the mean. While duly appreciating the value of microscopic examination, the younger botanists are awake to the advantage, or even the necessity, of a wide knowledge of plants. The whole area of facts upon which those who are now engaged in teaching draw in the course of their lectures is much wider than it was ten years ago, and the extension has, perhaps, been most marked in the province of external morphology.

This being so, there will be no need to press upon the men who are starting upon a career as botanists the importance of a visit to the tropics: they will look upon the collections in our Botanic Gardens, which they are hardly allowed to touch, as only a temporary substitute for a tropical jungle, where they may cut down plants as they please, in order to obtain specimens illustrating mature or developmental characters. Moreover, those characters of a tropical flora which are the most striking and characteristic are often those which must remain entirely unrepresented in our glass houses at home. An expedition to the tropics should, in fact, become a recognized item in the programme of preparation for a career as a teacher of botany.

The advantages offered by the Royal Gardens at Peradeniya have already been pointed out in NATURE (vol. xxxiv. p. 127); but since that article was written steps have been taken by a Committee of the British Association to add to them. Backed by a grant of money, they have undertaken the establishment of a permanent laboratory in which visitors may carry on their work. A room has been set apart for this purpose in the official bungalow by the directorate of the Royal Garden. It has every advantage of position, being placed centrally in the garden, and within easy reach of the herbarium, &c.; while, since it is under the same roof as the Director's office, visitors would have the great advantage of the presence of Dr. Trimen himself as a referee in recognition of the plants of the rich native flora. In this room are to be found such apparatus and reagents as are ordinarily required for laboratory work, and steps are being taken to add other facilities.

The mere mention of these facts will probably suffice to attract those who were not previously aware of them. The chief deterrent will be the cost of the journey. It has already been stated that £200 to £250 will suffice for all expenses of an expedition of six months' duration, while if two club together the individual cost would be considerably smaller. Though the Committee of the British Association have no power to use the money entrusted to them as a personal grant, still it is well known that there are sources from which such grants may be obtained in order to assist those who are engaged on a definite line of research. Bearing all these facts in mind, the value of such an expedition as that to Peradeniya cannot be too strongly urged on those who are about to enter definitely on a career as professed botanists. The widening of view, and opportunity for research, which any man of originality would obtain by it would amply repay him for his expenditure of time and money. Applications for the use of the laboratory, which is at present vacant, should be made to Prof. Bower (University, Glasgow), who is the secretary to the Committee.

THE ASTRONOMICAL OBSERVATORY OF HARVARD COLLEGE.

PROF. EDWARD C. PICKERING has presented to the Visiting Committee the forty-fourth Annual Report of the Director of the Astronomical Observatory of Harvard College. The following are the more important passages:—

Henry Draper Memorial.—The first research on the spectrum of over ten thousand of the brighter stars is now nearly completed and is partially in print. The photographs required for the second research on the spectrum of the fainter stars are also nearly complete. The eleven-inch telescope has been in constant use throughout nearly every clear night in photographing the spectrum of the brighter stars. This work is approaching completion for all stars bright enough to be photographed by means of our present appliances, with the large dispersion now employed. Good progress has also been made with the classification of the spectra, and the study of the slight differences in different stars. By the use of an improved process for staining plates with erythrosin, the yellow and green portions of the spectrum, even of the fainter stars, can be advantageously studied. Numerous experiments have been made with a device for measuring the approach and recession of stars, by means of an achromatic prism in front of the object-glass. Several peculiar spectra have been studied, especially that of ζ Ursæ Majoris. The periodic doubling of its lines seems to be due to the rotation of two components too close to be distinguished by direct observation. The detection of bright lines in one of the stars in the Pleiades suggests a possible explanation of the legend that seven stars were formerly visible in this group.

During last spring an expedition was sent to Peru in charge of Mr. S. I. Bailey, assisted by Mr. M. H. Bailey. A station was selected on a mountain about six thousand feet high and about eight miles from Chosica. All supplies for the station, including water, must be carried by mules for this distance. Two frame buildings covered with paper have been erected, one for an observatory, the other for a dwelling-house. Since May 9 the Bache telescope has been kept at work during the whole of every clear night. 1236 photographs have been obtained. The plan proposed will cover the sky south of $-15°$ four times, once with photographs of spectra having an exposure of an hour, which will include stars to about the eighth magnitude; secondly, with an exposure of ten minutes, giving the brighter stars; thirdly, with charts having an exposure of one hour, permitting a map of the southern stars to the fourteenth magnitude inclusive; and fourthly, with charts having an exposure of ten minutes, including stars to about the tenth magnitude. The weather for the first four or five months was excellent, being clear nearly every evening. Fogs and cloud which often covered the adjacent valleys and the city of Lima did not reach to the top of the mountain. The cloudy season is now beginning and the work will be more interrupted. But nearly one-half of the entire programme has already been carried out. A large number of interesting objects have been detected, among others several stars having bright lines in their spectra. Including the photometric work described below, the amount of material so far collected is unexpectedly large.

Boyden Fund.—The climate of Southern California seems especially favourable to the undertaking desired by Mr. Boyden. An expedition under the direction of Prof. William H. Pickering was accordingly sent in November 1888 to the summit of Wilson's Peak, in the vicinity of Los Angeles. In order that as much useful work as possible might be accomplished, the thirteen-inch telescope and the eight-inch telescope now in Peru were sent to Willows, California, where the total solar eclipse of January 1, 1889, was successfully observed. Forty-seven photographs were obtained by the party during the three minutes of totality, and the instrumental equipment was much superior to any previously used for such a purpose. It was not until May 11, that the large telescope was successfully mounted on Wilson's Peak, by Messrs. E. S. King and Robert Black, but since then it has been kept

at work throughout every clear night. The number of photographs obtained is 1155. The objects photographed are selected from a list of 625 double stars, 143 clusters and other celestial bodies, such as the moon and planets. As these same objects have been repeatedly photographed at Cambridge with the same instrument, an accurate comparison of the atmospheric conditions of the two places may be made. It will of course be impossible to derive a final conclusion until the observations have extended over at least a year, but the evidence already secured shows that in summer results can be obtained at Wilson's Peak which cannot be obtained here. The difference is very pronounced for such objects as the markings on Jupiter. Clusters like that in Hercules are well resolved, so that the individual stars are easily measured, which cannot be done with the best Cambridge photographs. As a test-object the sixth star in the trapezium of the Orion nebula is clearly photographed for the first time. A new variable star has been discovered in the midst of the cluster G. C. 3636. A beginning has been made of the measurements of the position and brightness of the double stars, and it is hoped to extend this work to the clusters, and thus furnish an extensive addition to this department of micrometic astronomy.

Much experimental work has also been done at Cambridge, as is shown by the fact that nearly a thousand photographs have also been taken there. Moreover, the expedition to Peru is largely supported by the Boyden Fund. The meridian photometer will be used to extend two large series of observations to the south pole. These are the "Harvard Photometry," and the zones used in the revision of the *Durchmusterung*. This work will furnish photometric magnitudes of stars as bright as the ninth magnitude in all parts of the sky. The Messrs. Bailey have observed 67 series, one of them including 293 stars. In all, during less than six months, about 6700 stars have been observed, which have required 26,800 settings.

The Bruce Photographic Telescope.—For the last six years experiments have been in progress here on the use of a photographic doublet in the preparation of maps of the stars. The eight-inch telescope now in Peru is of this form and was mounted here in 1885. Since then 4500 photographs have been taken with it. With an exposure of an hour twice as many stars can be photographed as are visible with a telescope having an aperture of fifteen inches, and as many stars as can be photographed in the same time with a telescope of the usual form having an aperture of thirteen inches. Moreover with a doublet a portion of the sky covering twenty-five square degrees can be photographed with good definition, while only three or four degrees can be covered equally well with telescopes of the usual form. The time required to photograph the entire sky will be reduced in the same proportion. With a doublet each hemisphere could be covered in one year with eight hundred plates. In 1885 it was proposed to photograph the entire sky with the eight-inch telescope, enlarging the plates three times. The results would resemble in scale and size the charts of Peters and Chacornac. The generous aid of Miss Bruce mentioned above will permit this result to be attained in the original photographs, without enlargement. A contract has been made with Messrs. Alvan Clark and Sons for a telescope having an aperture of twenty-four inches and a focal length of eleven feet. Meanwhile nineteen foreign Observatories have united in an Astrophotographic Congress to prepare a map of the stars to the fourteenth magnitude with telescopes of the usual form having apertures of thirteen inches. The plans have been matured with great care and skill. The courteous reference to the Bruce telescope and its proposed work by Admiral Mouchez shows that both plans can be carried out without disadvantageous duplication. Doubtless each plan will possess certain advantages over the other. The Bruce telescope will be especially adapted to studying the very faint stars. It is hoped that those of the sixteenth magnitude and fainter can be photographed. Its principal use will probably be for the study of the distribution of the stars, for complete catalogues of clusters, nebulæ, and double stars, and for the spectra of faint stars. The amount of material accumulated will be enormous, and the best method of discussion will form a very difficult and important problem.

NOTES.

THE bulletins relating to the health of Sir Richard Owen, who is suffering from a paralytic stroke, have called forth many expressions of sympathy from the general public, as well as from men of science. Hopes of his recovery are entertained, but at his advanced age the process must necessarily be slow.

A CIRCULAR letter from the Conseil Général des Facultés de Montpellier, issued March 1, 1890, and addressed to the chief learned bodies, sets forth that on October 26, 1289, a Bull of Pope Nicolas IV. "érigeait en *Studium generale* les Facultés de Droit, de Médecine et des Arts, qui existaient déjà depuis longtemps dans notre ville." It is proposed, therefore, as we have already noted, that during the present year the University shall commemorate its entry upon its seventh century. The *fête* will probably be held towards the end of May.

AFTER the reading of the papers at the ordinary meeting of the Royal Meteorological Society on Wednesday, March 19, the Fellows and their friends will have an opportunity of inspecting the Exhibition of Instruments illustrating the application of photography to meteorology, and of such new instruments as have been invented and first constructed since the last Exhibition. The Exhibition will, at the request of the Secretary of the Institution of Civil Engineers, be open in readiness for their meeting on Tuesday evening the 18th instant, and will remain open till Friday the 21st instant.

AN International Exhibition of Mining and Metallurgy will be held this year at the Crystal Palace from July 2 to September 30. The Lord Mayor is the patron, the Duke of Fife the Hon. President, and the list of Hon. Vice-Presidents contains the names of Lord Wharncliffe, Lord Brassey, Lord Thurlow, Sir Frederick Abel, Sir Alexander Armstrong, Sir F. Dillon Bell, Sir Graham Berry, Sir Charles Clifford, Sir James Kitson, Sir Roper Lethbridge, M.P., Sir John Lubbock, M.P., Sir John Pender, Sir E. J. Reed, M.P., Sir Saul Samuel, Sir Warington W. Smyth, Sir Charles Tennant, M.P., Sir Edward Thornton, Sir Charles Tupper, Sir H. Hussey Vivian, and Prof. Roberts-Austen. Mr. Pritchard Morgan, M.P., is chairman, and Mr. Henry Cribb deputy-chairman of the Executive Council, which consists of 20 gentlemen well known in engineering and mining matters. The following are the subjects likely to be included within the scope of the Exhibition:—Machinery, mining in gold and silver, diamonds and precious stones, ironstone and iron-ore mining, the manufacture of iron and steel, lead, tin, copper, and coal mining, petroleum and salt industries, and a number of other kindred subjects. Ambulance practice and the condition of miners will also be illustrated.

A GENERAL meeting of the Society for the Preservation of Ancient Monuments in Egypt will be held at the rooms of the Royal Archæological Institute to-morrow (Friday), at 5 p.m. Attention will be specially called to the wanton excision of portions of the well-known fresco paintings in the tomb of the Colossus on a sledge, dating from the Twelfth Dynasty, or between 2000 and 3000 years B.C., at Der-el-Barsha, the chipping out of cartouches of different Sovereigns from the Sixth

Dynasty tombs at the same place, the mutilations of tombs at Beni Hassan, the malicious removal of curious bas-reliefs at Tel-el-Armana, and other recent acts of vandalism. Such outrages as these ought surely to be made practically impossible. All that is needed is that the matter shall be seriously taken in hand by the Foreign Office.

AN attempt is being made by the Society of Antiquaries of London to raise a fund, the interest of which shall be used from time to time to defray the expense of excavations, or to advance archæological knowledge in such other ways as may seem suitable to the President and Council of the Society. The object is one which ought to commend itself to all who interest themselves in archæology. The Society wants a capital sum of only £3000. Subscriptions should be sent to the treasurer, Dr. E. Freshfield, 5 Bank Buildings, E.C.

MR. GLADSTONE has consented to open the new Residential Medical College at Guy's Hospital on Wednesday, March 26, at 2 p.m.

THE treasures of the Ruskin Museum at Sheffield are being transferred from the small building at Walkley, in which they have hitherto been kept, to more convenient premises. The Museum will be reopened by Lord Carlisle on July 15.

THE March number of the *Kew Bulletin* opens with an account of Indian Yellow, or Purree, about the origin of which there used to be much uncertainty. Some time ago, in consequence of inquiries made in India at the request of the authorities at Kew, the mystery was cleared up; and full information on the subject will be found in the present paper. Another paper deals with Bombay aloe fibre, and there are sections on the commercial value of loxa bark, and on barilla.

AN industrial and artistic Exhibition will shortly be opened in Ouéno, the most beautiful park in Tokio. M. de Lezey, writing to *La Nature* on the subject from Tokio, says that the Exhibition will be particularly rich in collections of Japanese antiquities.

ON February 22 the Johns Hopkins University celebrated the twelfth anniversary of its opening. It was announced that, of the various pressing needs of the University for expansion, that of the chemical laboratory was to be met by turning over to it for reconstruction the ill-ventilated Hopkins Hall.

THE collections belonging to the Academy of Natural Sciences of Philadelphia grow so rapidly that the accommodation provided for them is wholly inadequate. A new building is to be erected, and the State Legislature has voted $50,000 as a contribution towards the expenditure. It is hoped that another "appropriation" of the same amount will be made, and that the rest of the money required will be privately subscribed.

GERMAN papers announce the death of Dr. Karl Emil von Schafhäutl, Professor of Geology, Mining, and Metallurgy at Munich University, keeper of the geognostic collection of the Bavarian State, and member of the Academy of Sciences. He was not only an eminent physicist and geologist, but also a theoretical musician of some note. He was born at Ingolstadt on February 26, 1803, and died at Munich on February 25 last.

THE death of Victor, Ritter von Zepharovich, is also announced. He was Professor of Mineralogy at the German University of Prague, a member of the Academy of Sciences at Vienna, and author of the "Mineralogical Dictionary of the Austrian Empire," and many valuable mineralogical and crystallographical works. He was born at Vienna on April 13, 1830, and died at Prague on February 24 last.

ON Tuesday evening, Dr. Dallinger delivered an interesting lecture at the Royal Victoria Hall, on "The Infinitely Great and the Infinitely Small," to an audience numbering about 400, composed principally of working men. The lecture was illustrated by numerous lantern-views, and was evidently much appreciated.

IN the *Engineer* of the 7th inst., there is an excellent article on the latest express compound locomotive on the North-Eastern Railway. This engine is for the east coast Scotch traffic on the section between Newcastle and Edinburgh—about 125 miles. A trial was made with a train of thirty-two coaches (total weight of train 270 tons) between Newcastle and Berwick, a distance of sixty-seven miles; and the time was seventy-eight minutes, or three minutes less than the Scotch express. With the heaviest loads an assistant engine will not be necessary. In another trial with a special train of eighteen six-wheeled coaches, a speed of about ninety miles per hour was obtained. This is the highest recorded speed by several miles. Diagrams were taken at various speeds, one set at a speed of eighty-six miles per hour on the level. This speed was carefully measured by stop-watch and mile-posts; the highest speed observed was just over ten seconds per quarter mile run. It is evident from these facts that passengers to the north will not waste much time on the journey when the summer traffic begins on the east coast route.

SOME time ago we referred to a paper in which Dr. Daniel G. Brinton developed the theory that the ancient Etruscans were an offshoot or colony of the Libyans or Numidians of Northern Africa—the stock now represented by the Kabyles of Algeria, the Rifians of Morocco, the Touaregs of the Great Desert, and the other so-called Berber tribes. This paper Dr. Brinton has followed up by another, in which he compares the proper names preserved in the oldest Libyan monuments with a series of similar names believed to be genuine Etruscan. The resemblances in many cases are certainly striking, and Dr. Brinton's ideas on the subject deserve to attract the attention of scholars.

AT a meeting of the Royal Botanic Society on Saturday, reference was made to a very interesting collection of seeds of economic and food plants, timber trees, &c., of Uruguay, presented by Consul Alex. K. Mackinnon. On the table were plants in flower of *Narcissus poeticus*, lately received from China, and several varieties of the same flower from the Scilly Isles, illustrating the cosmopolitan nature of this family of plants. In the Scilly Isles narcissi are grown by the acre, and over ten tons of the flowers are sent off weekly to market.

IN the current number of the *Revue des Sciences naturelles appliquées*, M. Mégnin has a valuable paper on the existence of tuberculosis in hares. About two years ago he described a peculiar disease brought on by the presence of some species of *Strongylus* in the lungs of hares. The disease dealt with in the present paper is wholly different.

M. H. BEAUREGARD, *aide-naturaliste* in the Paris Museum of Natural History, has published an elaborate monograph on the Vesicant tribe of insects. It is illustrated by many fine plates.

THE skeleton of a mammoth has been discovered in the Russian province of Tula, and the Moscow Society of Naturalists have sent a commission to excavate it.

MESSRS. MACMILLAN AND CO. are issuing a thoroughly revised edition of "A Treatise on Chemistry," by Sir H. E. Roscoe, F.R.S., and C. Schorlemmer, F.R.S., and have just published Part II. of Vol. III., dealing with the chemistry of the hydrocarbons and their derivatives. Since this part of the work was published in 1884, many additions have been made to our knowledge of this department of organic chemistry; and the authors, as they themselves explain, have sought to represent the present position of the science by introducing the results of the latest and more important researches, with the effect that the greater part of the volume has been re-written.

MR. JOHN MURRAY has published the nineteenth edition of "The Reign of Law," by the Duke of Argyll.

THE *Amateur Photographer* has issued its fourth "home portraiture number." It reproduces one photograph each from the work contributed by sixty competitors for prizes.

IN the Report of the U.S. Commissioner of Education for the year 1887–88 it is stated that 48 educational institutions in the United States receive the benefit of the national land grant of 1862. Among these institutions are the Arkansas Industrial University, the State Agricultural College at Colorado, the Maine State College of Agriculture and the Mechanic Arts, the Massachusetts Institute of Technology, the Missouri School of Mines and Metallurgy, and the Scientific School of Rutgers College. In 38 of the Colleges an officer of the Army or Navy is detailed to act as professor of military science and tactics. If a State has more than one school endowed by the national land grant of 1862, the school which is reported by the Governor of the State as most nearly meeting the requirements of existing law is held to have the first claim to the officer allotted to the State.

M. A. ANGOT, of the French Meteorological Office, has published in the *Annales* of that office a very careful discussion of the diurnal range of the barometer, based upon the best available data for all parts of the globe. After having given the mean range for each month and for the year, he has calculated the amplitudes and phases of the first four simple harmonic oscillations into which the complex oscillation of the barometric diurnal range may be resolved, and which may be considered as the resultant of the superposition of two waves of different origin and character. One of these, which the author terms the thermic wave, is of a more or less complicated form in appearance, and is easily explained as being produced by the diurnal variation of temperature and by the differences that this variation presents between neighbouring stations. The other, the principal semi-diurnal wave, for which he has given the numerical law, presents a much more simple form, and is not at all affected by local conditions. It is possibly produced by the calorific action of the sun upon the upper strata of the atmosphere; but, as the author states, this is still only an hypothesis, and the theory of this part of the phenomenon remains to be established. His conclusions upon the effect of the thermic wave are very interesting, and the whole discussion will well repay a careful study.

MR. T. W. BAKER writes to us that, in his note regarding the meteor of March 3, he omitted to state the time of its appearance, which was 7.28 p.m.

AN important paper upon the crystalline allotropic forms of sulphur and selenium is contributed by Dr. Muthmann, of Munich, to the latest number of the *Zeitschrift für Krystallographie*. Besides the well-known rhombic pyramids and monoclinic prisms, sulphur may, under certain conditions, be obtained in a third crystalline modification, which has been termed by Gernez "*soufre nacré*." This third modification has been fully investigated by Dr. Muthmann, and, in addition, a new fourth totally distinct variety has been discovered. The third form is best obtained by boiling about five grams of powdered sulphur with 750 c.c. of absolute alcohol in a flask provided with an inverted condenser for one hour, filtering through a warmed funnel into a large flask heated to 70° C. in a water-bath, and allowing the alcohol to slowly evaporate. After about twelve hours a large deposit of brilliant tabular crystals is formed. Similar crystals of the third variety may be obtained by agitating a saturated alcoholic solution of ammonium sulphide with excess of powdered sulphur, filtering, diluting with a little alcohol and allowing to stand in a loosely covered cylinder. In a few hours crystals are found deposited, often measuring a couple of centimetres in length and 1–2 mm. thick. Another method which yielded very beautiful crystals of this modification consisted in allowing a solution of acid potassium sulphate to slowly diffuse into a solution of sodium thiosulphate. In about four weeks' time, perfect crystals, almost white in appearance, and exhibiting strongly the mother-of-pearl lustre, were obtained. This third variety of sulphur also crystallizes in the monoclinic system. The ratio of its axes is $a : b : c = 1{\cdot}0609 : 1 : 0{\cdot}7094$. The axial angle $\beta =$ 88° 13′. The symmetry plane, $b = (010) \infty \grave{P} \infty$, is so largely developed as to give the crystals the appearance of plates. At the edges of the plates the two primary pyramids $(111) - P$ and $(\bar{1}11) + P$, a prism $(210) \infty \bar{P} 2$, and a clinodome $(012) \tfrac{1}{2} \grave{P} \infty$ are well developed. These crystals are totally distinct from those of the second modification; the axial ratios of the latter are $a : b : c = 0{\cdot}9957 : 1 : 0{\cdot}9998$ and $\beta = 84° \, 14'$. Upon the sides of the vessel containing the alcoholic ammonium sulphide solution prepared as above, Dr. Muthmann noticed curious tabular crystals of hexagonal section, which immediately became altered upon contact with a disturbing body, such as a platinum wire or glass rod. They were likewise found to consist of pure sulphur, and, on optical and goniometrical examination, were found to consist of a distinct fourth modification, also monoclinic. They greatly resemble a rhombohedron with predominating basal plane. They are best obtained by allowing to slowly evaporate in a tall cylinder a saturated solution of sulphur in alcoholic ammonium sulphide diluted with four times its volume of alcohol. The temperature during this crystallization must not exceed 14° C. Occasionally in this experiment all four forms of sulphur are obtained; the surface is covered with crystals of the third variety, tables of the fourth modification are deposited upon the sides, and the base of the cylinder is spangled with rhombic pyramids interspersed with monoclinic needles of the second form. If crystals of the third variety are suspended in their mother liquors and left for some days, they are converted into a voluminous mass of minute rhombic pyramids. The conversion into the more stable rhombic form is almost instantaneous if a rhombic crystal be dropped into the liquid containing suspended third variety crystals. The immediate alteration of crystals of the fourth kind is even more remarkable, the mere movement of the cover-glass, when examining them under the microscope, being sufficient to instantly change the optical properties to those of the rhombic form. It is interesting that this fourth form of sulphur is isomorphous with the form of selenium obtained by evaporation of a hot saturated solution in carbon bisulphide.

THE additions to the Zoological Society's Gardens during the past week include two Badgers (*Meles taxus*) from Ireland, presented by Mr. P. Bicknell; a Grey Hypocolius (*Hypocolius ampelinus* ♂) from Scinde, presented by Mr. W. D. Cumming; a Rhesus Monkey (*Macacus rhesus* ♂) from India, a Spotted Ichneumon (*Herpestes nepalensis*) from Nepal, deposited; an Axis Deer (*Cervus axis*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at Greenwich at 10 p.m. on March 13 = 9h. 25m. 55s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) { G.C. 1861	—	White.	9 25 47	+21 58
(1) { G.C. 1863	—	White.	9 25 58	+22 0
(2) 8 Leo Minoris ...	5·7	Reddish-yellow.	9 24 51	+35 35
(3) ι Hydræ	4	Whitish-yellow.	9 34 12	− 0 39
(4) ο Leonis...	4	Yellowish-white.	9 35 18	+10 24
(5) 132 Schj.	Var.	Red.	10 32 7	−12 25

Remarks.

(1) Described by Herschel as a bright extended nebula with two nuclei, the north following one being very faint. In 1848, Lord Rosse observed that the nebula was distinctly spiral, and his drawing represents it as elliptical in shape. The nebula is about 3′ long and is situated about 2° south of the star λ Leonis. I am not aware that any record of the spectrum has been published.

(2) A star of Group II. Dunér states that the bands 2, 3, 7, 8 are visible, but are rather weak and not very wide. The bands 4 and 5 are very delicate. The star belongs to species 5 of the subdivision of the group, which means that the meteor-swarm of which the "star" is probably composed is somewhat sparse. The bright carbon flutings should therefore be well developed. Bright lines may possibly also be present, if the swarm is not too far condensed.

(3) Konkoly and Vogel both describe the spectrum of this star as a well-developed one of the solar type. The usual differential observations are required.

(4) A star of Group IV. (Vogel). The usual observations of the relative thicknesses of the hydrogen and other lines are required.

(5) A star of Group VI., with a spectrum of extraordinary beauty (Dunér). The spectrum consists of four zones, and all the bands 1–10 are strongly developed. Band 6 is not very dark. The specific differences in stars of this group have not yet been fully investigated. The principal variations so far observed are: (1) the length of continuous spectrum, as indicated by the number of zones visible; (2) the number and intensities of the secondary bands; (3) the intensity of band 6 as compared with bands 9 and 10.

Gould believes this star to be variable, his estimates of the magnitude varying between 4·3 and 6·1. Birmingham's values vary from 4·5 to 6·3. The star appears to be U Hydræ, and, if so, a maximum will be reached about March 18 (*Observatory Companion*, 1890). Espin believes the period to be about 195 days.

As yet, we have no information as to changes of spectrum accompanying changes of magnitude in stars of this group.

A. FOWLER.

THE SOLAR AND THE LUNAR SPECTRUM.—Prof. Langley's second memoir on this subject, which was read before the National Academy of Science in November 1886, has been received. In a previous memoir it was demonstrated that evidence of heat had been found in the invisible spectrum of the sunlit side of the moon, and the experiments indicated that this heat was chiefly not reflected but radiated from a surface at a low temperature. The amount of heat, however, was excessively minute, even when compared with the feeblest part of the solar spectrum known in 1882, yet it was easily recognizable because of the fact that, whereas in the typical solar spectrum heat is greatest in the short wave-lengths, in the typical lunar spectrum heat is greatest in the long wave-lengths.

In this second memoir the results of further observation of the infra-red solar spectrum are given, the newly investigated region being close to that which contains a large part of the lunar heat. The researches considerably extend those previously made. In passing from the visible part of the spectrum into the infra-red region, wider regions of absorption occur. To an eye which could see the whole spectrum, visible and invisible, the luminous part would be, as is well known, interrupted by dark lines, the lower part to 5 μ would appear to consist of alternate dark and bright bands, and the part below 5 μ be nearly dark, but with feeble "bright" bands at intervals. This appearance is shown in a plate accompanying the memoir. It is noted as a curious fact that the centres of several of the bands or lines are under some conditions found to be shifted to a recognizable extent, and hence their wave-lengths are, within certain limits, variable. This apparent shift is found to be because the absorption does not increase symmetrically with the centre of the band, but more on one side than another, so as to considerably modify the position of greatest absorption.

THE CORONA OF 1889 DECEMBER 22.—The March number of the *Observatory* contains a Woodburytype reproduction of this corona taken by the late Father Perry with a short focus reflector of Mr. Common's, and a note by Mr. W. H. Wesley, assistant secretary of the Royal Astronomical Society, upon its prominent features. Mr. Wesley finds that, as in the eclipse of January 1, 1889, the extension is greatest towards the equatorial regions, and on the longest exposed plate it can be traced to nearly a diameter from the limb. A wide rift at the north pole, extending 60° or 70° along the limb, contains several fine straight rays similar to the polar rays in 1878 and 1889 January 1, but not so numerous, regular, or distinct. The usual polar rays are scarcely distinguishable at the south pole. A remarkable fact is that the general mass of the corona on the eastern side is considerably broader from north to south than on the western side. This was also the case in 1878. Numerous prominences are seen on the eastern limb, and plates taken near the end of totality show a range of low prominences on the western limb. An interesting feature in the plates taken with the reflector is the photographic reversal of the prominences and the brighter parts of the corona. In the larger exposed negatives the prominences and the corona near the limb are bright instead of dark, whilst the limb itself is bounded by a very definite dark line indicating a double reversal.

THE NEBULAR HYPOTHESIS.—Mr. Herbert Spencer contributed an essay on Laplace's famous theory to the *Westminster Review* for July 1858. With the assistance of Mr. Thynne Lynn, a new edition of this essay has been prepared and distributed amongst leading astronomers at home and abroad.

The revised calculations bring out more strongly than ever Mr. Spencer's views of the nebular hypothesis, and in particular the portion referring to Mars. When the essay first appeared the density of this planet was taken as 0·95, but recent and more exact determinations show the value to be much too high, and taking this into account the fact comes out that to agree with Mr. Spencer's views Mars should have from one to four satellites as it has since 1877 been known to have.

Olbers's theory that the asteroids are fragments of an exploded planet is favoured, and the genesis of the thirteen short-period comets is found in the same catastrophe. It is needless to say that the theory is defended in a most masterly manner, although the arguments against its acceptation are overwhelming.

NEBULA, GENERAL CATALOGUE NO. 4795.—The Journal of the Liverpool Astronomical Society for December 1889, which has just been issued, contains a note by Mr. W. E. Jackson on this nebula, R.A. 22h. 24m., N.P.D. 111° 24′. It is described in the General Catalogue as "Remarkable, pretty faint, very large, extended or binuclear." Mr. Jackson has carefully observed the nebula several times, and finds that there are several stars involved, although no mention of them is made in the Catalogue, and that there is a strong suspicion of others beyond the reach of his 6 inch Grubb telescope. A sketch of the appearance accompanies the note.

A NEW ASTEROID.—Minor planet (283) was discovered by Prof. Luther (Hamburg) on February 24.

CAMBRIDGE ANTHROPOMETRY.

ABOUT two years ago the results were published, in the Journal of the Anthropological Society, of the first batch of measurements taken at Cambridge. These comprised rather more than 1100 cases. During the last two years a nearly equal number have been obtained, and it therefore becomes important to compare the results yielded by these distinct batches.

The measurements proposed by Mr. Galton, and adopted by the Cambridge Committee, were the following:—(1) A test for the eyesight. The extreme distance at which a man could read "diamond type" (viz. the print employed in the little pocket Common Prayer-books) was noted with each eye separately; the figures given in our tables indicate the mean of the two. It may be remarked that, as this instrument would only record up to 35 inches, and as about ten per cent. of the men could read at this distance, it is certain that many could have seen further. The arithmetical mean, therefore, though good enough for our present purposes, is here less scientifically appropriate than the "median." (2) A test of the muscular strength of the arms when employed in an action similar to that of pulling a bow. Two handles, connected at a convenient distance apart, are pulled away from each other against the pressure of a spring. (3) A test of the power of "squeeze" of each hand separately. In this case two handles stand a short distance apart, and are then pressed towards each other against the action of a spring. The figures here given denote the mean of the two results. (4) Measurement of the size of the head. This is taken in three different directions, viz. from front to back, between the two

sides, and upwards from a line between the eye and the ear. The product of these three measurements is what is given in the annexed tables as "head-volumes." It need hardly be said that these numbers do not assign the actual magnitudes of the heads; but they do all that is wanted for our purpose, viz. they are *proportional to* these magnitudes, on the assumption, of course, that the average shape of the head is the same throughout. (5) A test of the breathing capacity. The volume of air, at ordinary pressure, that can be expired is measured by the amount of water displaced from a vessel. The result is given in cubic inches. (6) The height; deducting, of course, the thickness of the shoes. (7) The weight, in ordinary indoor clothing. This is assigned, in our tables, in pounds.

As regards the persons measured, they are exclusively students—that is, undergraduates, with a small sprinkling of bachelors and masters of arts. Nine-tenths of them were between the ages of 19 and 24 inclusive. Statisticians will understand the importance of this fact in its bearing upon the homogeneity of our results; since a comparatively small number of measurements, in such cases, will outbalance in their trustworthiness a very much larger number which deal with miscellaneous crowds.

But it is not so much to the above characteristics that I wish to direct attention here as to one in respect of which our University offers an almost unique opportunity. No previous attempt, it is believed, has ever been made to determine by actual statistics the correlation between intellectual and physical capacities. What, however, with the multiplicity of modern examinations, and the intimate knowledge possessed by many tutors about the character and attainments of their pupils, this could here be effected to a degree which could not easily be attempted anywhere else. By appeal to these sources of information, the students were divided into three classes (here marked as A, B, and C), embracing respectively (1) scholars of their College, and those who have taken, or doubtless will take, a first class in any tripos; (2) those who go in for honours, but fall short of a first class; and (3) those who go in for in for an ordinary degree, to which class also are assigned those who fail to pass. It is not for a moment pretended that such a classification is perfect, even within the modest limits which it hopes to attain. Very able men may fail from indolence or ill-health, and very inferior ones may succeed through luck or drudgery. But it must be remembered that we only profess to deal with averages, and not with individuals, and on average results such influences have little power. There are probably few cricket or football clubs in which one or more men in the second eleven or fifteen are not really better than some in the first; but no one supposes that the second team would have much chance of beating the first. All that is maintained here is that our A, B, C classes, *as classes*, stand out indisputably distanced from each other in their intellectual capacities. The average superiority of one over the next is patent to all who know them, and would be disputed by very few even of the men themselves.

The plan adopted has been to classify the A, B, C men separately, arranging each of these in sub-classes according to their age. On the last occasion about 1100 were thus treated, and it is very important to observe that the new batch (of about 1000) independently confirms the conclusions based on the previous set. Space can scarcely be afforded for these tables separately, so I only give here the results of grouping the entire two sets together. But as a matter of evidence, it must be insisted upon that the two separate tables tell the same tale.

The following, then, are the results of thus tabulating the measurements of 2134 of our students:—

TABLE I.

Class A (487).

No.	Age.	Eyes.	Pull.	Squeeze.	Head.	Breath.	Height.	Weight.
10	18	21·3	75·8	75·3	235·8	244·0	68·13	142·6
42	19	22·6	75·3	80·9	242·9	255·5	69·04	148·0
99	20	23·7	81·2	83·5	242·8	252 7	69·00	152·1
104	21	23·6	81·6	82·8	242·1	255·2	68·82	152·3
94	22	24·6	83·9	87·1	244·3	257·2	68·71	154·0
48	23	21·9	82·0	84·2	242·9	262·8	69·11	149·7
33	24	23·6	84·9	84·0	245·9	261·5	68·90	154·8
57	25	23·0	80·9	82·7	247·2	251·0	68·59	154·6
Average..		23·4	81·5	83·5	243·6	255·6	68·85	152·5

Class B (913).

38	18	24·4	77·4	82·1	236·7	235·0	68·92	148·5
136	19	25·4	78·7	80·3	238·0	249·8	68·78	149·7
280	20	24·0	82·5	84·2	237·3	255·1	69·08	153·5
212	21	23·5	83·7	83·7	235·5	257·2	68·84	153·0
136	22	24·6	84·7	85·3	239·2	257·2	69·17	153·3
54	23	22·7	81·5	83·5	234·4	259·0	69·31	154·0
21	24	26·1	90·6	87·4	245·5	261·5	68·93	157·7
36	25	22·6	85·8	86·1	237·1	264·5	68·83	157·2
Average..		24·1	83·2	84·4	237·3	254·9	69·00	152·8

Class C (734).

32	18	24·4	82·4	83·7	234·2	238·0	68·68	156·0
98	19	24·8	81·8	83·6	231·4	250·0	69·10	152·9
185	20	24·8	83·5	82·8	235·0	252·7	69·03	153·6
163	21	23·7	86·1	84·5	239·6	258·1	69·23	156·0
123	22	24·4	89·5	86·6	236·8	255·5	68·79	155·4
57	23	23·8	88·1	87·2	238·5	256·4	68·97	156·2
26	24	25·4	87·4	86·1	239·3	244·0	68·35	156·0
50	25	24·0	82·5	84·2	243·2	247·5	68·24	154·2
Average..		24·4	85·2	84·5	236·8	252·9	68·93	154·8

These tables may be looked at from two points of view, which would commonly be called the practical and the theoretical. By the former, to speak in the more accurate language of statistics, I understand any conclusions to be involved which do not recognize distinctions of less than about 4 or 5 per cent. of the totals in question. Looked at with this degree of nicety, the main fact that the tables yield is, that there is no difference whatever (with a single exception, to be presently noticed) between the physical characteristics of the different intellectual grades. Whether in respect of height, weight, power of squeeze, eyesight, breathing capacity, or head-dimensions, there is no perceptible distinction. There *are* differences, of course, but to say whether or not these are of any significance requires an appeal to the theory of statistics and to tests beyond the reach of the "practical" standard.

The one exception is in the power of "pull." I called attention to this two years ago; but, with the bulk of statistics at that time at our command, I felt somewhat doubtful as to its real significance. But there can scarcely be any doubt as to the non-casual nature of a difference of power between the A and C classes amounting to 4·6 per cent., when this difference displays itself between the averages of such large numbers as 487 and 734 respectively. At least, if there were any doubt, it would be removed by another mode of displaying the results, to explain which a brief digression must be made. In the preceding tables the primary division into three classes was based on intellectual differences. Let us make, instead, one based on physical differences. Let the first class, in respect of each kind of measurement, embrace "the best in ten"; in other words, select the top 200, or thereabouts, in each separate list. Such a table will show, for one thing, the extent to which one kind of physical superiority is correlated with another; and also, by reference to the triposes and tutors' information, it will show how these classes are composed in respect of their A, B, C constituents. The following is such a table, arranged to show how such "first classes" in one physical department stand in relation to the principal other such departments.

TABLE II.

Comparative Excellence in Different Physical Capacities.

	Eyes.	Pull.	Squeeze.	Breath.	Height.	Weight.
1st Class, Eyes	34·6	86·6	83·5	263·2	69·40	157·1
" Pull	25·4	113·0	93·9	280·2	69·82	167·3
" Squeeze	24·2	96·5	103·7	278·7	70·45	170·1
" Breath	24·9	94·3	92·4	320·5	71·19	167·3
" Height	25·3	88·0	90·4	286·7	73·25	171·5
Average student..	24·1	83·5	84·2	254·5	68·94	153·4

I shall call attention hereafter to certain conclusions furnished by this table as to the correlation of these various physical characteristics. At present they are only appealed to in confirmation of the fact alluded to above. It is rather curious that, when we sort out these first classes into their A, B, C constituents, we find that, with the same single exception, the distribution is about what it would be on a chance arrangement. That is, the men of exceptional height or breathing capacity are just as likely to be found amongst the A's as amongst the B's or C's. This is the case even with the eyesight. The first class here was confined to men who could read distinctly the small print (diamond) employed, at a distance of at least 35 inches; with the additional restriction that the weaker eye of the two could read the same at 33 inches. Of such men there were 196 out of 2134. Now had these been taken indiscriminately from the three classes A, B, C, the most likely proportions would have been respectively 44, 84, and 68. The actual numbers were 46, 88, and 62. But when we select in the same way a first class (consisting of 182) of the strongest "pullers," we find that whereas A, B, C, should contribute respectively 41, 78, and 63, they actually contribute 28, 78, and 83. Taken in connection with our previous results, the conclusion seems inevitable that this particular kind of physical superiority is, to a certain extent, for some reason or other, hostile to intellectual superiority.

The question *why* this is so is one which it is not easy to answer with confidence, but the following suggestion may be offered. The action of "pulling" is the only one in the above list of physical tests which is much practised in any popular games: it obviously is so in rowing, whilst in cricket a similar set of muscles appear to be exerted. But no known game appears much to practise our "squeezing" power; and, as regards the height, weight, breathing, and seeing powers, probably any form of exercise which keeps a man in good health offers sufficient scope for development. It would therefore seem to meet all the observed facts if we suppose that our hard-reading men take amply sufficient exercise to develop their *general* physical powers fully up to the same relatively high standard found amongst the others; but that the non-reading men, or a certain proportion of them, are rather apt to devote themselves to certain kinds of exercise which develop a proportional superiority in one special muscular development.

I should not have directed so much attention to this second table if it were not that such considerations have a very direct bearing upon a question of importance at the present day. As some readers of this journal probably know, it has been seriously discussed, in influential quarters, whether it is not advisable to take some account of physical qualifications in our Civil Service or other State examinations.[1] By this, we may presume, is not to be understood any mere *pass* examination. The necessity of some test of that kind may be taken for granted, and would naturally be secured by a medical certificate. Something much more serious than this may plausibly be defended, and on the following grounds.

In most of the examinations of any magnitude with which the State is concerned, it may be taken as a fact of experience that the number of selected candidates bears some moderate ratio to that of those who compete. If two hundred men are found to go in and try, it will seldom be the case that there were very many more or less than fifty vacancies. Supply and demand, in a country in the present social and economic condition of England at any rate, will generally obviate any extreme disproportion between the two quantities. Now it is well known that where many aims of any kind are made at an object the so-called "law of large numbers," or "law of error," comes into play. At the two ends of our list of competitors the discrepancies in their performances will be very great. But, for a wide range on both sides of the middle, the differences will be comparatively small. A glance at any one of the lists, which are published in the papers from time to time, of the selected candidates for the army, with the number of marks gained by each, will illustrate this. Near the top the difference between one candidate and the next may be measured by hundreds of marks, whilst towards the bottom of the selected candidates (*i.e. towards the middle of the competitors*) the difference will be given in tens only, or even in units. So marked is this tendency that any well-informed statistician could often give a very shrewd guess, from the mere inspection of such a list, as to the number of candidates who had failed to pass, and whose names therefore were not mentioned.

Now, this being so, it follows that the differences between, say, the last 20 per cent. who succeeded, and the first 20 per cent. who failed, are extremely slight, *in respect of the qualities thus tested*. Might it not then be wise to take account of some other quality, and what better could be found than the physical? If by sacrificing little or nothing of mental superiority we can gain a good deal of physical superiority, there is much to be said in favour of such a final appeal. If, for instance, we accepted, in the first instance, 20 per cent. more than we wanted to retain, and then subjected the whole number to some physical test, for which a moderate amount of marks were assigned, the men finally excluded would at worst necessarily be those who were only just admitted on the customary plan, and those finally admitted would at worst necessarily be those who otherwise would only just have been rejected.

There is not space here to discuss fully any such proposal, but if any scheme of this kind is ever introduced its justification must rest on considerations such as those displayed in our second table. One or two results may be pointed out. In the first place, it must be insisted that the whole merit of any such scheme rests upon the assumption that mental superiority may be considered as perfectly "independent" (in the mathematical sense) of physical. This we find is *not* quite the case as regards the "pulling" power, but is the case as regards every one of the other qualities here displayed. If we set much store upon tall men, or upon men with good eyes, we may rest assured that little or nothing will be sacrificed in the way of mental results by giving reasonably good marks for such excellence. Again, it may be remarked to what extent these different kinds of physical superiority are correlated. It appears that great superiority in any one kind of physical power is accompanied by considerable superiority in every other. It is a striking fact that in only one of the thirty subdivisions there indicated, do we fail to find the "first class" man, in any one department, standing above the average man in every department.

This being so, it is rather for the physiologist, or for the man of affairs, to select the particular physical test which is likely best to serve the public interest. So far as mere statistics are concerned, I should give the preference to the *breathing power*. For one thing, this appears, in my judgment, to be correlated, on the whole, with a higher general physical superiority than is the case with the other qualities. I apprehend also that good breathing power could not readily be "crammed," so to say, by attendance at a gymnasium, and by aid of professional advice and direction, as can be done to some considerable extent in the case of muscular power.

It has been already remarked that high excellence in one physical capacity seems correlated with decided superiority in all the others. This is evident from a glance at the tables. But it deserves notice that *equally* high excellence is not by any means implied. The chance of a man who is in one of these physical first classes being also in another such class is not very much more than what it would be if the two capacities were distributed at random. As a matter of fact, four men only out of the entire number are in every one of these first classes. As between the exertions of muscular strength apparently so closely similar as those of pulling and squeezing, it is found that only 44, out of the total of 195 in the latter, also secured a place in the former; whereas a purely chance distribution might have been expected to secure as many as about 20. As between the corresponding selections, of about equal numbers, from the best in respect of eyesight and breathing, it appears that not more than 30 obtain a place in both classes.

Turn now to some of the less obviously certain conclusions. Comparing the "head-volumes" of the students, two facts claim notice, viz. first, that the heads of the high-honour men are distinctly larger than those of the pass men; and, second, that the heads of all alike continue to grow for some years after the age of 19.

The actual amount of difference as between the A and C students is, of course, small. On our scale it is just about 7 inches—that is 3 per cent. on the real size of the head. Is this small difference to be regarded as significant? The answer can only be given by an appeal to the theory of statistics, which yields the following conclusions.

I must premise that the figures given here as average head-volumes were thus obtained. The average was taken of each of the three separate head-measurements (in the three directions

[1] See Mr. Galton's paper on this subject at the last meeting of the British Association.

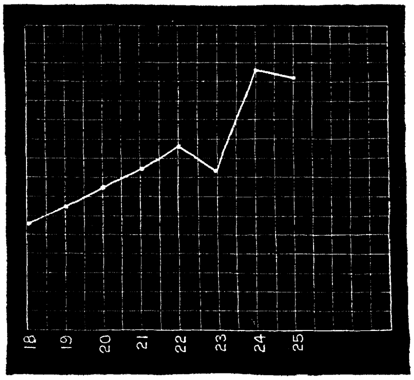
already explained) of each sub-class of students—*e.g.* of those of the A class who were 19 years of age; these three were then multiplied together, and the product resulting (in the case in question, 242·9) was entered in the table. What we have, therefore, is not strictly the mean of the products, but the product of the means. Theoretically, I apprehend, the former should have been preferred; but as the extra labour entailed would have been very great, and as the difference, when dealing with large numbers of cases and small amounts of divergence, is extremely small, I have been content with the latter. It may be added that the actual computation was made in both of these ways for a sample number of cases, and the insignificance of the difference for our purposes of comparison was statistically verified.

What theory directs us to do is of course to begin with determining the probable error of the individual head-volumes of the men generally. This is found to be, on the scale in question, about 17 inches. The usual formula for the difference between the means of 734 and of 487 would then assign to this difference a probable error of $17 \times \sqrt{\frac{1}{734} + \frac{1}{487}}$, viz. nearly one inch. The actual observed difference, of nearly 7 inches, thus lies enormously outside the bounds of probability of production from mere statistical chance arrangement. But in this calculation there is a source of error omitted to which attention was directed not long ago by a correspondent in NATURE, viz. the actual errors (in the literal sense of that rather unfortunate technical term) committed by the observer, or involved in the mechanism of the instrument. Two years ago I had taken it for granted that these were insignificant; and, had it been otherwise, the materials at our disposal would hardly have enabled us to make the due allowance. But, as the correspondent pointed out, the error is by no means to be neglected, and we have now the means of fairly estimating it. A considerable number of men have been measured five or six times, and some even oftener, whilst one man, who seems to have had a morbid love of this physical inspection, has actually had his various dimensions and capacities tested no less than eighteen times during the course of some three years. These cases have furnished a fair basis of determination. They show that these personal errors are certainly greater than they should be (they seem to arise in part from a certain looseness in the machine, which will be remedied in future), amounting in certain extreme cases to as much as even half an inch on the single measurement, and therefore to much more in what appears here as a "head-volume." The resultant "probable error" from this fresh source of disturbance amounts to about five (cubic) inches. Those unfamiliar with probability may perhaps be staggered by such an admission, but they may be assured that the healing tendency of the averages of large numbers is very great, and that the results remain substantially unaffected. The problem appears to be simply one of the superposition of two independent sources of error, and may be stated thus: Given a large number (over 2000) of magnitudes, with a mean of 239, and a "probable error," about this mean, of 17; and assume that these magnitudes are inaccurately measured with a further probable error of 5 inches (as seems to be the fact), what is the probable error of the divergence between the two averages obtained respectively from 734 and 487 of these results? The answer is still a little less than one inch. It is, that is to say, an even chance that the two averages will not differ by more than this; and it is, consequently, thousands to one that they will not differ by so much as seven inches. The conclusions, therefore, previously drawn, lose little of their force.

It seems to me almost as certain that the size of the head continues to increase up to at any rate the age of 24. This will be made clear by looking at the following diagram, which is drawn to show the sum of the figures of the head-measurements as contained in Table III.

As regards the comparative physical endowments, in the other respects, of the different classes of students, there does not seem to be much to say. The differences—sometimes one way and sometimes the other—between them in respect of height, weight, breathing, and squeezing power, are so small as to be statistically insignificant, averaging only about 1 per cent. That the first-class honour men, however, have slightly inferior eyesight seems established, especially when we bear in mind that each batch of about 1000 cases tells the same tale; the only evidence telling the other way is the fact, already adverted to, that when a class comprising "the best in ten," as regards eyesight, is selected from the whole number, we do not find any appreciable intellectual selection to be thereby entailed.

An equally trustworthy basis of comparison is found by observing the distribution of the short-sighted men. Let us take as the limit of what shall be termed "short sight" the inability to read the diamond print with both eyes at a distance greater than ten inches. Adopting this test, we find that the A, B, C classes furnish respectively 14, 11, and 11 per cent., indicating a very small difference between them.



The general conclusion to be drawn here seems, then, to be this. With the single exception of eyesight—and this to a very slight extent—it does not appear that intellectual superiority is in the slightest significant degree either correlated with any kind of natural physical superiority or inferiority, or that it tends incidentally to produce any *general* superiority or inferiority. I emphasize the word "general" in the last clause in order to allow for the difference shown in respect of pulling power. It seems probable, as has been already suggested, that the superiority of the non-honour men does not point to the slightest superiority of their general bodily development—as would be indicated perhaps if it displayed itself in respect of their height, weight, or breathing capacity—but is solely brought about by greater muscular exercise in the pursuit of certain athletic games.

So much as regards the first and second tables. As regards the third—which is arranged in order to show the development

TABLE III.

Physical Development of Students from 18 *to* 25.
A, B, C *combined* (2134).

No.	Age.	Eyes.	Pull.	Squeeze.	Head.	Breath.	Height.	Weight.
80	18	24·0	79·2	81·9	235·6	237·3	68·72	150·8
276	19	24·8	79·3	81·6	236·4	250·8	68·93	150·5
564	20	24·2	82·6	83·6	237·5	253·9	69·05	153·3
479*	21	23·6	84·0	83·8	238·3	257·0	68·96	154·1
353	22	24·6	86·2	86·2	239·7	256·6	68·91	154·2
159	23	22·8	84·0	85·0	238·4	259·4	69·12	153·5
80	24	24·8	88·4	85·6	243·6	255·8	68·73	156·0
143	25	23·3	82·7	84·1	243·3	253·2	68·53	155·1

of the physical powers between 18 and 25—there is very little to be said, as statistics of this character offer no particular novelty. Such merit, therefore, as this may possess must depend mainly on the homogeneity of the class of men concerned. As indicated at the commencement of this paper, this homogeneity is equivalent to a considerable increase in the total numbers where more heterogeneous materials are dealt with. They appear to indicate that the physical powers, as a whole, culminate at the age of 22 or 23, and thence begin to steadily decline. Too much stress, however, must not be laid upon the rate of decline here, since the last subdivision is of a somewhat less homogeneous character than the others. For one thing, the men of twenty-five really include those also who are *over* that age, though these are relatively but few. Again, whilst the men up to 24 remain (for all statistical purposes) identically the same individuals, with a year or two more added on to their

age, it would probably be found that a not insignificant proportion of those marked as 25 were men who were already older when they came into residence. J. VENN.

ABOUT eighteen months ago a brief memoir of mine—"Head Growth in Students at the University of Cambridge"—read before the Anthropological Institute, was published in NATURE (vol. xxxviii. p. 15). The *means* obtained by Dr. Venn, of the "head-products" of Cambridge students between the ages of nineteen and twenty-five were there thrown into the form of a diagram, and discussed. The head-product, I may again mention, is the maximum length of the head, × its maximum breadth, × its height above the plane that passes through the following three points: 1 and 2, the apertures of the ears; 3, the average of the heights of the lower edges of the two orbits. I drew curves that appeared to me to approximately represent the true average rate of growth, and deduced from them the following conclusions, in which I have now interpolated a few words in brackets, not because any criticism has been founded on their omission, but merely as a safeguard against the possibility of future misapprehension.

(1) Although it is pretty well ascertained that in the masses of the population the brain ceases to grow after the age of nineteen, or even earlier, it is by no means so with University students.

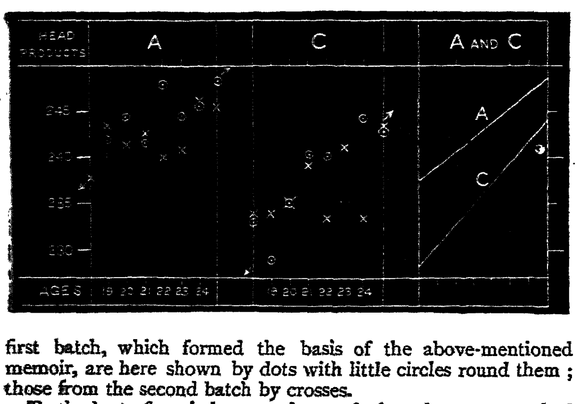
(2) That men who have obtained high honours have had [on the average] considerably larger brains than others at the age of nineteen.

(3) That they have [on the average] larger brains than others, but not to the same extent, at the age of twenty-five; in fact, their predominance is by that time diminished to [about] one-half of what it was.

(4) Consequently, "high honour" men are presumably, as a class, both more precocious and more gifted throughout than others. We must therefore look upon eminent University success as [largely due to] a fortunate combination of these two helpful conditions.

These conclusions have been latterly questioned by two of your correspondents, partly on the ground of discordance among the data, and partly on that of insufficient accuracy of the individual observations. To this I replied, that materials had since been accumulating, and that a second batch of observations, about equally numerous with those in the first, were nearly ripe for discussion, and that I thought it better to defer discussion until these had been dealt with; then, their agreement or disagreement with the first batch would go a long way towards settling the doubt.

This second batch of observations has now been discussed by Dr. Venn on exactly the same lines as the first one, and I give the results of both in the annexed diagram. The data from the



first batch, which formed the basis of the above-mentioned memoir, are here shown by dots with little circles round them; those from the second batch by crosses.

To the best of my judgment, the conclusions that were reached before are now confirmed. No person can, I think, doubt that the swarm of the A dots, and that of the C dots, are totally distinct in character. I have avoided drawing curves through either of them, lest by doing so the effect of the marks, when standing alone, should be overpowered, and it might be prejudiced. In their place, small arrow-heads are placed outside each diagram, to indicate the direction of the stretched thread that seemed most justly to represent the general trends of the two swarms of dots. Then, for the sake of convenient comparison, lines corresponding to these threads have been placed on the third diagram. It must, however, be understood that I have supposed the lines to be drawn straight, merely for convenience. In making my own final conclusions, I should take into account not only what the swarms of dots appear by themselves to show, but also the strong probability that the rate of head-growth diminishes in each successive year, and I should interpret the true meaning of the dots with that bias in my mind. FRANCIS GALTON.

SOCIETIES AND ACADEMIES.

LONDON.

Chemical Society, February 6.—Dr. W. J. Russell, F.R.S., in the chair.—The following papers were read:—Observations on nitrous anhydride and nitric peroxide, by Prof. Ramsay, F.R.S. The author recommends as the best method of preparing pure nitrogen peroxide that the deep blue-green liquid, supposed to be a mixture of this oxide with nitrous anhydride, which is obtained by condensing the products of the interaction of arsenious oxide and nitric acid, be added to a solution of nitric anhydride in nitric and phosphoric acids, prepared by adding phosphoric anhydride to well-cooled nitric acid; after agitating the mixture, the upper layer is decanted and distilled. He assumes that the two oxides interact according to the equation: $N_2O_3 + N_2O_5 = 2N_2O_4$. The melting-point of the peroxide was found to be 10°·14, in agreement with Deville and Troost's statement. The depression of the freezing-point caused by one part of chloroform in 100 parts of the peroxide was 0°·35, and by one part of chlorobenzene 0°·37; the molecular depression is therefore 41°. The heat of fusion, W, of the peroxide, calculated from this number and the observed fusing-point, by Van't Hoff's formula $W = \frac{0{\cdot}02T^2}{t}$, where T is the freezing-point of the solvent in absolute degrees and t the molecular depression, is 33·7 cals.; a direct determination gave 32·3 cals. To determine the molecular weight of nitrous anhydride, a known quantity of nitric oxide was passed into the peroxide, and the depression of the freezing-point determined. Assuming that an amount of nitrous anhydride equivalent to the nitric oxide was formed, the results gave the values of 80·9, 92·7, and 81·0 against 74, the value corresponding with the formula N_2O_3. The author was unsuccessful in freezing nitrous anhydride even at −90° by means of liquefied nitrous oxide. It was found to be soluble in this liquid, and it was further observed that as evaporation took place nitric oxide gas was given off together with the nitrous oxide; it would therefore appear that N_2O_3 is unstable even at the very low temperature at which nitrous oxide is liquid. In the discussion which followed the reading of the paper, Mr. Pickering pointed out, with reference to Prof. Ramsay's determination of the heat of fusion of nitric peroxide, that observations on substances which exercise an appreciable influence on each other cannot safely be used in deducing the heat of fusion. Thus in the case of mixtures of water and sulphuric acid, solutions containing 29·5, 18·5, 8·6, 1·0, and 0·07 per cent. of acid, gave respectively the values 37·4, 58·3, 79·9, 74·9, and 56·3 as the heat of fusion of water, instead of 79·6. In reply to Mr. Wynne, who remarked that nitric oxide alone should interact with nitric anhydride in the way attributed to N_2O_3, Prof. Ramsay stated that he had not examined the action of nitric oxide on nitric anhydride.—Note on the law of the freezing-points of solutions, by Mr. S. U. Pickering.—The action of chromium oxychloride on nitrobenzene, by Messrs. G. G. Henderson and Mr. J. M. Campbell.—Studies on the constitution of the tri-derivatives of naphthalene; No. 1, The constitution of β-naphthol- and β-naphthylaminedisulphonic acids R. and G.; naphthalenemetadisulphonic acid, by Prof. H. E. Armstrong, F.R.S., and Mr. W. P. Wynne. After alluding to the great theoretical importance of a study of the tri-derivatives of naphthalene, the authors draw attention to the necessity of determining the constitution of those tri-derivatives which are employed technically in the manufacture of azo-dyes in order that the dependence of colour and tinctorial properties on structure may be determined; and especially is this the case, since all are not equally valuable—β-naphtholdisulphonic acid G. (Gelb), like Bayer's β-naphtholmonosulphonic acid, interacting but slowly

with diazo-salts, whilst the corresponding β-naphthylamine-disulphonic acid G, like the Badische modification of β-naphthyl-aminemonosulphonic acid, is incapable of forming azo-dyes with the majority of diazo-salts. The method adopted in this and the following papers consists firstly in displacing the NH_2 radicle by hydrogen by v. Baeyer's hydrazine method and determining the constitution of the resulting naphthalenedisulphonic acid, and secondly in substituting chlorine for the NH_2 radicle by Sandmeyer's method, and characterizing the resulting chloronaphthalene-disulphonic acid and the trichloronaphthalene derived from it by treatment with phosphorus pentachloride. β-naphthylamine-disulphonic acid R is in this way found to have the constitution [$NH_2 : SO_3H : SO_3H = 2 : 3 : 3'$ (for nomenclature, see NATURE, vol. xxxix. p. 598)], and β-naphthylaminedisulphonic acid G, the constitution [$NH_2 : SO_3H : SO_3H = 2 : 1' : 3'$]. From the latter acid by the hydrazine method naphthalenemeta-disulphonic acid, the fifth known naphthalenedisulphonic acid, has been prepared; this yields a disulphochloride melting at 137°, and 1 : 3-dichloronaphthalene melting at 61°·5. The further investigation of derivatives of this acid is expressly reserved by the authors. The results obtained in the case of the G acid make it evident that, as in the case of the Bayer β-naphthol-sulphonic acid [$OH : SO_3H = 2 : 1'$] and Badische β-naphthyl-aminesulphonic acid [$NH_2 : SO_3H = 2 : 1'$], the action of diazo-salts is either retarded or prevented by the "protecting influence" exercised by an α-1'-sulphonic group.—Studies on the constitution of the tri-derivatives of naphthalene; No. 2, α-amido-1 : 3'-naphthalenedisulphonic acid, by the same. The constitution of the acid known technically as α-naphthylamine-ϵ-disulphonic acid is found to be [$NH_2 : SO_3H : SO_3H = 1' : 1 : 3'$], a result agreeing with that arrived at by Bernthsen (*Ber. der. deut. chem. Gesellsch* .22, 3327).—Studies on the constitution of the tri-derivatives of naphthalene; No. 3, α-naphthylaminedisulphonic acid, Dahl, No. iii., The constitution of naphthol-yellow S., by the same. α-naphthylaminedisulphonic acid No. iii. of Dahl's patent (Germ. pat. No. 41,957), which when diazotised and warmed with nitric acid yields naphthol-yellow S., is found to have the constitution [$NH_2 : SO_3H : SO_3H = 1 : 4 : 2'$], whence it follows that naphthol-yellow S. has the constitution [$OH : NO_2 : NO_2 : SO_3H = 1 : 2 : 4 : 2'$]. The trichloronaphthalene prepared from the α-naphthylaminedisulphonic acid affords a remarkable case of dimorphism: it is sparingly soluble in hot alcohol from which it crystallizes in slender needles melting at 66°; if the melting-point be redetermined as soon as solidification has taken place, it is found to be 56°, but if determined after a longer interval, 66°, as in the first instance. The trichloronaphthalenes prepared by Cleve from nitro-1 : 3'-dichloronaphthalene (m.p. given as 65°), and by Widman from 1 : 4-dichloronaphthalene-β-sulphochloride (m.p. given as 56°) are found to be identical with this compound, and to behave in the same way on fusion.

Geological Society, February 21.—Annual General Meeting.—Dr. W. T. Blanford, F.R.S., President, in the chair.—After the reading of the reports of the Council and of the Library and Museum Committee for the year 1889, the President handed the Wollaston Medal to Prof. J. W. Judd, F.R.S., for transmission to Prof. W. Crawford Williamson, F.R.S.; the Murchison Medal to Prof. E. Hull, F.R.S.; the Lyell Medal to Prof. T. Rupert Jones, F.R.S.; the balance of the Wollaston Fund to Mr. W. A. E. Ussher; the balance of the Murchison Geological Fund to Mr. E. Wethered; the balance of the Lyell Geological Fund to Mr. C. Davies Sherborn; and a grant from the proceeds of the Barlow-Jameson Fund to Mr. W. Jerome Harrison.—The President then read his anniversary address, in which, after giving obituary notices of several Fellows, Foreign Members, and Foreign Correspondents deceased since the last annual meeting, including the Venerable Archdeacon Philpot (who was the senior Fellow of the Society, having joined it in 1821), Dr. H. von Dechen (the oldest Foreign Member, elected in 1827), Mr. Robert Damon, Mr. J. F. La Trobe Bateman, Mr. W. H. Bristow, Dr. John Percy, the Rev. J. E. Tenison Woods, Mr. Thomas Hawkins, Prof. F. A. von Quenstedt, Prof. Bellardi, Dr. Leo Lesquereux, and Dr. M. Neumayr, he referred briefly to the condition of the Society during the past twelve months, and to a few works on palæontological subjects published in the same period. He also mentioned the finding of coal *in situ* in a boring at Shakespear Cliff, and then proceeded with the main subject of his address—namely, the question of the permanence of continents and ocean-basins. After reviewing the evidence derived from the rocks of oceanic islands, and the absence of deep-sea deposits in continental strata of various ages, he proceeded to the points connected with the geographical distribution of animals and plants, and gave reasons for believing that Sclater's zoological regions, founded on passerine birds, were inapplicable to other groups of animals or plants, and that any evidence of continental permanence based on such regions was worthless. He also showed that both elevations and depressions exceeding 1000 fathoms had taken place in Tertiary times, and gave an account of the biological and geological facts in support of a former union between several lands now isolated, and especially between Africa and India *viâ* Madagascar, and between Africa and South America. From these and other considerations it was concluded that the theory of the permanence of ocean-basins, though probable, was not proved, and was certainly untenable to the extent to which it was accepted by some authors.—The ballot for the Council and Officers was taken, and the following were duly elected for the ensuing year:—President: A. Geikie, F.R.S. Vice-Presidents: Prof. T. G. Bonney, F.R.S., L. Fletcher, F.R.S., W. H. Hudleston, F.R.S., J. W. Hulke, F.R.S. Secretaries: H. Hicks, F.R.S., J. E. Marr. Foreign Secretary: Sir Warington W. Smyth, F.R.S. Treasurer: Prof. T. Wiltshire. Council: Prof. J. F. Blake, W. T. Blanford, F.R.S., Prof. T. G. Bonney, F.R.S., James Carter, John Evans, F.R.S., L. Fletcher, F.R.S., A. Geikie, F.R.S., Prof. A. H. Green, F.R.S., A. Harker, H. Hicks, F.R.S., Rev. Edwin Hill, W. H. Hudleston, F.R.S., J. W. Hulke, F.R.S., Major-General C. A. McMahon, J. E. Marr, H. W. Monckton, E. T. Newton, F. W. Rudler, Sir Warington W. Smyth, F.R.S., W. Topley, F.R.S., Rev. G. F. Whidborne, Prof. T. Wiltshire, H. Woodward, F.R.S.

PARIS.

Academy of Sciences, March 3.—M. Hermite in the chair.—On the absorption of atmospheric ammonia by soils, by M. Th. Schlœsing. Experiments were made on the quantities of ammonia absorbed in a given time by various soils—viz. non-calcareous earths, similar to those previously used in the fixation of free nitrogen, earths containing 40 per cent. of calcareous matter, and entirely calcareous earths. The analytical results are given for each case.—Contribution to the chemistry of the truffle, by M. Ad. Chatin.—Upon the method of using, and the theory of, seismographic apparatus; note by M. G. Lippmann. The theory of the deduction of the true movement of the soil from the apparent movement, as indicated by the instruments, is mathematically discussed. A general solution of the problem is given, and applied to some special cases.—An historical note on batteries with molten electrolytes, by M. Henri Becquerel. It is shown that M. Lucien Poincaré was not justified in claiming the invention of such batteries, as M. Jablochkoff, so long ago as 1877, proposed the combustion of carbon in the nitrates as a source of electricity; and still earlier, thirty-five years ago, M. A. C. Becquerel studied similar methods.—A facsimile atlas to illustrate the history of the earliest period of cartography, by M. A. E. Nordenskiöld.—Observations of the new minor planet, Luther (288) (Hamburg, February 24, 1890), made at the Paris Observatory (equatorial of eastern tower), by Mdlle. D. Klumpke.—On the transversal magnetization of magnetic conductors, by M. Paul Janet.—On the localization of interference fringes produced by Fresnel mirrors; note by M. Charles Fabry.—Researches upon the dispersion of aqueous solutions, by MM. Ph. Barbier and L. Roux. The authors find, for concentrated solutions, that, if B be the dispersive power and p the weight of anhydrous substance dissolved in unit of volume of the solution, the relation $B = Kp + b$ holds, b being always sensibly equal to the dispersive power of water. The specific dispersive power is practically a constant quantity for each substance.—On the vapour-density of the chlorides of selenium, by M. C. Chabrié.—Upon some derivatives of erythrite, by MM. E. Grimaux and Ch. Cloez. The writers, by investigating the transformations of hydrofurfural, have attempted to establish its constitution and the method whereby it is formed from erythrite. They conclude that hydrofurfurane may be represented by the formula $\left.\begin{array}{l} CH \cdot CH_2 \\ \| \\ CH \cdot CH_2 \end{array}\right\rangle O$.—Derivatives of heptamethylene; note by M. Markownikoff.—Researches on the

preparation and properties of aricine, by MM. H. Moissan and Ed. Landrin.—Influence of light and of the leaves upon the development of the tubers of the potato, by M. Pagnoul.—The comparative physiology of the sensations of taste and touch; note by M. Raphael Dubois.—A method of studying the nuclei of white corpuscles, by M. Mayet.—On the localization, in plants, of the principles which yield hydrocyanic acid, by M. Léon Guignard.—On the intensification of sexuality in a hybrid (*Ophrys tenthrediniféro-scolopax*), note by M. L. Trabut.—On the relations which appear to exist between the Cretaceous Mammalia of America and the Mammalia of the Cernaysienne fauna in the neighbourhood of Rheims.—Remarks by M. Albert Gaudry on the communication of M. Lemoine; appearances of inequality in the development of the beings of the Old and New Worlds.—New anthropological discoveries at Champigny (Seine), by M. Émile Rivière.—Note on the formation of the delta of the Neva, according to the latest researches, by M. Venukoff.

DIARY OF SOCIETIES.

LONDON.

THURSDAY, MARCH 13.

ROYAL SOCIETY, at 4.30.—On the Organization of the Fossil Plants of the Coal-Measures, Part 17: Prof. W. C. Williamson, F.R.S.—The Nitrifying Process and its Specific Ferment, Part 1: Prof. P. F. Frankland and Grace C. Frankland.

MATHEMATICAL SOCIETY, at 8.—Some Groups of Circles connected with Three given Circles: R. Lachlan.—Perfect Numbers: Major P. A. MacMahon, R.A.

SOCIETY OF ARTS, at 5.—Agriculture and the State in India: W. R. Robertson.

INSTITUTION OF ELECTRICAL ENGINEERS, at 8.—The Theory of Armature Reactions in Dynamos and Motors: James Swinburne.—Some Points in Dynamo and Motor Design: W. B. Esson. (Discussion.)

ROYAL INSTITUTION, at 3.—The Early Development of the Forms of Instrumental Music (with Musical Illustrations): Frederick Niecks.

FRIDAY, MARCH 14.

ROYAL ASTRONOMICAL SOCIETY, at 8.

ROYAL INSTITUTION, at 9.—The Glow of Phosphorus: Prof. T. E. Thorpe, F.R.S.

SATURDAY, MARCH 15.

SOCIETY OF ARTS, at 3.—The Atmosphere: Prof. Vivian Lewes.

ROYAL INSTITUTION, at 3.—Electricity and Magnetism: Right Hon. Lord Rayleigh, F.R.S.

SUNDAY, MARCH 16.

SUNDAY LECTURE SOCIETY, at 4.—A Trip to British Columbia—the Life of an Emigrant in North-West Canada (with Oxyhydrogen Lantern Illustrations): Dr. James Edmunds.

MONDAY, MARCH 17.

SOCIETY OF ARTS, at 8.—Some Considerations concerning Colour and Colouring: Prof. A. H. Church, F.R.S.

ARISTOTELIAN SOCIETY, at 8.—Symposium—The Relation of the Fine Arts to one another: B. Bosanquet, E. W. Cook, and D. G. Ritchie.

TUESDAY, MARCH 18.

ZOOLOGICAL SOCIETY, at 8.30.—On the South American Canidæ: Dr. Mivart, F.R.S.—A Revision of the Genera of Scorpions of the Family Buthidæ, with Descriptions of some New South African Species: R. I. Pocock.—On some Points in the Anatomy of the Condor: F. E. Beddard.

SOCIETY OF ARTS, at 5.—Brazil: James Wells.

MINERALOGICAL SOCIETY, at 8.—An Account of a Visit to the Calcite Quarry in Iceland: J. L. Hoskyns Abrahall.—Mineralogical Notes: H. A. Miers.—The History of the Meteoric Iron of Tucson: L. Fletcher, F.R.S.

ROYAL STATISTICAL SOCIETY, at 7.45.—On Marriage-Rates and Marriage-Ages, with Special Reference to the Growth of Population: Dr. William Ogle.

INSTITUTION OF CIVIL ENGINEERS, at 8.—Lough Erne Drainage: James Price, Jun.

ROYAL INSTITUTION, at 3.—The Post-Darwinian Period: Prof. G. J. Romanes, F.R.S.

WEDNESDAY, MARCH 19.

SOCIETY OF ARTS, at 8.—Commercial Geography: J. S. Keltie.

ROYAL METEOROLOGICAL SOCIETY, at 7.—A Brief Notice respecting Photography in Relation to Meteorological Work: G. M. Whipple.—Application of Photography to Meteorological Phenomena: William Marriott.

ROYAL MICROSCOPICAL SOCIETY, at 8.—On the Variations of the Female Reproductive Organs, especially the Vestibule, in different Species of Uropoda: A. D. Michael.

UNIVERSITY COLLEGE CHEMICAL AND PHYSICAL SOCIETY, at 5.—The Manufacture of Aluminium by the Deville-Castner Process: F. A. Anderson.

THURSDAY, MARCH 20.

ROYAL SOCIETY, at 4.30.

LINNEAN SOCIETY, at 8.—The External Morphology of the Lepidopterous Pupæ; Part 2, the Antennæ and Wings: E. B. Poulton, F.R.S.—On the Intestinal Canal of the Ichthyopside with especial Reference to its Arterial Supply: Prof. G. B. Howes.

CHEMICAL SOCIETY, at 8.—The Evidence afforded by Petrographical Research of the Occurrence of Chemical Change under Great Pressures: Prof. Judd, F.R.S.

ZOOLOGICAL SOCIETY, at 4.

INSTITUTION OF ELECTRICAL ENGINEERS, at 8.

ROYAL INSTITUTION, at 3.—The Early Developments of the Forms of Instrumental Music (with Musical Illustrations): Frederick Niecks.

FRIDAY, MARCH 21.

PHYSICAL SOCIETY, at 5.—On the Villari Critical Point of Nickel: Herbert Tomlinson.—On Bertrand's Idiocyclophanous Prism: Prof. Silvanus Thompson.

INSTITUTION OF CIVIL ENGINEERS, at 7.30.—Economy Trials of a Compound Mill-Engine and Lancashire Boilers: L. A. Legros.

ROYAL INSTITUTION, at 9.—Electro-magnetic Radiation: Prof. G. F. Fitzgerald, F.R.S.

SATURDAY, MARCH 22.

SOCIETY OF ARTS, at 3.—The Atmosphere: Prof. Vivian Lewes.

ROYAL BOTANIC SOCIETY, at 3.45.

ROYAL INSTITUTION, at 3.—Electricity and Magnetism: Right Hon. Lord Rayleigh, F.R.S.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

The Reign of Law, 19th Edition: Duke of Argyll (Murray).—Recherches sur les Tremblements de Terre: J. Girard (Paris, Leroux).—The English Sparrow in North America: Dr. C. H. Merriam and W. B. Barrows (Washington).—Facsimile-Atlas to the Early History of Cartography: A. E. Nordenskiöld; translated by J. A. Ekelöf and C. R. Markham (Stockholm).—Birds' Nests, Eggs, and Egg-Collecting: R. Kearton (Cassell).—Force as an Entity with Stream, Pool, and Wave Forms: Lieut.-Colonel W. Sedgwick (Low).—Notes on Indian Economic Entomology (Calcutta).—National Academy of Sciences, vol. 4; Second Memoir, the Solar and the Lunar Spectrum: S. P. Langley.—Erläuterungen zu der Geologischen Uebersichtskarte der Alpen: Dr. F. Noë (Wien, Hölzel).—Journal of Morphology, vol. 3, No. 3 (Collins).—North American Fauna, No. 3: C. H. Merriam (Washington).—Himmel und Erde, Heft 6 (Berlin).

CONTENTS.

THURSDAY, MARCH 20, 1890.

A NATURALIST IN NORTH CELEBES.

A Naturalist in North Celebes. By Sydney J. Hickson, M.A. (Cant.), D.Sc. (Lond.), M.A. (Oxon. Hon.Caus.). With Maps and Illustrations. Pp. 392. (London: John Murray, 1889.)

THIS book is the outcome of the residence of a specialist for nearly a year upon a small island off the extreme north point of Celebes. Of books of travel there is in these days no lack, and so beaten are the paths along which authors for the most part lead us, that the reader in search of amusement or instruction not infrequently arrives at the index without having met with either. But Dr. Hickson's is not a book of travel: it is a record of a naturalist's life with an almost boundless submarine field for observation close at hand—albeit terrestrially somewhat limited—and when he leaves his coral-girt island, it is to wander in that little-known archipelago which links Celebes to the Philippines, the Sangir, Nanusa, and Talaut groups, whither few but adventurous Dutchmen have penetrated.

Of the fourteen chapters, three are devoted to Talisse, the island on which Dr. Hickson conducted his observations. Four are descriptive of his wanderings in the groups just mentioned, and the remainder for the most part treat of the Minahassa district, its natives, and their mythology and customs. Of these, the author tells us in his preface that "the greater part of the ethnological portion of the book is borrowed from the valuable writings to be found in many of the reports of missionary and other societies, and in Dutch periodicals."

Dr. Hickson owing his voyage almost entirely to a desire to study the corals of the Malay Archipelago, it is naturally to that part of the book which treats of them that we first turn. No one has ever yet done justice to the wonderful beauties of coral-land, and the author, in common with his predecessors, has failed—as everyone must fail—to convey to the untravelled reader an adequate idea of the appearance of a vigorous reef. Perhaps the very fact of being an authority has lessened his chance of success. The description is nevertheless a good one, and the chapter (vi.) the most important in the book. Dr. Hickson has wisely relegated his technical work to the publications of the various learned societies, but he tells us much of interest. The first sight of a coral reef at close quarters astonished him—specialist as he was:—

"I could not help gazing with wonder and admiration on the marvellous sight. . . . I had expected to see a wonderful variety of graceful shapes in the branching madrepores and the fan-like, feather-like alcyonarians, . . . but I was not prepared to find such brilliancy and variety of colour" (p. 15).

That vexed and most important question, the growth of coral reefs—a question upon which it was to be hoped that Dr. Hickson might be able, from the length of his stay and his varied opportunities, to enlighten us—is left pretty much where it was. We should be able to predict with certainty the direction and the rapidity of growth. As it is now, charts of coral islands and reefs become almost valueless in the course of a few years. But the causes both of growth and erosion are still undetermined. Much, no doubt, depends upon the rapidity of the tides. In strong tide-races no true coral reef is ever formed. "Flowing water, which is neither too swift nor too stagnant, bearing the kind of food necessary for the proper nourishment of the corals," is, as Dr. Hickson justly remarks, a strongly predisposing element to vigorous growth. Yet this is not always the case, neither does the converse always hold good; and we cannot agree entirely with the author when he says, "in deep bays or inlets, where tidal and ocean currents are scarcely felt, there is but little vigour in the reef." The inner harbour of Amboyna displays as rich a "sea garden," perhaps, as any in Malayan seas.

Dr. Hickson's daily work on the reefs led him to the certain conclusion that but one true species of *Tubipora* exists. The size of the tubes and the character of the septa—upon which most of the species are founded—are shown to be utterly without specific value; these differences depending entirely upon the position of the coral on the reefs. The following remarks upon a fact which must have struck most naturalists in tropic seas, but which we do not remember ever to have seen in print before, are worthy of quotation. Talking of sunrise and early morning, he says:—

"Not only are the birds and insects, which disappear as the sun becomes more powerful, particularly visible at that hour, but it is the time of day above all others when the surface of the sea teems with animal life. I remember well my disappointment when I first got into tropical waters at finding that my surface-net invariably came up almost empty. It was not until I had been at work some time that I made the very simple discovery that in the early morning hours every sweep of the net brings up countless pelagic forms of all sizes and descriptions" (p. 58).

The question of the food of corals is yet unsettled; but the author, after careful examination of polypes of various kinds, is inclined to the belief that many of them may be, partially at least, vegetable feeders. No doubt the water in the vicinity of mangrove-swamps is very largely charged with the *débris* of leaves and fruit and wood, some of which, sinking to the bottom, must enter the mouths of the polypes. Upon the mesenterial filaments of the Alcyonarians, indeed, particles of vegetable fibre are frequently found. It is suggested that the vigorous reefs frequently seen near extensive swamps, may be explained by such an hypothesis. Upon Darwin's theory of the formation of atolls, Dr. Hickson had little opportunity of forming an opinion—little, at least, until he visited the archipelagos already mentioned. He ultimately came to a disbelief in the general subsidence theory, and is not opposed to Mr. Murray's view—that coral reefs can, under favourable circumstances, grow out into deep sea-water upon the talus of their own *débris*.

Among many references to birds occurs an account (p. 41) of the existence of the maleo, or brush-turkey, in Ruang Island. Unfortunately, we are not told whether this is *Megacephalon maleo*, or the smaller *Megapodius gilberti*. They were most probably the latter; but it would be interesting to know, for the true Megacephalon of Celebes has never, we believe, been recorded as

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occurring in the smaller islands. Meyer's story of the whimbrels nesting on trees (probably *Numenius uropygialis*, Gould, by the way—not *N. phæopus*) is quoted, but without comment, and it is worthy of remark that no naturalist has as yet confirmed it. Dr. Hickson is not quite accurate in his statement that there are only two Celebean birds which are likewise English. He must often have noticed, in his rambles along shore, not only the common sandpiper, but also the wide-ranging *Strepsilas interpres* and one or more of the genus *Totanus*, which are not unfamiliar to us at home.

Perhaps one of the best passages in the book is that describing a mangrove-swamp, where the extraordinary conditions of life obtaining within its limits, and the interdependence of that tree and the coral reef, are well illustrated. The scenery of Talisse Island is not particularly beautiful, although the author does not tell us so; but that of the district of Minahassa on the mainland is strikingly lovely, and he describes the view of the Tondano Lake as one without an equal. It was unspoilt to him even by the thought of the "*heerendienst*"—that system of compulsory service which has acted as a red rag to so many Englishmen. Dr. Hickson is not so prejudiced, and is wise enough to recognize—as did Wallace—the enormous advantage which it has conferred upon the people.

"I cannot help thinking," he says (p. 208), "that everyone who is really acquainted with the circumstances of these colonies and the character and condition of the people must admit that it is a service both necessary and just. The Dutch Government has brought to the people of Minahassa not only the blessings of peace and security, but also the possibilities of a very considerable civilization and commercial prosperity. . . . In return for all this, it is only just that every able-bodied man should be compelled to lend a hand in maintaining this happy condition of affairs. In a land where the necessities of life are so easily obtained, . . . it would be impossible for the Government to obtain a sufficient number of them to labour on the roads at a reasonable wage."

The consequence is that they would be neglected. The *heerendienst*, then, as Dr. Hickson shows, is the only system possible, without overburdening the Exchequer, or increasing the taxation beyond the endurance of the people.

We have not space to dwell upon the description of the Sangir Islands, or on the mythology and customs of the natives of Minahassa, which Dr. Hickson has done well to put within the grasp of those who are unacquainted with the Dutch language. Among the folk-lore it is interesting to notice (p. 241) the story of Lumimuüt's impregnation by the west wind—a story which, if we mistake not, is almost identical with one of Egyptian source. The "swan-maiden" tale—which, perhaps, has as wide a distribution over the surface of the globe as any other—again occurs in Celebes. Enough has been said to show that "a naturalist in North Celebes" had a varied interest in his surroundings, which he has contrived to communicate to his readers with success. A little more care, perhaps, would have purged the volume of several misprints, and one or two instances of involved diction.

The woodcuts with which the book is furnished are well enough. We wish that anything could be said in favour of the "process" illustrations. That at p. 33 is bad, and another at p. 137 still worse. But anything more muddy and meaningless than that facing p. 45 we confess never to have seen.

F. H. H. GUILLEMARD.

SAINT-VENANT'S ELASTICAL RESEARCHES.

The Elastical Researches of Barré de Saint-Venant. (Extract from Vol. II. of Todhunter's "History of the Theory of Elasticity.") Edited, for the Syndics of the University Press, by Karl Pearson, M.A., Professor of Applied Mathematics, University College, London. (Cambridge: At the University Press. London: C. J. Clay and Sons. 1889.)

OUR fears lest this "History of the Theory of Elasticity" should, like Thomson and Tait's "Natural Philosophy,". remain a magnificent mathematical torso have been agreeably falsified by the early appearance of this instalment of the second volume. It is devoted entirely to the work of Saint-Venant, the distinguished French mathematical engineer.

Saint-Venant is one of the rare examples of a writer who is equally popular with the mere mathematician and with the practical engineer. To quote from the author's preface to this part of the "History of Elasticity," "we live in an age when the physicist awaits with not unreasonable excitement for greater revelations than even those of the past two years about the ether and its atomic offspring; but we live also in an age when the engineer is making huge practical experiments in elasticity, and when true theory is becoming an absolute necessity for him, if his experiments are to be of practical as well as of theoretical value." This is the opinion of the theorist; but the engineer points to his work as magnificent experiments on a gigantic scale, to which he invites the theorist to an inspection, for him to deduce his theoretical laws.

So far as pure theory is concerned, the engineer trusts only to Hooke's law, and Euler's theory of the beam, which neglects the warping of the cross-sections. But Hooke's law is shown by the testing-machine to be only a working hypothesis within very narrow limits of extension and compression, after which the baffling phenomena of plasticity make their appearance, and destroy all the simple mathematical harmony; while as to Euler's theory of the flexure of the beam, the editor, Prof. Pearson, is at present engaged on the mathematical discussion of the permissible limits of the application of the ordinary theory, and, so far, the result of his investigations (in the *Quarterly Journal of Mathematics*) is such as to strike dismay in the heart of the practical man who would be willing to apply his conclusions.

The purely mathematical theory of Elasticity is, at the present moment, in a very curious condition, for a subject in the exact science *par excellence*. Not only are elasticians divided into opposite camps of *multi-constancy* and *rari-constancy*, but we find a war of opinion raging among the most recent investigators—Lord Rayleigh, Chree, Love, Basset, and others. All are compelled to violate apparently the most fundamental rule of mathematical approximation; and, in considering the elasticity of a

curved plate, to begin by neglecting the terms depending on the stretching of the material, which involve the first power of the thickness of the plate, in comparison with the terms depending on the bending, involving the cube of the thickness; thus apparently neglecting the first power compared with the third power of small quantities. But, if we take a thin sheet of brass or iron in our hands, we shall find it quite easy to bend, but apparently impossible to stretch or shear in its own plane, showing that the stretching stresses may be considered as non-existent, by reason of requiring such large forces to produce them.

Before pure mathematical treatment can make much progress in Elasticity, much more experimental demonstration is required of the behaviour of pieces of metal of mathematical form under given applied forces; and such experiments can be carried out in testing-machines, now forming an indispensable part of a physical laboratory.

Saint-Venant's memoir on torsion, analysed in Section I., is familiar to us through its incorporation by Thomson and Tait, and shows that Saint-Venant carried out, with the comparatively crude methods at his disposal, valuable experiments, from which much theoretical deduction has been made; the analogues of the mathematical analysis in the problem of the torsion of the cylindrical beam of given cross section, and of the flow of viscous liquid through a pipe of the same section, or of the rotational motion of a frictionless liquid filling the cylinder being very striking. Prof. Pearson introduces great elegance and interest into the series which arise by a free use of the notation of hyperbolic functions, and we think there is still some interesting work for pure mathematicians in the identification of those series which are expressible by elliptic functions. But it certainly looks curious to find in § [287] the old familiar polar co-ordinates treated as mere conjugate functions, without reference to their geometrical interpretation.

Section II. is occupied with the analysis of Saint-Venant's memoirs of 1854 to 1864, in which he attacks such questions in practical elasticity as the longitudinal impact of bars, illustrated by very ingenious graphic diagrams, and also the conditions of stress of a cylindrical shell, in equilibrium under given applied internal and external pressures. This is the problem required in the scientific design of modern built-up artillery; and it is noticeable that Saint-Venant's solution differs materially from Lamé's, subsequently popularized by Rankine, the theory employed, as far as it will go, by scientific gun-designers all over the world.

The researches in technical Elasticity of Section III. arose in the annotations of Navier's "Leçons sur la Résistance des Corps solides"; the mantle of Navier descended on the shoulders of Saint-Venant, and ultimately the notes of Saint-Venant overwhelmed the original text of his master Navier; and, according to Section IV., Saint-Venant has practically done the same thing with Clebsch's "Elasticität."

Being the mathematical referee for all the difficult theoretical problems arising with the extensive use of the new materials iron and steel in architecture and engineering, Saint-Venant was provided with a number of useful problems on which to exercise his ingenuity; such as the impact of bars, the flexure of beams due to a falling weight or a travelling load, the critically dangerous speeds of fly-wheels and piston-rods, and so on; all problems hitherto solved by practical rule of thumb, the practical constructor encountering and opposing the difficulties without knowing why and how they arose.

Saint-Venant's investigations urgently need extension and application to the critically dangerous conditions which can arise in the stresses in artillery, when the dynamical phenomena are analysed, due to the sudden and periodic application of the powder pressure, and to the wave-like propagation and reflection of the stresses in the material. At present, we can only investigate the theoretical strain set up in the material of the gun by a steady hydrostatic pressure equal to the maximum pressure of the powder, employing Lamé's formulas, and then employ an arbitrary factor of safety, say 10, in the design of the gun, to provide against the contingencies of the dynamical phenomena we have not yet learnt how to discuss.

In the old times, before the Cambridge Mathematical Tripos was reduced to its present meagre curriculum, the examiner would have found the present volume very useful in suggesting good ideas, capable of testing reasonably the mathematical power of the candidates; at present, the chief class to profit by the present work are the practical constructors, who will learn where to look for the useful information on the narrow technical point which concerns them.

Prof. Pearson has brought his onerous task one step nearer to completion in this interesting volume, a monument of painstaking energy and enthusiasm.

A. G. GREENHILL.

GLOBES.

Hues's Treatise on the Globes (1592). Edited by Clements R. Markham, C.B., F.R.S. (London: Reprinted by the Hakluyt Society, 1889.)

THE Hakluyt Society has for its object the reprinting of rare or unpublished voyages and travels, and few are worthier of this honour than the "Tractatus de Globis" of Robert Hues. The author of this work was an intimate friend of Sir Walter Raleigh, and combined book-learning with practical knowledge gained by joining in some of the voyages to the New World with navigators whose names have made the sixteenth century famous. He strongly urged that his countrymen would have still further surpassed their Spanish and Portuguese rivals if they had "but taken along with them a very reasonable competency and skill in geometry and astronomy." In those days logarithms were unknown, and the solution of the problems of nautical astronomy required advanced mathematical knowledge. It was hoped that this difficulty would be overcome by the extended use of globes, which at once reduces these complex questions to approximate solution by inspection. After the construction of the Molyneux globes, Hues's treatise came into very general use, and no doubt played an important part in the explorations of the succeeding century.

It seems strange in these days, when a globe can be purchased for a few shillings, to read that only three centuries ago the construction of globes entailed such great expense that the liberal patronage of a merchant

prince was required before such an undertaking could be entered upon. Readers of Kingsley's masterpiece will not need to be reminded that the funds were supplied by "Alderman Sanderson, the great geographer and setter forth of globes." Emery Molyneux, a mathematician of whom little is known, was entrusted with the construction of the globes, but although several were manufactured and sold, only one set has been preserved, and this has found a strange resting-place in the library of the Middle Temple.

From the admirable introduction by the editor, we learn that the celestial preceded the terrestrial globe by many centuries. It has been asserted that Atlas, of Libya, discovered the use of the globe, and thus gave origin to the fable of his bearing up the heavens on his shoulders. There are several allusions to globes by the ancient writers, and on the medallion of the Emperor Commodus a celestial globe is clearly represented. None of the Greek or Roman globes, however, have been preserved. Amongst the oldest in existence are those made by the Arabian astronomers, dating from the thirteenth century. These are made of metal, on which the stars are engraved, and five of them are still with us, one belonging to the Royal Astronomical Society. The oldest globe, now at Florence, was constructed in 1070; and, though it is only 7·8 inches in diameter, 1015 stars are engraved upon it.

The terrestrial globe apparently dates from 1492. Baron Nordenskiöld points out that this is the first adoption of the notion of antipodes, and the first to show a sea-passage from Europe to India. The first map on which the name of America appears was found amongst the papers of Leonardo da Vinci at Windsor Castle; it is drawn on eight gores, and was probably intended for a globe. The next terrestrial globe of interest was that completed by Mercator in 1541, having a diameter of 16 inches. Others succeeded, and finally we come to the enlarged and improved globes constructed by Molyneux. These are twenty-six inches in diameter, and differ little in construction from our modern globes, but the geography, of course, differs very considerably.

The original work of Hues was in Latin, and went through several editions. Nine editions in Dutch and French followed, the most important being the Dutch one annotated by Isaac Pontanus. The latter was translated into English by John Chilmead in 1638.

The treatise is divided into five parts, the first dealing with things common to both globes, the second with planets and stars, the third with the geography of the terrestrial globe, the fourth with the use of the globes for purposes of navigation, and the fifth is a treatise on the use of rhumb lines, by Thomas Herriot. The book is especially interesting on account of the many references to the theories of the ancients and contemporaries, the whole forming a valuable history. The discussions of the size and shape of the earth are particularly striking. After giving the diverse opinions as to the length of a degree, the measures varying from 480 to 700 furlongs, the author concludes with the remark: "Let it be free for every man to follow whomsoever he please."

A geographical index at the end gives a long list of places, with their latitudes and longitudes, which has been reprinted with the hope that it may be of use in identifying old names. Longitudes in those days were measured from a point in the Azores, London thus having a longitude of about 26°.

Two other indices have been added, one a biographical index, and the other an index to the names of stars and constellations. Both of these are very complete, and will be of great interest to those wishing to learn a little about ancient astronomers and the origins of astronomical names.

A. F.

THE PSYCHOLOGY OF ATTENTION.

The Psychology of Attention. By Th. Ribot. Authorized Translation. (Chicago: The Open Court Publishing Company, 1890.)

IN this neat little volume of little more than a hundred pages we have a very careful and lucid consideration of *attention* from the standpoint of scientific psychology. Adopting the division of attention into two well-defined forms—the one spontaneous or natural (non-voluntary or reflex of Mr. Sully's "Outlines"), the other voluntary or artificial—Prof. Ribot devotes his first chapter to the former and his second to the latter. In a third he deals with "morbid forms of attention." These, with a short introduction and a short conclusion, constitute the compact little work. Although there is not very much that is, strictly speaking, new—and is this to be expected?—there is scarcely a page without some apt illustration, some pithy epigram, or some well-expressed generalization. It is a closely-reasoned and luminous exposition of a genuine piece of psychological work.

The four points on which the author lays most stress are the following:—(1) Attention is caused by, or has its origin in, emotional states. (2) Under both its spontaneous and voluntary manifestations it is, "from its origin on, bound up in motory conditions." (3) Intellectually it is a state of relatively perfect monoideism. (4) It has a biological value. Of these, the second is the most essential. The motor element in attention is the keynote of the whole argument. The emotions from which we start are not merely complexes of pleasurable or painful elements floating free in a purely mental atmosphere. They are the psychological accompaniments of certain activities or tendencies to activity. The pleasure and pain associated with these activities are "the hands of the clock, not its works"—or, to change the analogy, "they follow tendency as the shadow follows the body."

And as the motor element is present at the emotional initiation of attention so too is it present through every phase of its existence. The motor effect may, however, be manifested under either of two forms: it may be impulsive and produce movement; or it may be inhibitory and withhold movement. Attention accordingly means the concentration or the inhibition of movements; while its converse, distraction, means diffusion of movements. Steadily applied work is the concrete, the most manifest form of impulsive attention; steadily applied thought the ultimate goal of inhibitive attention; for, as Prof. Bain has well said, "To think is to refrain from speaking or acting." Such movements as are still requisite for continued life, such as those of respiration, are under strict control. The master-idea, so far as is

possible, drains for its own use the entire cerebral activity.

Attention from the first has had a biological value.

"Any animal so organized that the impressions of the external world were all of equal significance to it, in whose consciousness all impressions stood upon the same level, without any single one predominating or inducing an appropriate motory adaptation, were exceedingly ill-equipped for its own preservation."

Attention has thus been a factor in the progress of life, or, as Prof. Ribot puts it epigrammatically, attention is a condition of life. In the lower animals, under normal conditions, attention is for the most part spontaneous; or, to use the author's alternative term, natural. One may perhaps say that in natural or spontaneous attention the motive or interest is inherent, while in voluntary or artificial attention it is extraneous. And the process by which voluntary attention is developed is by rendering attractive by artifice what is not attractive by nature; by giving an artificial interest to things that have not a natural interest. This, too, is a factor in progress; this, too, has a biological value.

"In the course of man's development from the savage state, so soon as (through whatever actual causes, such as lack of game, density of population, sterility of soil, or more warlike neighbouring tribes) there was only left the alternative of perishing or of accommodating oneself to more complex conditions of life—in other words, going to work—voluntary attention became a foremost factor in this new form of the struggle for existence. So soon as man had become capable of devoting himself to any task that possessed no immediate attraction, but accepted as only means of livelihood, voluntary attention put in an appearance in the world. It originated, accordingly, under the pressure of necessity, and of the education imparted by things external."

We have thought it more just to our author, and more satisfactory to our readers, to give some account of Prof. Ribot's main theses with which we are in full sympathy, than to select minor points, of which there are but few, in which we differ from his conclusions. The translation is, on the whole, satisfactory, but some expressions, such as "the marrow and the bulb" (for the spinal cord and medulla), "moderatory centres," and "the fundament of emotional life rests in tendencies," &c., strike one as somewhat unusual.

C. LL. M.

OUR BOOK SHELF.

Handleiding tot de Kennis der Flora van Nederlandsch Indië: Beschrijving van de Families en Geslachten der Nederl. Indische Phanerogamen. Door Dr. J. G. Boerlage. Eerste Deel, Eerste Stuk. "Ranunculaceæ—Moringaceæ." Pp. 312. With an Index. ("Introduction to a Knowledge of the Flora of the Dutch East Indies." (Leyden: E. J. Brill, 1890.)

THIS is the first part of a work consisting of descriptions of the natural orders and genera of flowering plants represented in the Dutch East Indies. A work thus limited must necessarily be of limited utility; but we have Dr. Treub's testimony in a preface thereto that he regards it as a highly useful forerunner of a new Flora of the country. It is nearly five-and-thirty years since Miquel began publishing his "Flora," and the last part of it appeared in 1860, before Bentham and Hooker's "Genera Plantarum" commenced; and systematic botany generally has experienced extraordinary development since then. Further, one of the great advantages claimed for the present work is that it is wholly in Dutch. It is based on Bentham and Hooker's "Genera Plantarum," and we find on comparison that the ordinal, tribual, and generic definitions are to a great extent translations, though later additions to the flora, both in genera and species, have not been neglected. Dr. Boerlage's book will also be useful to the phytographer, as it is already something to have a synopsis of the genera found in the large eastern area under Dutch dominion. Geographically, the next descriptive "Flora" of the region should include the whole of "India aquosa," which means, at least, an examination of the plants of the whole of tropical Asia, of tropical Australia, and of Polynesia. Such a work, on lines similar to Hooker's "Flora of British India," would be of immense value; but it requires qualified men, with sufficient time, money, and ample materials from the whole area.

W. B. H.

The Elements of Laboratory Work. By A. G. Earl, M.A., F.C.S. (London: Longmans, Green, and Co., 1890.)

THIS volume is of such a character that the reader is at once tempted to seek for its excellences rather than for its weak points. It aims at presenting "an introduction to all branches of natural science," and is intended to be used as a hand-book in the laboratories of public schools that have well-equipped rooms devoted to practical science. The author says in his preface that such rooms "are nowadays considered a necessary part of all public schools and colleges." Granting that this is the case, that the teacher is good, and that his pupils are already highly trained and anxious to learn pure science for its own sake, this volume might be accepted as an excellent guide. It is marked by a total absence of the "familiar examples" which we have hitherto associated with elementary scientific works. The student is made to accustom himself to technical language from the very first. For example, "a set of *weights*," is, on p. 2, explained as being "a number of bodies so arranged," &c.; and a few paragraphs further on the student is directed to "verify the graduation of a burette," and is introduced to reading telescopes and cathetometers. The first introduction of the student to chemical changes is an experiment consisting of the ignition of silver nitrate with quantitative observations, the second experiment is similar but with silver iodate, and the third is the heating of silver nitrate in a closed tube over a small Bunsen flame. In an explanation of the significance of what are commonly known as atomic weights and molecular weights, the expressions atomic masses and molecular masses are used. We do not see the advantage of this novel nomenclature. If the volume had an index, we should be prepared to recommend it in unqualified terms for the use of school-boys who can carry out such instructions as the following: "Perform experiments illustrating the law that chemical combination takes place between definite quantities of different kinds of matter."

Magnetism and Electricity. Part II. Voltaic Electricity. By Prof. Jamieson, M.Inst.C.E., &c. (London: Griffin and Co., 1890.)

IF the third part of this work prove equal in excellence to the two already published, Prof. Jamieson may claim to have produced one of the best introductory text-books on the subject. Like its predecessor, Part II. treats the subject in an essentially practical way. A competent electrician himself, the author is well able to understand the difficulties which beginners are likely to meet with, and his attempts to make obscure things clear will probably be found highly successful. The theoretical side of the subject is carefully considered, and no important application of a principle is passed over without reference.

Instruments in actual use for what have now become every-day purposes are fully illustrated and described.

The book is well up to date both in the experimental and applied branches. Mr. Shelford Bidwell's apparatus for studying the changes in length of a bar during magnetization is described in such a way as to make the object of the experiment and the method of carrying it out easily understood. More of this kind of thing in our text-books is very desirable as showing that progress in a science is not made by chance, but is the outcome of careful thought on the part of patient investigators.

As a text-book for classes where experimental work is encouraged it is especially suitable, but we recommend it to the notice of all beginners. Numerous questions and specimen answers follow the various chapters, and an appendix gives instructions for making simple apparatus.

Astronomy with an Opera-Glass. By Garrett P. Serviss. (London and New York: D. Appleton and Co., 1889.)

WE are glad to welcome this, the second edition of a popular introduction to the study of the heavens. The author has surveyed, with the simplest of optical instruments, all the constellations visible in the latitude of New York, and carefully noted everything that seemed of interest to amateur star-gazers. In addition to the map and directions given to facilitate the recognition of the constellations and the principal stars visible to the naked eye, many facts are stated concerning the objects described which render the work a compendium of useful and interesting information—an astronomical text-book as well as a star-atlas. Similar combinations are very desirable introductions to every science, and offer the best means of extending true knowledge. To lead the student to Nature, and direct his attention to some of her marvellous works, to make him see natural phenomena intellectually, should be the basis of all scientific instruction, and works constructed on these lines read like story-books. With such works the one before us should be included, and there could hardly be a more pleasant road to astronomical knowledge than it affords; replete with information, elegant in design, easy of reading, and practical throughout, it deserves to rank high among similar guides to celestial phenomena. A child may understand the text, which reads more like a collection of anecdotes than anything else, but this does not mar its scientific value, and if the work multiplies the number of observers, as it is calculated to do, the dearest wish of every astronomer will be gratified.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Electrical Radiation from Conducting Spheres, an Electric Eye, and a Suggestion regarding Vision.

I DO not know how far the description of little isolated experiments is serviceable, but I am tempted to communicate a simple plan I use for exciting electric oscillations in dumb-bells, ellipsoids, elliptical plates, spheres, or other conducting bodies of definite geometrical shape unhampered by a bisecting spark-gap. I do it by supplying electricity to opposite ends of the conductor by means of Leyden jar knobs brought near enough to spark to it: said knobs being likewise connected with the terminals of a small Ruhmkorff coil. The charge thus supplied or withdrawn at every spark settles down in the conductor after a few oscillations, and these excite radiation in surrounding space.

There are many ways of arranging the Leyden jars: some more effective than others. The outer coats of the two jars may or may not be connected together. Connecting them in some cases brightens the sparks at short range, but seems to have a tendency to weaken them at long ranges. It is not difficult to surmise why this is so.

Of course, when the outer coats are disconnected, only an insignificant portion of the capacity of the jars is utilized; but unless the thing to be charged has too large a capacity it works perfectly well.

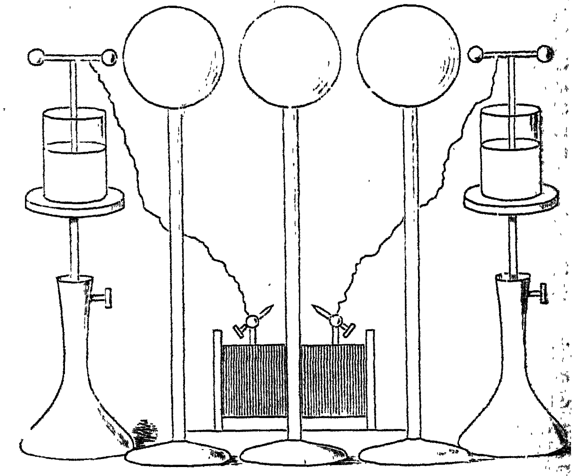
The receiver or detector is a precisely similar conductor touched to earth by a point held in the hand. The distance at which such a receiver responds is surprising. Or one may use a pair of similar conductors and let them spark into each other; but this plan is hardly so sensitive, and is more trouble.

The fact of being able in actual practice to get radiation from a sphere, is interesting, inasmuch as the subject of electrical oscillations in a perfectly conducting sphere has been worked out accurately by Prof. J. J. Thomson in the London Mathematical Society's Proceedings. I have not the volume by me, but I think he reckons the period of oscillation as the time required by light to travel 1·41 diameters of the sphere.

The case of spheres of ordinary metal will not be essentially different, with these rapid oscillations, for the electric currents keep to a mere shell of surface in either case; and in so far as *damping* affects the period, the dissipation of energy by radiation (which is common to both) is far greater than that caused by generation of heat in the skin of a metal sphere.

I happen to have four similar spheres of nickel-plated metal on tall insulating stems; each sphere 12·1 centimetres in diameter. Applying spark knobs to each end of a diameter of one of them, and applying the point of a penknife to another one standing on the same table at a distance of two and a half metres, I am able to get little sparks from it without using any reflector or intensifier.

Or arranging three spheres in a row, with intervals between and knobs outside, 5 short spark-gaps in all (see figure), and



using a fourth sphere as detector of this triple-sourced radiation, I draw little sparks from it to a touching penknife at a distance of 12 feet (366 centimetres, actual measurement).

In this case it may be a trifle better to hold one's hand near the receiving sphere at the side opposite to the penknife, and thus vary its capacity by trial so as to imitate the disturbing effect of the contiguous spheres in the transmitter.

The complete waves thus experimented on and detected are only 17 centimetres (six and a half inches) long, and I imagine are about the shortest yet dealt with.

But we do not seem near the limit set by lack of absolute suddenness in sparks yet, and are going on to try a large number of little globes.

Exciting a lot of little spheres by a coil in this way forcibly recalls to mind the excitation of a phosphorescent substance by a coil discharge.

And a receiver not very unlike the rod-and-cone structure of the retina can likewise be made. My assistant has been experimenting on a sort of gradated receiver which he made himself. I have recently had made a series of long cylinders with diameters ranging above and below 12 centims.; and th

length of each which responds to radiation is a kind of measure of specific intensity. They form (speaking sensationally) an electric eye with a definite range of colour sensation. It would be easy to supply it with a pitch or paraffin lens.

There is no need to suppose the retinal bodies to be conducting: a body of high refractive index should be subject to electric vibrations, and its surface to spurious electrifications, when radiation falls upon it; and the optical density of the rods and cones is known to be high. They may, however, be electrolytic conductors; and I find that a liquid sphere—*e.g.* a flask of inky water—responds to radiation, giving a glow to a point touching its glass.

The diameters of the rods, as measured by various physiologists, are not very different from dimensions adapted to respond to actual light-vibration frequency; and if this idea substantiates itself, these bodies can be supposed to constitute a sort of Corti's organ responding to etherial instead of to aërial vibrations, and stimulating in some still unknown, but possibly mechanical, manner, the nerve-fibre and ganglion with which each appears to be associated. OLIVER J. LODGE.

University College, Liverpool, March 11.

"Peculiar Ice-forms."

MAY I add another to the long series of communications which from time to time have been addressed to you under the above heading? Most of them have described and discussed the occurrence of ice in the form of filaments. One signed by J. D. Paul (NATURE, vol. xxxi. p. 264) seems (the description is somewhat vague) to refer to a mode of ice formation which is of somewhat frequent occurrence here, and is the only reference to this mode which I can find in that portion of the literature of physics which is accessible to me.

It happens now and again in our variable climate that a loose porous soil which has been thoroughly soaked with rain is made by a sudden and a sharp frost to produce a crop of little columns of ice. I observed a striking instance lately on a piece of hard compact ground, which, not being quite smooth, had been covered with an inch or so of loose pebbly soil for levelling purposes. Before the loose soil had been rolled or trampled upon, it became saturated with water through two days of continuous rain; and while it was still saturated, a sharp frost set in at night. In the morning the ground, to the extent of 60 square yards, was found to be covered with little columns of ice, some of them about two inches in length. They were roughly circular in section; and each column had approximately the same section throughout. Their diameters ranged from one-tenth to one-third of an inch. They were not transparent, but were whitish in appearance, and carried on their summits pebbles or frozen earth. They were thus obviously not ice crystals, such as Brewster describes in the *Edinburgh Journal of Science*, vol. ix. p. 122, as occurring in similar circumstances. The columns started from the ground at various inclinations to the vertical, and in the great majority of cases they curved upwards to a greater or less extent. I had never noticed this upward curving of the ice columns before, but other persons familiar with the phenomenon assure me they have observed it.

The explanation of this mode of ice formation seems pretty obvious. The sudden frost solidifies the crust of the soil; and it may therefore sometimes happen (in the above case it clearly must happen) that water becomes imprisoned between the frozen crust and the impervious sub-soil. Further freezing enables nature to perform Major Williams's experiment for us. If the crust does not give way as a whole, it must at its weak points; and the internal pressure is relieved by the protrusion of ice columns through apertures formed at these points. These columns would naturally carry portions of the crust on their summits, and during their protrusion might be expected to have innumerable minute fissures or cracks produced in them so as to exhibit a whitish snowy appearance. At the base of any column, at points where the freezing-point has been lowered by the pressure to the actual temperature, melting is continually occurring, and the water thus formed will flow into the fissures referred to. If the axis of the column is inclined to the vertical, and if we assume that the fissures and the points at which melting occurs are pretty uniformly distributed, more water will flow into the fissures of the lower side of the column than into those of the upper side. When the water re-freezes therefore, the lower side must elongate more than the upper, and the column must consequently in general curve upwards. That in exceptional cases the upward curving may not occur is obvious.

J. G. MACGREGOR.

Dalhousie College, Halifax, N.S., March 1.

On a Certain Theory of Elastic After-Strain.

IN a recent paper (Proc. Lond. Math. Soc., April 11, 1889), Prof. Karl Pearson has discussed at some length the possible forms of the additional terms which may be introduced into the general equations of elasticity by a consideration of the mutual action of the molecules and the ether, and has examined what physical phenomena may admit of explanation in this way. In particular, certain terms which thus appear admissible are made to yield a theory of the phenomenon known as "*elastische Nachwirkung*," or "after-strain." The attempt to explain such a comparatively slow process by the intervention of the ether certainly invites scrutiny, and in fact a very slight examination serves, I think, to show that the theory in question rests on a mistake. The author, after writing down the equations which (on his view) represent the steady application of stress to a portion of matter, proceeds to integrate them in the usual way by assuming a time-factor e^{mt}, and arrives at a quadratic in m^2 whose roots are μ/μ' and $(3\lambda + 2\mu)/(3\lambda' + 2\mu')$, where λ, μ are the ordinary elastic constants of Lamé, and λ', μ' are the coefficients of the additional terms referred to. He continues:—"Now m cannot be *positive*, so long at least as we are dealing with elastic-strain. For λ' and μ' are small as compared with λ and μ, the effects we are considering being only of the second order. Hence m^2 is large, and if m were positive the strain would rapidly grow immensely large, which is contrary to experience. Thus, we must give m the negative values $-\sqrt{(\mu/\mu')}$ and $-\sqrt{\{(3\lambda + 2\mu)/(3\lambda' + 2\mu')\}}$." The positive values of m are certainly inconvenient, but they are on the same footing with the negative ones; all are solutions of the author's equations, and all are required for the purpose of satisfying arbitrary initial conditions. The proper inference is surely that the substance is unstable, so long as the constants μ/μ' and $3\lambda' + 2\mu'$ are (as the author has tacitly assumed them to be) positive. If, to avoid this disaster, we change the signs of these constants, we get circular instead of exponential functions, and all analogy to elastic after-strain of course disappears. In its place we have *vibrations* (not molecular, but "molar") whose period is intrinsic to the substance and independent of the dimensions of the portion considered. To what physical reality these may correspond I do not undertake to say. HORACE LAMB.

The Owens College, March 4.

Foreign Substances attached to Crabs.

IF, as Mr. Garstang seems to suppose, the presence of tunicates on a crab is to be regarded as a danger-signal to its enemies, then *Hyas* must belong equally to both his groups α and β. I have found simple tunicates (*A. sordida*) on two small specimens of *H. coarctatus*. In one example they almost completely hid, and several were larger than the crab. I do not know if anyone has observed *Hyas* "dressing" itself with tunicates. I should think it was an operation of some difficulty, at least in the case of *A. sordida*, which adheres pretty tightly to stones and shells. It cannot be said to be brilliantly coloured, so that its assumption by *Hyas* might be regarded as only an adaptation for concealment, as in the case of *Algæ*—belonging, therefore, to group α. It seems to me, however, very doubtful whether a small *Hyas* would, even if it could, willingly burden itself with such a serious incubus as half a dozen tunicates. Probably their presence is in no way due to any act of the crab's.

The shore-crab, as pointed out long ago by Prof. McIntosh, frequently suffers loss of sight by the usurpation of its orbit by a growing mussel, and the Norway lobster has been found with one eye grown over by a Polyzoan. Such foreign bodies are surely rather hurtful than protective, and the same may perhaps be said of the tunicates on *Hyas*. It is also a question whether the crab likes the smell of tunicates any better than its neighbours.

I think Mr. Garstang is wrong in assuming the inedibility of tunicates. Prof. McIntosh, in "The Marine Invertebrata and Fishes of St. Andrews," speaks of *Molgula arenosa* as being found abundantly, and of *Pelonaia corrugata* as occasionally in the stomach of the cod and haddock; and Mr. W. L. Calder-

wood has found *Pelonaia* in some numbers in the intestine of the common dab.

Amongst anemones, *A. mesembryanthemum* is certainly a favourite food of the cod, and is not uncommon on the carapace of *Cancer pagurus*. It is difficult to see in what way the anemone is there protective to the crab. Both young crabs and anemones (of this and some other species) are equally preyed on by the cod; and though the crab may perhaps be big enough (as in a recent specimen 5 inches broad) to enjoy immunity from the cod's attack, yet, by parading such a gaudy bait, it must at least run the risk of a severe shaking. It may be added that, in the last-named case, the anemone quitted the crab, when moribund, for a more desirable basis.

ERNEST W. L. HOLT.

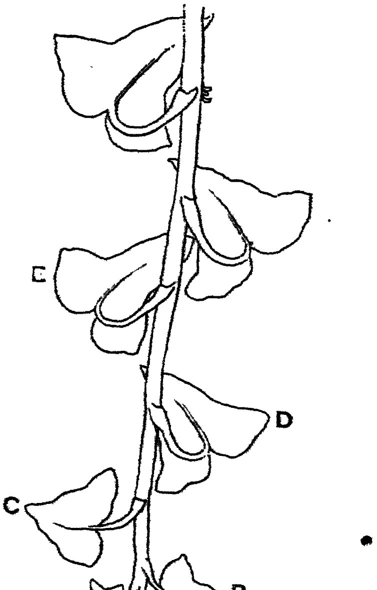
St. Andrews Marine Laboratory, N.B., March 9.

Abnormal Shoots of Ivy.

THE accompanying sketch represents a condition which is exhibited by a certain group of ivy plants in the neighbourhood of Plymouth. The plants are rooted upon the top of a high bank, which bounds the southern side of the road from Mount Edgcumbe to Tregantle; the branches pass downwards from the top of the bank on to its northern side.

The young shoots of each plant are conspicuous, because their leaves appear red, and so contrast strongly with the green of the older leaves. This appearance is due to the fact that the lower surface of each leaf is uppermost.

The sketch represents the terminal portion of a young shoot. The growing point is directed downwards. The three terminal leaves, A, B, C, have their upper surfaces directed upwards. The



leaves beyond these, however (D, E, &c.), are twisted in a two-fold way. First, each leaf-stalk is twisted on its own axis, so that the lower side of the leaf is directed upwards; and secondly, the apex of each leaf is rotated through 180°, so that it points away from the growing point of the shoot which bears it, towards the root.

This twisted condition is exhibited by about twelve or fourteen leaves on every young shoot—say, through a dozen inches from the growing point. The older leaves lose both kinds of torsion, so that each old leaf has its upper side uppermost, and its apex is directed towards the growing point of the stem. The under sides of the older leaves have completely lost their red colour.

The condition described is exhibited by all the shoots of a plexus of ivy plants just beyond the fifth milestone from Mount Edgcumbe, on the road above mentioned. It is absent in all the many bushes and creeping masses of ivy which grow on the same bank of the road between this point and Mount Edgcumbe. Whether all the plants composing the abnormal plexus are the offspring of a single parent cannot now be determined.

Plymouth, March 10. W. F. R. WELDON.

Earth-Currents and the Occurrence of Gold.

GOLD has been so large a factor in the prosperity and greatness of Australia, that the interesting subject of the origin of gold drifts and reefs must always possess to us something more than a purely scientific attraction. In the earlier days of the gold-fields there was among the diggers much speculation, of a scientific and semi-scientific nature, as to the processes by which Nature had produced the accumulations of coarse and fine gold dust which it was their business to extract from the alluvial drifts. The most obvious explanation, of course, was that the grains of gold had an origin similar to that of the *débris* and detritus of various characters which made up the alluvium itself; and this explanation seemed to harmonize so completely with the general processes of Nature that at one time it was almost universally accepted as the correct one. But many thoughtful mining authorities had their doubts upon the subject, and these doubts were not founded, as so frequently happens, upon mere prejudice, but were fortified by the fact that certain phenomena characteristic of the occurrence of drift gold were not only not explained by the "detrital hypothesis," as it is called, but were absolutely inconsistent with it. Chief among these objections may be mentioned the undoubted generalization that drift gold is nearly always purer than the gold in the reefs of the neighbourhood in which it occurs. No explanation as to the long distances to which grains of gold might be conveyed, or to the possible purifying effects of natural chemical action, made up any satisfactory explanation of the known facts, and accordingly under the detrital theory these facts had to remain shrouded in mystery. Then, again, it was a frequent occurrence for gold to be found so peculiarly embedded in pieces of wood, or in conjunction with natural crystals of minerals, such as the sulphides, that those who were constantly being brought into contact with such phenomena were firmly convinced that at all events there was a certain proportion of the gold found in alluvial drifts which had its origin in some other source than the breaking down of quartz reefs by the ordinary processes of Nature. The majority of those who held to this belief had at first but little scientific knowledge of natural reactions; and when questioned as to their theory on the subject, they were accustomed to say of the alluvial drift-gold, that it appeared to be actually growing—a statement which sometimes provoked, not unnaturally, a smile of pity for misplaced credulity.

These objectors, however, were right. Of this there is now scarcely the shadow of a doubt. It would be tedious to trace the steps by which such a strange conclusion has come to be virtually established. Suffice it to say that at the present day there are but few scientific men in Australia who have studied the subject who do not hold that by some agency or another the gold that is in our alluvial drifts has been formed, and probably is at present accumulating at the present moment, in its present position. It seems probable, indeed, that drift gold has its origin in the salts held in solution by the water by which it was formerly supposed to have been merely carried from one place to another. The most common salt of the precious metal is chloride of gold; and of this salt there is an appreciable quantity present in sea water along with the common sea salt, which, of course, is mainly chloride of sodium. In geological epochs, when the rocks of our present gold-fields were submerged below the ocean, and later on, when they held upon their surfaces vast imprisoned lakes of salt water, it is probable that they became saturated with sea water and retained large amounts of gold in solution. According to a computation quoted by Mr. Skey, the Government Geological Analyst for New Zealand, it is probable that every cubic mile of rock contains something like a million ounces of gold. Hence the underground streams of Australia, in certain districts, are particularly rich in salts of the precious metal, and there is an enormous area over which slight quantities of gold can always be obtained, while surface streams which are fed by deep-seated springs accumulate gold upon alluvial flats and hollows. Some of the gold found in such streams may undoubtedly be ascribed to the destruction of quartz reefs. It stands to reason that these reefs, like other rocks, must contribute to the *débris* in the beds of rivers and streams. But most of the purer alluvial coarse gold has evidently a different origin.

Up to this point, the new explanation of the origin of drift gold seems feasible, and, indeed, almost conclusive. The gold is present in minute quantities in the water of the drift, and this fact has been conclusively demonstrated experimentally by various investigators, among whom may be mentioned Messrs. Newberry and Skey. But it is one thing to prove that chloride of gold exists in the drift waters, and quite another thing to suggest in what manner and by what agency the precious metal has been reduced from its salt, and deposited in the form of coarse or fine grains or in that of large and strangely-shaped nuggets. Precipitation was the first and most obvious suggestion. The addition, for instance, of a minute quantity of sulphate of iron to a solution of chloride of gold would cause the formation of minute particles of metallic gold, and sulphate of iron, of course, is present in Nature abundantly. But such an explanation would only account for the formation of the very finest gold dust. It would give no solution of the origin of coarse gold and nuggets, nor would it account for any of the many peculiar anomalies of which I shall presently mention some striking examples.

In order to afford a possible extension of this purely chemical theory which might give a clue to the origin of nuggetty gold, it has been pointed out that if a crystal of some sulphide, such as iron pyrites, be immersed in a solution of chloride of gold, it will be covered with a film of metallic gold. Following the track of investigation thus apparently opened up, it has been ingeniously suggested that possibly the material of the metallic sulphide, and that of the golden film, may be regarded as a sort of miniature electric battery, in which the gold would form one anode and the pyrites the other. A current would pass between the two, and the result would be the deposition of metallic gold upon the film, at the same time that the material of the pyrites would continually become decomposed. The electroplater, in his laboratory, places the salt of gold in his bath, and uses an ordinary battery from which to obtain a current sufficiently strong to deposit gold upon the articles to be plated. But in this case it was suggested that the article to be plated, which was the film of gold itself, might be regarded as one of the elements supplying the current. The theory seems from the outset somewhat far-fetched, and it is open to very strong objections on the ground of improbability. The amount of material which the electroplater has to use up in order to deposit an ounce of gold is very considerable, even in the most efficient forms of batteries known to science. It is scarcely conceivable that a piece of pyrites, weighing about two pennyweights, would, by its decomposition, afford sufficient current to deposit an ounce of gold. Yet something of the sort would have to be established before it could be proved that electro-chemical action *in situ* supplies the electric current as a reducing agent.

In seeking for an explanation of the deposition of gold which would afford a surer or more probable basis for conjecture, I was at first mainly influenced by two remarkable facts which could hardly be referred to any imaginable phenomena of a chemical or electro-chemical origin. These were that in a drift supplying gold in abundance it is by no means uncommon to find a patch in which the gold gives out altogether, and is picked up further along the line; and the second was that there has always been observed at many of the leading goldfields a certain correspondence between the richness of the alluvial drifts and reefs and the points of the compass. The direction in which the richest drifts run may vary from one locality to another. But no matter how broken in contour the country may be, there is almost always a marked parallelism between the richest drifts.

Taking these and one or two other facts as a starting-point, I was led to form the hypothesis that the probable origin of the deposition of gold is to be found in thermo-electric earth-currents, probably generated by the unequal heating of the surface of the earth by the sun's rays in passing from east to west. This theory of earth-currents has attracted a good deal of attention in Australia, and it is remarkable how rapidly facts in support of it have been brought forward during the past few months. It would be impossible for me, within brief limits, to refer to all of these; but it will be of interest to summarize a few of the leading points:—

(1) The existence of earth-currents has been frequently demonstrated, and has attracted special attention since the invention of the telephone. In 1880, Prof. Trowbridge, of Harvard, conducted a series of experiments at the Observatory, and recorded it as one of his results that these currents appeared to be most pronounced along the water-courses.

(2) In Victoria remarkable instances of deflection of the compass have been particularly numerous, hinting at the presence of strong currents, more especially at the lines of junction between permeable and impermeable rocks.

(3) There is a remarkable relation between the conductivity of the adjacent rock country and the richness of an alluvial drift. Thus, in passing through slate or below an overhanging mass of basalt, the drift is generally richer than in passing through moist sandstone, suggesting that, where an earth-current is concentrated along the line of the water in consequence of the presence of rocks of low conductivity, the process of deposition has been facilitated.

(4) There are places at which the gold gives out altogether, although no discernible change has taken place in the nature of the country. These places seem to be the localities of a sort of short-circuiting, which we may readily suppose to take place very frequently in earth-currents.

(5) At particular pinched localities the current would be peculiarly strong, and would lead to the formation of nodules or nuggets of gold, the existence of which cannot be satisfactorily explained by any chemical theory hitherto advanced.

(6) Nuggets of an alloy of gold and copper have sometimes been met with, and the two metals have even been found to lie in alternate layers, suggesting that at one time a copper salt, and at another a gold salt, has been subjected to the action of a reducing current.

(7) In presence of a large amount of organic matter, it is almost invariably found that a drift becomes especially rich. The formation of acid by decomposition is what would be peculiarly required to facilitate the passage of an earth-current through the water of an underground drift, the existence of free acid being the requirement for an artificial electro-depositing bath.

(8) Conversely, the vicinity of large masses of calcite has been observed to be most inimical to the richness of a drift, and, of course, this could be explained by the fact that the carbonate of lime would destroy the free acid, and reduce the conductivity of the water so as to impede the transmission of a current.

(9) The peculiar shapes of the grains of what is known as coarse gold, are very suggestive of the action of a feeble current in piling up the metal upon the prominent portions, and leaving deep indentations between. Electric action of an extraneous nature is also strongly indicated by the strange strings and filaments which are constantly being met with.

(10) If we accept the crenitic theory of the origin of quartz reefs, the theory of earth-currents would at once apply with particular force to show how the action of such currents in hot siliceous solutions would produce a formation of gold simultaneous with that of quartz, thus accounting for the finely divided state of the gold in such reefs.

(11) At the same time it is necessary to account for the existence of the large masses of gold which are sometimes found associated with quartz, at places where the reefs become narrow in pinched localities. The theory of precipitation cannot account for these. But that of earth-currents would naturally lead us to expect the phenomenon, because in such a locality, while the formation of quartz would be retarded, the formation of gold would be accelerated by the concentration of the current as already explained.

The hypothesis is thus well supported by *primâ facie* evidence. For the experimental detection of earth-currents on goldfields I have strongly recommended the close observation of the most minute deflections of the magnetic needle, especially in underground workings. I believe also that the use of the telephone, as in Prof. Trowbridge's experiments, will be of great service in indicating the lines of greatest conductivity in the earth's crust, and in enabling us to decide whether these are identical in goldfields with those lines in which the drifts contain the richest gold.

GEORGE SUTHERLAND.

Angas Street, Adelaide, South Australia.

THE PRIMITIVE TYPES OF MAMMALIAN MOLARS.

SO much light has recently been thrown on the origin and mutual relations of the Mammalia by the labours of the Transatlantic palæontologists, that in the case of the limbs we have long since been able to trace the evolution of the specialized foot of the Horse from

that of the five-toed *Phenacodus* (see NATURE, vol. xl. p. 57). Till quite lately, however, we have been unable to follow the mode of evolution of the more complicated forms of molar teeth from a common generalized type, although Prof. Cope, by his description of the so-called "tritubercular" type of molar structure, paved the way for the true history of this line of research.

The common occurrence of this tritubercular type of dentition among the mammals of the Lower Eocene at once suggests that we have to do with a very generalized form of tooth-structure; and by a long series of observations Prof. H. F. Osborn, of Princeton, New Jersey, has succeeded, to a great extent, in showing how the more complicated modifications of molars may have been evolved from this generalized type. These observations are of so much importance towards a right understanding of the phylogenetic relationships of the Mammalia that a short summary cannot fail to be interesting to all students of this branch of zoology.

The tritubercular molar (Fig. A, 6), consists of three cusps, cones, or tubercles, arranged in a triangle, and so disposed that those of the upper jaw alternate with those of the lower. Thus, in the upper teeth (Fig. A, 7), there are two cusps on the outer side, and one cusp on the inner side of the crown; while in the lower teeth (Fig. A, 8, 8*a*) we have one outer and two inner cusps. This type, when attained, appears to have formed a starting-point from which the greater number of the more specialized types have been evolved. The Monotremes, the Edentates, perhaps the Cetaceans, and the extinct group of Multituberculata (*Plagiaulax* and its allies), must, however, be excepted from the groups whose teeth have a tritubercular origin.

It appears probable, indeed, that "trituberculism," as this type of tooth-structure may be conveniently termed, was developed from a simple cone-like tooth during the Mesozoic period, and that in the Jurassic period it had developed into what is termed the primitive sectorial

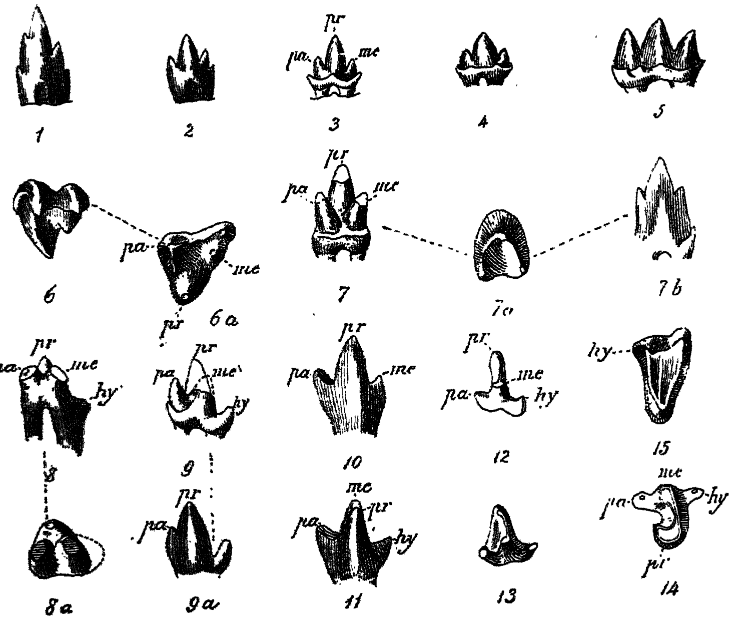


FIG. A.—Types of Molar Teeth of Mesozoic Mammals. 1–5, Triconodont Type (1, *Dromatherium*; 2, *Microconodon*; 3, *Amphilestes*; 4, *Phascolotherium*; 5, *Triconodon*). 6, 7, 10, Tritubercular Type (6, *Peralestes*; 7, *Spalacotherium*; 10, *Asthenodon*). 8–9, 11–15, Tuberculo-Sectorial Type (8, *Amphitherium*; 9, *Peramus*; 11, *Dryolestes*; 12, 13, *Amblotherium*; 14, *Achyrodon*; 15, *Kurtodon*). 6 and 15 are upper, and the remainder lower molars. *pa*, paraconid; *pr*, protoconid; *me*, metaconid; *hy*, hypoconid. In the upper teeth the termination ends in cone.

type (Fig. A, 9). The stages of the development of "trituberculism" may, according to Prof. Osborn, be characterized as follows:—

(1) The *Haplodont* type.—This is a hypothetical type at present undiscovered, in which the crown of the tooth forms a simple cone, while the root is probably in most cases single, and not differentiated from the crown.

(*a*) The *Protodont* sub-type.—This sub-type is a slight advance on the preceding, and is represented by the American Triassic genus *Dromatherium*. The crown of the tooth (Fig. A, 1) has one main cone, with fore-and-aft accessory cusps, and the root is grooved.

(2) The *Triconodont* type.—In this Jurassic type the crown (Fig. A, 4, 5) is elongated, with one central cone, and a smaller anterior and posterior cone situated in the same line; the root being differentiated into double fangs. *Triconodon*, of the English Purbeck, is the typical example.

(3) The *Tritubercular* type.—In this modification the crown is triangular (Fig. A, 7), and carries three main cusps or cones, of which the central one is placed internally in the upper teeth (Fig. A, 6), and externally in the lower molars (Fig. A, 7). The teeth of the Jurassic *Spalacotherium* are typical examples. In the first and second types the molars are alike in both the upper and lower jaws; but in the third or tritubercular type, the pattern is the same in the teeth of both jaws, but with the arrangement of the homologous cusps reversed. These features are exhibited in Fig. B.

These three types are regarded as primitive, but in the following sub-types we have additional cusps grafted on to the primitive tritubercular triangle, as it is convenient to term the three original cusps.

(*a*) *Tuberculo-sectorial* sub-type.—This modification of the tritubercular type is found in the lower molars, like those of *Didelphys*. Typically the primitive tritubercular triangle is elevated, and the three cusps are connected by cross ridges, while a low posterior talon or heel is added (Fig. A, 9). This modification embraces a quinquetubercular form, in which the talon carries an inner and

an outer cusp; while by the suppression of one of the primitive cusps we arrive at the quadritubercular tooth, bunodont tooth (Fig. C), like that of the Pigs. In the upper molars the primitive triangle in what is termed the secodont series may remain purely tricuspid. But by the development of intermediate tubercles in both the secodont and bunodont series a quinquetubercular form is reached; while the addition of a postero-internal cusp in the bunodont series gives us the sextubercular molar.

There is no doubt as to the homology of the three primary cusps in the upper and lower molars; and Prof. Osborn proposes the following series of terms for all the cusps above mentioned. The first secondary cusps (hypocone and hypoconid) respectively added to the upper and lower molars are also evidently homologous, and modify the crown from a triangular to a quadrangular form; but there is no homology between the additional secondary cusps of the upper molars termed protoconule and metaconule with the one termed ectoconid in the lower molars.

Terms applied to the cusps of molars:—

Upper Molars.

Antero-internal cusp .	=	Protocone	—pr.
Postero- ,, ,, or 6th cusp	=	Hypocone	—hy.
Antero-external ,, .	=	Paracone	—pa.
Postero- ,, ,, .	=	Metacone	—me.
Anterior intermediate cusp .	=	Protoconule[1]	—ml.
Posterior ,, ,, .	=	Metaconule	—pl.

Lower Molars.

Antero-external cusp .	=	Protoconid	—pr^d.
Postero- ,, ,, .	=	Hypoconid	—hy^d.
Antero-internal or 5th cusp .	=	Paraconid	—pa^d.
Intermediate, or antero-internal cusp (in quadritubercular molars)	=	Metaconid	—me^d.
Postero-internal cusp .	=	Entoconid	—en^d.

Having thus worked out the homology and relations of the tooth-cusps, Prof. Osborn gives some interesting observations on the principles governing the development

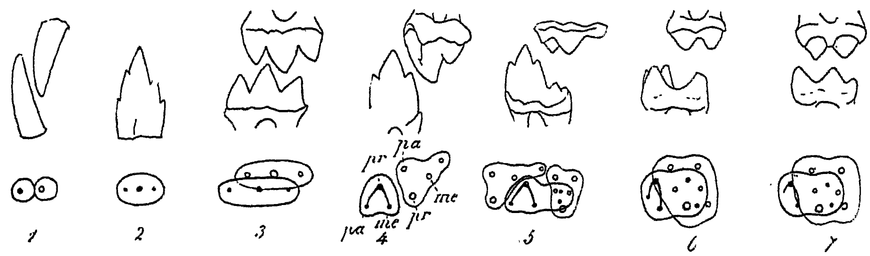


FIG. B.—Upper and Lower Molars in mutual apposition. 1, *Delphinus*; 2, *Dromatherium*; 3, *Triconodon*; 4, *Peralestes* and *Spalacotherium*; 5, *Didymictis*; 6, *Miocænus*; 7, *Hyopsodus*. Letters as in preceding figure.

of these cusps. It is considered that in the earliest Mammalian, or sub-mammalian, type of dentition (Haplodont), the simple cones of the upper and lower jaws

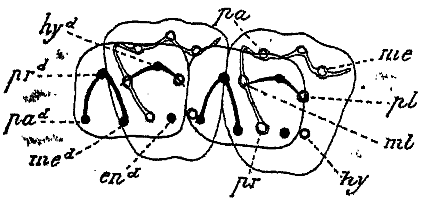


FIG. C.—Diagram of two upper and lower quadritubercular molars in apposition. The cusps and ridges of the upper molars are in double lines, and those of the lower ones in black. The letters refer to the table given above. The lower molars are looked at from below, as if transparent.

interlocked with one another, as in the modern Dolphins (Fig. B, 1). The first additions to the primitive protoconid appeared upon its anterior and posterior borders, and the growth of the para- and metaconids involved the necessity of the upper teeth biting on the outer side of the lower (Fig. B, 2), this condition being termed anisognathism, in contrast to the isognathism of the simple interlocking cones. In the typical tritubercular type (Fig. A, 7) it has been suggested that the para- and metaconids were rotated inwards from the anterior and posterior borders of the triconodont type; but it is quite possible that they may have been originally developed in their present position. By the alternation of the primitive triangle in the upper and lower jaws of the tritubercular type, the retention of an isognathous arrangement is permitted, the upper and lower teeth biting directly against one another.

Finally, Fig. C shows the mutual relations of the upper and lower teeth of the complicated quadritubercular molars, with the positions held by the primitive tritubercular triangles.

OXFORD "PASS" GEOMETRY.

ἀγεωμέτρητος μηδεὶς ἐνταυθοῖ εἰσίτω.

WHETHER poultry are to be regarded as descended from a primeval egg or a primeval hen, is a question on which some amount of scholastic ingenuity is supposed to have been exercised, and whether teachers or examiners are responsible for defective training in geometry is a question on which much might, more or less unprofitably, be said, and on which teachers and examiners may be expected to take different views. Happily for the mental equipment of the present generation of students, many teachers and examiners, avoiding barren controversy, have both laboured, as far as in them lies, to encourage soundness and thoroughness.

Probably, the old-world teachers who, hearing a "Euclid" lesson with the open Simson in their hand, looked upon "therefore" as an unwarrantable substitute for "wherefore," and could not be induced to accept "angle CAB" as a legitimate equivalent for what they saw in the text presented as "angle BAC," are fast disappearing, if not already extinct. Unfortunately, we are still under the influence of bad examination papers. Take, for instance, the papers set last year at Responsions. The sole directions from the examiner to the printer, necessary for getting these set up, might have been, and very likely were, as follows:—

Trinity.		Hilary.		Michaelmas.	
(1) I. 4	(6) I. 46	(1) I. 5	(6) I. 48	(1) I. 2	(6) I. 45
(2) I. 14	(7) II. 5	(2) I. 10	(7) II. 3	(2) I. 7	(7) II. 6
(3) I. 21	(8) II. 7	(3) I. 17	(8) II. 6	(3) I. 26	(8) II. 10
(4) I. 22	(9) II. 12	(4) I. 31	(9) II. 9	(4) I. 34	(9) II. 12
(5) I. 42	(10) II. 14	(5) I. 39	(10) II. 14	(5) I. 46	(10) II. 14

[1] The symbols *ml.* and *pl.* should properly apply respectively to the metaconule and protoconule, but since they bear the opposite signification in Fig. C, they are placed as above.

We believe that those qualified to give an opinion will agree as to the tendency of papers like these. They are direct incentives to learning propositions by rote—a practice to which beginners are by nature only too prone, without being encouraged by the grave authority of an ancient University: and they tend to paralyse any efforts a tutor may make to teach his subject intelligently. How is he to get pupils to listen to any discussion of difficulties, or to care for any deductions from the propositions, when they know as well as he does that not a mark can be gained by anything which goes beyond a bare knowledge of the Simsonian text?

Well might the Council of the Association for the Improvement of Geometrical Teaching, in its last Report, "regret to notice that the Euclid papers set for Responsions at Oxford still consist exclusively of bookwork," and remark that "the entire absence of riders or other questions designed to test the real knowledge of the student seems calculated to foster 'cram.'" The Council confined itself, as we have done, to the "Responsions" papers, but its remarks apply with equal force to "Moderations." The Euclid paper in the "First Public" and "Second Public" of Michaelmas 1889 are, in effect:—

"Write out IV. 1, III. 10, 3rd case of III. 35, III. 2, III. 25, III. 28, III. 12, III. 17, IV. 4, IV. 7.

"Define plane superficies, rhomboid, sector, similar segments, ratio, ex æquali.

"Write out the three postulates and the twelfth axiom.

"Write out I. 7, I. 29, I. 48, II. 12, III. 15, III. 26, IV. 6, VI. 5, VI. 18."

Though we regret the absence of "riders," we do not attach so much importance to it as to that of "other" questions arising naturally from the definitions, axioms, postulates, and propositions set to be written out: questions, for instance, on the redundancy of the definitions; on the distinction between the general and the geometrical axioms; on the axioms tacitly assumed by Euclid; on the truth or falsehood of the converse of a given proposition; on the interdependence of two contrapositives; or on the difficulties of Euclid's treatment of parallels.

It is instructive to contrast the Mathematical Responsions papers with those set in the classical part of the same examination. In these the University is by no means satisfied, as in the mathematical, with a knowledge which may be obtained by efforts of the memory alone, but applies the sharp test of prose composition and "unseens." To this inequality we draw the special attention of readers of NATURE. Compare the course open to a classical man with that which lies before one who intends to take his degree in science or mathematics. The classical man appears to have everything in his favour: he most likely knows enough mathematics to feel quite comfortable as to the paltry modicum required at Responsions. The other is in a very different position. If he has attained to anything like scholarship in his own subject, it will only be in rare cases that he can hope to get through Responsions without devoting a large amount of valuable time towards the acquirement of some facility in prose composition. We should like to see a vigorous protest by the science graduates against this anomaly.

PRZEWALSKY'S ZOOLOGICAL DISCOVERIES.[1]

WITH great satisfaction naturalists will observe that a complete account of Przewalsky's zoological observations and discoveries is to be given to the world, and has in fact been for some time in course of publication. The great Russian explorer, although perhaps best known in Western Europe as a geographical traveller, was at heart a naturalist, and one of no mean rank. Those who have read the narratives of his four great journeys will recollect how full they are of notes on the animals and plants met with during his routes. The specimens obtained by him and his companions were carefully preserved, and deposited in the Museum of the Imperial Academy of Sciences at St. Petersburg. Up to the present time these collections have only been made known to the public by various fragmentary accounts of them in scientific journals, and in the appendices to Przewalsky's volumes of travels, which were in many cases of the most unsatisfactory character. The Imperial Crown Prince Nicolas of Russia has now, however, placed at the disposal of the Imperial Academy, in whose Museum Przewalsky's collections are stored, a sum sufficient to cover the cost of the publication of a connected account of them. To no more worthy object could Royalty devote its income, and the resulting volumes promise to be alike a credit to the great nation to which Przewalsky belonged, and to form a very material contribution to zoological science.

As is almost the universal and necessary custom nowadays, the different branches of the collections to be investigated have been placed in the hands of different specialists. The mammals had been undertaken by Eugene Büchner, the Conservator of the Division of Mammals in the Academy's Zoological Museum. Herr Theodor Pleske, who has lately succeeded Herr Russow in the charge of the birds of the same Museum, supplies the portion of the work relating to the objects under his care. Similarly, to Herr S. Herzenstein have been assigned the fishes. Each section is prepared on a similar plan. The text is given in parallel columns of Russian and German. We cannot complain of a great national work like the present being published primarily in the national language, but our thanks should be given to the learned Academy for letting us have it also in a tongue generally understood by scientific men. The work is well illustrated, and the plates are excellently drawn, those of the mammals and birds mostly by Mützel, the well-known German lithographic artist. Up to the present time we have seen three parts of the mammals, one of the birds, and two of the fishes of this important work, which is a credit alike to the Academy which has produced it, and to the distinguished personage who has supplied the necessary means.

[1] "Wissenschaftliche Resultate der von N. M. Przewalski nach Central-Asien unternommenen Reisen: auf Kosten einer von seiner Kaiserlichen Hoheit dem Grossfürsten Thronfolger Nikolai Alexandrowitsch gespendeten Summe." Herausgegeben von der Kaiserlichen Akademie der Wissenschaften. Zoologischer Theil. (St. Petersburg, 1888-89.)

NOTES.

THE Chemical Society will this year for the first time hold its anniversary meeting (March 27) in the afternoon at 4 p.m., and the Fellows and their friends will dine together in the evening at the Whitehall Rooms, Hotel Métropole. It is hoped that the Fellows will signify their approval of this alteration by attending in considerable numbers.

A MEETING was held in Berlin on Monday, March 10, under the auspices of the German Chemical Society, to celebrate the 25th anniversary of the promulgation of Prof. Kekulé's theory of the constitution of the aromatic compounds. A very large number of chemists assembled in the Rathhaus in the afternoon. After an introductory address by the President, Prof. v. Hofmann, Prof. A. Bayer delivered a lecture in which he pointed out how completely modern investigations had confirmed Kekulé's views. A congratulatory address from the German Chemical Society was then presented to Prof. Kekulé. Prof. Armstrong attended on behalf of the London Chemical Society, Prof. Korner on behalf of the Italian chemists, Prof. Bischof on behalf of the Russian chemists; and besides the addresses presented by those representatives, there were very numerous letters

and telegrams of congratulation from various sources. Dr. Glover, on behalf of German artificial dye-stuff manufacturers, then presented a most admirable portrait of Prof. Kekulé which had been painted by the celebrated painter Angeli; this is to be placed in the Berlin galleries. Prof. Kekulé returned thanks in an eloquent address. Subsequently a banquet was held which was very numerously attended.

LORD RAYLEIGH has been elected a correspondent of the Paris Academy of Sciences in the department of physics.

THE discourse to be given by Lord Rayleigh at the Royal Institution on Friday evening, March 28, will be on "Foam."

MR. H. CARRINGTON BOLTON, the eminent American bibliographer, wishes to associate himself with those who recommend the system of Russian transliteration, explained lately in NATURE (p. 397). His letter was not received in time to permit of his name being included in the list of signatures.

THE visit of the Iron and Steel Institute to America is likely to be remarkably successful. At a meeting held the other day at New York, upon the invitation of Mr. Andrew Carnegie, a committee was appointed to arrange a reception for the members. The Philadelphia Correspondent of the *Times* says so many invitations have been received from various parts of the country that the belief is that the month given to the visit will be insufficient. The members will meet in New York. There will also be an international session at Pittsburg.

A STATED meeting of the Royal Irish Academy was held in Dublin on the 15th inst., at which the President and Council for the ensuing year were elected. Prof. Sollas, F.R.S., read a paper on the mica which occurs in well-formed crystals in the famous geodes of the Mourne Mountain granite: it was described as a lithium mica of the species Zinnwaldite. Most of the crystals possessed an exquisitely defined zonal structure, and in a single crystal a change in colour, density, composition, and in the magnitude of the angle of the optic axes could be traced on passing from the centre to the surface; this gradual transition from a more ferro-magnesian character near the centre to a more alumino-alkaline one near the surface was compared to the change from a more anorthite-like to a more albitic character, which accompanies the growth of many zonal felspars. This subject is also referred to in Prof. Sollas's paper on the granites of Leinster, which is to appear in the Academy's Transactions. The Report of the Council, giving the details of work done by the Academy during the past year, with notices of deceased members—among these John Ball, F.R.S., Sir Robert Kane, F.R.S., and Robert McDonnell, F.R.S.,—was read and adopted. Dr. E. Perceval Wright, Secretary to the Academy, was elected, in the place of the late Sir R. Kane, a visitor to the Museum of Science and Art, Dublin.

THE Royal Society of Medical and Natural Sciences of Brussels offers a gold medal of the value of 200 francs for the best essay on the influence of temperature on the progress, duration, and frequency of karyokinesis in an example belonging to the vegetable kingdom. The essay must be written in French, and must be sent in before July 1 to Dr. Stiénon, 5 Rue du Luxembourg, Brussels.

MR. J. WERTHEIMER, head master of the Leeds School of Science and Technology, has been elected to the head mastership of the Merchant Venturers' School, Bristol, the largest technical school in the West of England.

RECOGNIZING the difficulty experienced by Western naturalists in following the valuable scientific work now carried on in Russia, a number of influential men of science of that country have arranged for the publication of a monthly review—the *Vyestnīk Estestvoznaniya*. This will consist of original articles and short reports, with French *résumés*, and an index, in French, to Russian periodical scientific literature; the subjects included will be zoology, botany, physiology, geology, and microscopical technology, with the allied sciences. As, with the exception of Nīkītīn's admirable geological bibliography, no adequate attempt has been made to record Russian general scientific literature, this review will supply a very general want. The facts that it is published under the auspices of the St. Petersburg Society of Naturalists, and that the list of promised contributors includes most of the leading Russian naturalists, are sufficient guarantee for its value. The bibliographical index commences in the second number. The first consists of eight original articles. W. Wagner treats of the Infusoria of the body-cavity of Sipunculus and Phascolosoma; J. Wagner of some points in the development of Schizopods; Schīmkevīch of the alternation of generation in the Hydromedusæ; Borodīn and Tanfil'ev contribute botanical articles, the former discussing the nature and distribution of dulcite, and the latter the causes of the extinction of *Trapa natans*. Geology is represented by an account of the Devonian rocks of Mughodzhares, a criticism of Lévy's classification of the eruptive rocks by Polyenov, and an interesting account of the formulæ and relations of the different chemical types of the eruptive rocks by F. Levinson-Lessīng. The subscription to the review, it may be added, is 3 roubles 50 kopecks, and the office of publication, the Society of Naturalists, St. Petersburg University.

THE Vienna correspondent of the *Standard* telegraphed as follows on Monday:—"Dr. Eder, Professor of the Photographic Institute of Vienna, has announced that a photographer named Verescz, living in Klausenburg, Transylvania, has succeeded in solving the problem of photographing in natural colours. Up to the present, only the shades between deep red and orange can be retained, and even these, if exposed to the light, fade in from two to three days; but the experiments are being continued, with good prospects of complete success."

RECENTLY Lord Reay, the Governor of Bombay, laid the foundation-stone at Poona of a Bacteriological Laboratory, which is to be annexed to the College of Science in that town. Dr. Cooke, the Principal of the College, to whose efforts the establishment of the Laboratory is due, stated that it was originally intended that the study of the diseases of the lower animals in Poona should be directed to check the losses from anthrax in cattle by the introduction into India of protective inoculation. With this object two Bengal students at the Cirencester Agricultural College underwent a course of study at M. Pasteur's laboratory in Paris. One of these gentlemen devoted his attention entirely to sericulture, the other studied M. Pasteur's system of vaccination against anthrax. He returned to India, and has since conducted some experiments on cattle in Calcutta. Subsequently, Mr. Cooper, of the Veterinary Service, was deputed to M. Pasteur's Institute for instruction in the system of inoculation against anthrax. While in Paris, Mr. Cooper submitted a report, and explained that for the work in question a special laboratory would be required. At the same time he advocated the adoption of artificial gas for the culture-stoves and glass-blowing, and for the purpose of obtaining the high temperature required for sterilizing vessels, instruments, &c. Subsequent inquiry showed that anthrax is not the only contagious disease of a fatal nature with which the Indian cattle-owner has to contend. He has also to take into account rinderpest, tuberculosis, pleuro-pneumonia, and, in a minor degree, foot and mouth disease. It was, therefore, evident that if an institution was established for the preparation of an anthrax vaccine its value would be greatly enhanced if diseases other than anthrax could receive attention. The main objects of the Poona

Laboratory therefore are:—(a) The preparation of anthrax vaccine for despatch to districts where anthrax prevails. (b) The conduct of experiments in rinderpest with a view to the discovery of the pathogenic micro-organism of the malady, its cultivation in broth and other media, and attenuation, so as to provide a vaccine that shall give immunity to animals in rinderpest-infected districts. (c) Experimental research into the epizootic diseases generally of the ox and horse. (d) The instruction of trained native veterinarians in a proper method of performing vaccination and of the precautions necessary to avoid risk of septic infection.

On March 17, at six minutes past 11, a severe shock of earthquake was felt at Bonn, and reports from the surrounding districts on the following morning showed that it was very generally perceived in the vicinity of the town. On March 18, in the morning, a strong shock of earthquake was felt at Malaga and the neighbouring towns. The inhabitants were greatly alarmed, but no damage is reported.

According to a telegram sent from New York by Reuter's Agency on March 15, the captain of the steamer *Slavonia* reported having encountered a waterspout during the voyage from Europe. The vessel sustained no damage.

The Pilot Chart of the North Atlantic Ocean for the month of March states that the weather during February was much more moderate than during the two preceding months. An area of very high barometer extended over nearly the entire length of the Transatlantic steamship routes during the first five days. After this date the pressure fell, and gales of varying force were experienced from time to time. The most important of these storms was one south of Newfoundland on the 21st, whence it moved rapidly eastward. The storm on the 11th in about lat. 49° 30′ N., long. 22° W., was also of considerable energy. The most extensive fog bank reported during the month occurred on the coast from the 24th to the 26th, from Boston to Norfolk. The unprecedentedly large amount of ice this season has been the cause of considerable delay and damage to vessels; there are not only vast fields of ice, but also a very large number of bergs, some of which are of enormous dimensions. The importance of the knowledge of ice movements to navigation is recognized to be so great, that the Navy Department has, at the request of the U.S. Hydrographer, despatched an officer to Halifax and St. John's to collect information upon the ice movements during this season and past years, for the purpose of facilitating predictions of the general movements in future. A petition is also being drawn up for transmission to the Canadian Government to take such steps as they may deem advisable to obtain as thorough a knowledge as possible of the currents in the Gulf of St. Lawrence and adjacent waters, on account of their dangerous character during thick weather.

In the summary of a meteorological journal kept by Mr. C. L. Prince, at his observatory, Crowborough, Sussex, during 1889, he draws attention to the great preponderance of north-east wind over all other wind currents, and more particularly over that from the south-west, which has obtained during the last five years. He has examined his registers for the thirty-one years ending with 1889, and finds that between 1859 and 1883 there were only two years, viz. 1864 and 1870, in which the north-east wind has been in excess. In 1884 the north-east and south-west winds were nearly balanced, but during the last five years the average frequency has been north-east 102, south-west 72. Comparative observations would be interesting with the view of seeing whether this reversal of the ordinary conditions holds good for other stations. The Greenwich observations show that this great preponderance of north-east wind is not borne out there, at all events in all of the years mentioned.

Technical instruction, according to the *Times of India*, now takes a leading place in the educational programme of the Central Provinces. A year ago an entirely new curriculum was devised, whereby, among other changes, agricultural and engineering classes were established at Nagpore; the scholarship rules were revised with special reference to technical education; drawing-masters were appointed at a large number of schools, and every encouragement was given to the study of that subject; and new subjects of a technical and scientific character were grafted on to old school programmes. When the fact is taken into consideration that the year was one of transition, the progress made may be pronounced most satisfactory. Eleven students out of thirty who applied were admitted into the engineering class after a test as to general education. These did well, and most of them have entered on a second year's course. The agricultural class had an average strength of twenty-five throughout the year, the pupils working on the model farm and in the laboratory established in connection with this technical education scheme. No fewer than seventeen of the lads came through the ordeal of a strict examination at the end of the session. When it is remembered how largely the economic future of India will depend on the development of her agricultural resources, the value of this work, now fairly initiated in the Central Provinces, cannot be over-estimated.

In the current number of the *American Naturalist* Mr. R. E. C. Stearns continues his interesting series of papers on the effects of musical sounds on animals. One of his correspondents writes:—"Some time since I had an ordinary tortoise-shell cat, which had a peculiar fondness for the tune known as 'Rode's Air.' One day I chanced to whistle it, when, without any previous training, she jumped on my shoulder, and showed unmistakable signs of pleasure by rubbing her head against mine, and trying to get as near my mouth as possible. I have tried many other tunes, but with no avail." Captain Noble, of Forest Lodge, Maresfield, England, testifies that he formerly had a cat which displayed a corresponding sensitiveness, but it was only by plaintive tunes that she was affected. When such an air was whistled, she would climb up, and try to get her mouth as close as possible to that of the whistler. "I used as a rule," says Captain Noble, "to whistle the 'Last Rose of Summer,' when I wished her to perform. I never could satisfy myself as to her motive in putting her mouth to mine. The most feasible conjecture that I was able to make seemed to be that she imagined me to be in pain, and in some way tried either to soothe me, or to stop my whistling."

A paper on forestry in India and the colonies was read last week by Dr. W. Schlich before the Royal Colonial Institute. He said that for 700 years a gradual destruction of the forests of India had gone on. Under British rule the process had been hastened by the extension of cultivated and pasture land, and by the laying down of railways. After a time difficulty was experienced in meeting demands for timber, and in the early part of the century a timber agency was established on the west coast, while, in 1873, a teak plantation on a large scale was made at Nilambur. Through the energy of a few officials the matter was kept before the public, and in 1882 the Forests Department of Madras was entirely reorganized. Several Acts were passed to provide for the management of the forests under the protection of the State, and a competent staff of officers was provided, to be reinforced from time to time by those educated at Cooper's Hill College. Under the charge of the Department were some 55,000,000 acres of forest lands, and the figures relating to the cost of the work done were very satisfactory. Dr. Schlich then gave an account of the action of the Australian colonies with regard to the regulation of wooded lands by the State, contending that in no case had sufficient steps been taken to ensure a lasting and continuous supply of timber.

WE print to-day a review of Dr. Sydney J. Hickson's valuable work, "A Naturalist in North Celebes." It may be well at the same time to call attention to an "Album" which has been sent to us, containing reproductions of photographs taken in Celebes. The collection has been formed by Dr. A. B. Meyer, director of the Zoological and Ethnographical Museum of Dresden, and includes 37 plates, on which about 250 reproductions are printed. In 1870 and 1871 Dr. Meyer spent some time in Celebes, and the greater number of the photographs which have been reproduced he brought back with him. Others he has received from friends. We cannot say that the process employed has always yielded perfectly satisfactory results; nevertheless, the "Album" contains many representations that cannot fail to interest students of anthropology and ethnography. There are groups of portraits from northern, central, and southern Celebes, and any one who carefully studies them will find that they give him a very vivid idea of the various types of the native population. The tables are accompanied by short explanatory notices, some of the best of which are by Dr. J. G. F. Riedel, Utrecht. The work is edited by Dr. Meyer, and issued by Messrs. Stengel and Markert, Dresden.

MESSRS. MACMILLAN AND CO. have published a second edition of Sir John Lubbock's well-known "Scientific Lectures." The author includes in this edition the Presidential address read by him before the Institute of Bankers in 1879. The address contains many interesting suggestions as to the development of coinage, and is illustrated by two excellent plates representing ancient coins.

WE have received the fifth volume of "Blackie's Modern Cyclopædia," edited by Dr. Charles Annandale. The volume includes words from "Image" to "Momus," and the articles, so far as we have tested them, are, like those of the preceding volumes, concise and accurate.

THE Literary and Philosophical Society of Liverpool has published Nos. 41, 42, and 43 of its Proceedings. Among the papers printed, we may note "Life and Writings of the Hon. Robert Boyle," by Mr. E. C. Davies; "An Ideal Natural History Museum," by Prof. Herdman; "On the Remains of Temperate and Sub-Tropical Plants found in Arctic Rocks," by the Rev. H. H. Higgins: "Notes on the Cooke Collection of British Lepidoptera," by Mr. J. W. Ellis; "Lake Lahontan, an Extinct Quaternary Lake of North-West Nevada, U.S.A.," by Mr. R. McLintock; "On the Individuality of Atoms and Molecules," by the Rev. H. H. Higgins; note on the foregoing, by Prof. Oliver J. Lodge; "The Complete Analysis of Four Autopolar 10-Edra," by the Rev. T. P. Kirkman; and "On the Cradle of the Aryans," by Principal Rendall.

MR. FLETCHER, the well-known manufacturer of gas appliances, has just issued a little work of 70 pages on "Coal Gas as a Fuel" (Warrington: Mackie and Co.). Perhaps no one has given more attention to the subject than Mr. Fletcher, and his book is therefore of considerable importance. He gives an account of the precautions necessary to obtain the greatest efficiency in every case where coal gas can be applied—in the kitchen, bath-room, greenhouse, workshop, and laboratory. There is a useful chapter giving instructions to fitters with respect to flues and dimensions of service pipes. All who consume gas for purposes other than ordinary house illumination, will do well to read Mr. Fletcher's book.

A CURIOUS observation relating to influenza is quoted in *La Nature* from a Copenhagen journal. At the Royal Institution for education of deaf-mutes there, the pupils (about 70 boys and girls) have for seven years been regularly weighed every day in groups of 15 and under. This new experiment has yielded some interesting results. Thus it has been found that the children's growth in weight has occurred chiefly in autumn and in the first part of December; there is hardly any in the rest of winter and in March and April, and a diminution then occurs till the end of summer. Last year proved an exception. The curves of weight were quite like those of previous years till November 23. In the four weeks thereafter, while each child has usually gained on an average over 500 grammes, the girls last year gained nothing, and the boys only 200 grammes each (less than two-fifths of the normal amount). The contrast with 1888 was even more remarkable, 700 grammes having been the average four-weeks' gain in that year. There was no modification as regards food or other material conditions. Now, the influenza epidemic appeared in Copenhagen towards the end of November. While six of the professors at this institution were attacked, there were no pronounced cases among the pupils; but it is supposed that germs of the disease having entered the place, the struggle with these on the part of the children absorbed so much vital force that the organs of nutrition failed to give the normal increase of weight after November 23.

A REMARKABLE fall of a miner down 100 metres of shaft (say 333 feet) without being killed, is recorded by M. Reumeaux in the *Bulletin de l'Industrie Minérale*. Working with his brother in a gallery which issued on the shaft, he forgot the direction in which he was pushing a truck, so it went over and he after it, falling into some mud with about 3 inches of water. He seems neither to have struck any of the wood *débris*, nor the sides of the shaft, and he showed no contusions when he was helped out by his brother after about ten minutes. He could not, however, recall any of his impressions during the fall. The velocity on reaching the bottom would be about 140 feet, and time of fall 4·12 seconds; but it is thought he must have taken longer. It appears strange that he should have escaped simple suffocation and loss of consciousness during a time sufficient for the water to have drowned him.

AN extremely useful piece of apparatus has been devised by Prof. Lunge, and is described in the current number of the *Berichte*, by use of which all the troublesome reductions to standard temperature and pressure in the measurement of gas volumes may be avoided, the volume being actually read off corrected to 0° C. and 760 mm. pressure. The arrangement is at once simple and capable of adaptation to any form of gas apparatus. It consists essentially of three glass tubes, A, B, and C, arranged parallel to each other vertically, and all connected with each other below by means of a glass T tube and stout caoutchouc tubing. A is the measuring vessel, graduated in cubic centimetres; any gas measuring vessel, such as that of a nitrometer, or of a Hempel or other gas analysis apparatus, may be used for this purpose. It is closed at the top by the usual well-fitting stopcock, through which the gas to be measured is introduced in the ordinary manner. Below, the gas is enclosed by mercury which is poured down the tube C; Prof. Lunge terms this latter the pressure tube. The pressure tube is simply an ordinary straight glass tube of similar diameter and length to the measuring tube A, and open at the top. The tube B, called the reduction tube, is of about the same length, but of somewhat greater diameter in its upper half. This cylindrical expansion narrows again at the top, and terminates with a well-greased stopcock. A is firmly clamped to the stand, while B and C are held in spring clamps which permit of ready lowering or raising. The reduction tube B is then prepared as a reference tube, once for all, in the following manner. The stopcocks of A and B are opened, and mercury is poured down C until it rises nearly to the expanded portion of B. A drop of water is then introduced into B so that the enclosed air is saturated with aqueous vapour. The thermometer and barometer are next observed, and the apparent volume calculated of 100 c.c. of gas at 0° and 760 mm. A mark is then made upon the reduction tube B so that the volume of the tube between this mark and the stopcock is the

calculated apparent volume of the standard 100 c.c. The size of the tube is so arranged that this mark falls on the narrower portion of the tube, just below the expanded part. The pressure tube C is then raised or lowered until the mercury in B stands at the mark, when the stopcock at the top of B is closed. Thus a volume of air is enclosed which at 0° and 760 mm. and in the dry state would occupy exactly 100 c.c. In order to determine the corrected volume of a gas it is then only necessary to introduce it into the measuring tube A, allow it to cool to the temperature of the room, and then adjust B until the mark is a little higher than the mercury meniscus in A; C is next raised until the mercury in B rises to the mark, when B and C are finally simultaneously lowered until the level of the mercury in A and B is the same. The gas in A and the air in B are evidently equally compressed, and thus the volume read off upon the measuring tube A represents the corrected volume at 0° and 760 mm. The simplicity of the arrangement and the rapidity with which it can be worked are sure to recommend it for general use; and its applicability to the estimation of nitrogen in organic substances, which Prof. Lunge discusses in detail, will doubtless be especially appreciated by those who employ the volumetric method.

The additions to the Zoological Society's Gardens during the past week include two Red Tiger Cats (*Felis planiceps* jv.) from Malacca, a —— Fish Eagle (*Polioaëtus ichthyaëtus*) from the Himalayas, deposited; a Gayal (*Bibos frontalis* ♀), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

Objects for the Spectroscope.

Sidereal Time at Greenwich at 10 p.m. on March 20 = 9h. 53m. 31s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G.C. 2008	—	—	9 59 44	− 7 11
(2) π Leonis	5	Yellowish-red.	9 54 24	+ 8 34
(3) α Ursæ Majoris ...	2	Yellow.	10 57 0	+62 21
(4) β Ursæ Majoris ...	2	White.	10 55 12	+56 59
(5) D.M. + 68° 617 ...	6	Red.	10 37 26	+67 59
(6) R Virginis	Var.	Red-yellow.	12 32 55	+ 7 35·6
(7) U Boötis	Var.	—	14 49 15	+18 8·6

Remarks.

(1) This nebula is described in the General Catalogue as "Very bright; large; very much extended in a direction 45°; at first very gradually, then very suddenly much brighter in the middle to an extended nucleus." The spectrum of the nebula was observed by Lieut. Herschel in 1868, but his observations are not quite complete. He states that a continuous spectrum was suspected, and that there were probably no lines present. Further observations are obviously required.

(2) A star of Group II. Dunér states that the bands 2–8 are well seen, but that 4 and 5 are somewhat feeble. The spectrum is not strongly marked. The star is probably approaching the temperature at which the bands will be replaced by lines, and affords an opportunity of studying the order of the appearance of the lines.

(3) A star of the solar type (Gothard). The usual differential observations are required.

(4) A star of Group IV. (Gothard). The usual observations are required.

(5) One of the finest examples of stars with spectra of Group VI. Dunér states that the four bright zones and all the bands which he has numbered 1–10 are visible. In this star, band 6 is weaker than the other carbon bands. Band 5 is strong; 1, 2, and 3 are weaker; and 7 and 8 are visible with difficulty.

(6) This variable will reach a maximum on March 28. The period is about 146 days and the magnitudes at maximum and minimum 6·5–7·5 and 10–10·9 respectively (Gore). The spectrum is a remarkable one of the Group II. type, and the great range suggests the possible appearance of bright lines at maximum, as in R Andromedæ, &c., observed by Mr. Espin. Mr. Espin has noticed that in the variables, where F is very bright, the bright lines do not appear until some time *after* the maximum. It is therefore important to continue observations for a considerable period.

(7) No record of the spectrum of this variable appears to have been published. The period is about 176 days. The magnitude at maximum is 9–9·5, and that at minimum 13·5 (Gore). A maximum will be reached about March 23.

A. Fowler.

The Méguéia Meteorite.—This meteorite was observed to fall at Méguéia, in Russia, on June 18, 1889, and a short account of Prof. Simaschko's analysis of it is found in the current number of *L'Astronomie*. It is noted that the meteorite belongs to that remarkable division containing carbon in combination with hydrogen and oxygen. The meteorites of this class are Alais, 1806, Cold Bokkeveldt, 1838, Kaba, 1857, Orgueil, 1864, and Nogoya, 1880. The Méguéia meteorite is covered with a thin (0·5 mm.) crust, is black, partly dull and partly shiny, and somewhat friable. A microscopical examination showed dark grey specks distributed through the black mass, varying in size from a mustard-seed to a hemp-seed. These grey specks have a more or less chondritic structure, and are different in composition from the mass of the meteorite. Besides these chondrules, the greenish, semi-transparent particles of olivine are seen as in almost all other meteorites, whilst nickel-iron is disseminated through the mass in small grains, and occurs in a half-fused state on the crust. Account is also given of white angular scales, much resembling certain fossils, but this is not the first time that the chondrules with their eccentrically radiating crystallization have been mistaken for organisms. Like other carbonaceous meteorites, that of Méguéia has a bituminous smell.

The Velocity of the Propagation of Gravitation.—M. J. Van Hepperger, in a paper read before the Vienna Academy of Science, has assigned an inferior limit to the velocity of propagation of gravitation. It results from this limit that the time taken by gravitation to travel the radius of the earth's orbit does not exceed a second.

The Vatican Observatory.—The work to be undertaken at this new Observatory will be in connection with meteorology, terrestrial magnetism, seismology, and astronomy. The astronomical portion will mainly be directed to the photography of the sun and other celestial bodies, and to take part in the construction of the photographic map of the heavens, under the direction of the International Committee.

Double-star Observations.—Mr. S. W. Burnham, of the Lick Observatory, gives his sixteenth catalogue of double-stars in *Astronomische Nachrichten*, Nos. 2956–57. The observations were made in May, June, and July 1889, and 62 new pairs have been discovered and measured during this period.

Sun-spot in High Latitudes.—The *Comptes rendus* of the Paris Academy of Sciences for March 10 contains a short note by M. G. Dierckx, in which he states that he observed a sun-spot on March 4 in N. lat. 65°. If this were substantiated, it would be an almost unprecedented observation. But the photograph of the sun taken at the Royal Observatory, Greenwich, on that day, shows no trace of a spot in so high a latitude. A fine group did indeed appear on the sun on March 4, but its latitude was only 34°. This, however, is a very interesting circumstance, for though spots have been observed at considerably greater distances from the equator, they have usually been only small, and have lasted but a few hours, or two or three days at most. It would seem probable, therefore, this is the group which M. Dierckx observed, but that he made some error in determining its latitude.

GEOGRAPHICAL NOTES.

The limits of the ever-frozen soil in Siberia are the subject of a paper by M. Yatchevsky, in the *Izvestia* of the Russian Geographical Society (vol. xxv. 5). It is now generally admitted that Karl Baer's criticism of Middendorff's measurements in the Sherghin shaft at Yakutsk—from which measurements Middendorff concluded that the depth of frozen soil at Yakutsk reaches 600 feet—are well founded. The walls of the shaft, which was pierced seven years before Middendorff came to Yakutsk, had cooled in the meantime through the free access of cold air, and therefore a smaller increment of increase of

temperature with depth was found by Middendorff than would have been found if the measurements had been made in a shaft immediately after its being pierced. Nevertheless, the fact of the frozen soil extending to a great depth, especially in the valley of the Lena, is not to be contested; nor can there be any doubt as to the extension of frozen soil over large parts of Siberia. M. Yatchevsky attempts to determine its limits from general considerations about the average yearly temperature of separate regions, and the thickness of their snow-covering; and he gives a map of the probable southern limits of the frozen soil in Siberia, which do not differ much from the yearly isotherm of $-2°$ C. It must, however, be remarked that though the map approximately shows where the ever-frozen soil *may* be found beneath the thin layer of soil which thaws every summer, it ought not to be concluded that ever-frozen soil *will* be found everywhere within those limits. For instance, the granite rocks on the surface of the Vitrin plateau being immediately covered with immense marshes, the water from these marshes infiltrates into the rocks, and, while the marshes are covered during the winter with a crust of ice, their depths remain unfrozen. It may thus be considered certain that immense spaces will be found within the theoretical limits marked on the map, where no ever-frozen soil will be discovered. The Russian Geographical Society is sending out a series of questions, in the hope of obtaining accurate information, and it would be well if the same thing were done in Canada.

According to a letter from Iceland, dated Reykjavik, February 5, 1890, a translation of which is printed in the current number of the *Board of Trade Journal*, the population of Iceland during the four years from 1885 to 1888 inclusive has diminished by about 2400, the total number at the close of each of these years having been, in 1885, 71,613; in 1886, 71,521; in 1887, 69,641; and in 1888, 69,224. This diminution was greatest (1880) in 1887, the explanation for which may be sought in the enormous emigration to America which took place in that year. The diminution in the remaining years, though less sensible, must be attributed to the same cause, as in these years the number of births exceeded that of deaths. The chief diminution has been shown by the northern and eastern districts. The prefecture of Hunavatn in particular has fallen off in respect to inhabitants from 4800 in 1885 to 3785 in 1888. In Reykjavik, the capital, the population has risen from 3460 to 3599.

ATMOSPHERIC DUST.[1]

The infinitely small particles of matter we call *dust*, though possessed of a form and structure which escape the naked eye, play, as you are doubtless aware, important parts in the phenomena of nature. A certain kind of dust has the power of decomposing organic bodies, and bringing about in them definite changes known as putrefaction, while others exert a baneful influence on health, and act as a source of infectious diseases. Again, from its lightness and extreme mobility, dust is a means of scattering solid matter over the earth. It may float in the atmosphere as mud does in water, and blown by the wind will perhaps travel thousands of miles before again alighting on the earth. Thus Ehrenberg, in 1828, detected in the air of Berlin the presence of organisms belonging to African regions, and he found in the air of Portugal fragments of Infusoriæ from the steppes of America. The smoke of the burning of Chicago was, according to Mr. Clarence King (Director of the United States Geological Survey), seen on the Pacific coast.

Dust is concerned in many interesting meteorological phenomena, such as fogs, as it is generally admitted that fogs are due to the deposit of moisture on atmospheric motes. Again, the scattering of light depends on the presence of dust, and you may remember my showing you on a former occasion that beautiful experiment of Tyndall, illustrating the disappearance of a ray of light when made to travel through a glass receiver free from dust, whilst reappearing as soon as dust is admitted into the vessel. There is no atmosphere without dust, although it varies largely in quantity, from the summit of the highest mountain, where the least is found, to the low plains, at the seaside level, where it occurs in the largest quantities.

The origin of dust may be looked upon, without exaggeration, as universal. Trees shed their bark and leaves, which are powdered in dry weather and carried about by ever-varying currents of air, plants dry up and crumble into dust, the skin of man and animal is constantly shedding a dusty material of a scaly form. The ground in dry weather, high roads under a midsummer's sun, emit clouds of dust consisting of very fine particles of earth. The fine river and desert sand, a species of dust, is silica ground down into a fine powder under the action of water.

If the vegetable and mineral world crumbles into *dust*, on the other hand it is highly probable that dust was the original state of matter before the earth and heavenly bodies were formed; and here we enter the region of theory and probabilities. In a science like meteorology, where a wide door is open to speculation, we should avoid as much as possible stepping out of the track of known facts; still there is a limit to physical observation, and in some cases we can do no more than glance into the possible or probable source of natural phenomena. Are we on this account to give up inquiring for *causes*? This question I shall beg to leave you to decide, but where we have such an experienced authority as Norman Lockyer, I think the weight attached to possibilities and theories is sufficiently great to warant my drawing your attention for a few moments to the probable origin of the stars and of our earth.

I dare say many of you have read the interesting article in the *Nineteenth Century* of November last, by Norman Lockyer, and entitled "The History of a Star." The author proposes to clear in our imagination a limited part of space, and then set possible causes to work; that dark void will sooner or later be filled with some form of matter so fine that it is impossible to give it a chemical name, but the matter will eventually condense into a kind of dust mixed with hydrogen gas, and constitute what are called nebulæ. These nebulæ are found by spectrum analysis to be made up of known substances, which are magnesium, carbon, oxygen, iron, silicon, and sulphur. Fortunately for persons interested in such inquiries, this dust comes down to us in a tangible form. Not only have we dust shed from the sky on the earth, but large masses, magnificent specimens of meteorites which have fallen from the heavens at different times, some of them weighing tons, may be submitted to examination. From the spectroscopic analysis of the dust of meteorites we find that in addition to hydrogen their chief constituents are magnesium, iron, silicon, oxygen, and sulphur.

There are swarms of dust travelling through space, and their motion may be gigantic. We know, for instance, some stars to be moving so quickly that, from Sir Robert Ball's calculations, one among them would travel from London to Pekin in something like two minutes. From photographs taken of the stars and nebulæ, we are entitled to conclude that the swarms of dust meet and interlace each other, becoming raised from friction and collision to a very high temperature, and giving rise to what looks like a star. The light would last so long as the swarms collide, but would go out should the collision fail; or, again, such a source of supply of heat may be withdrawn by the complete passage of one stream of dust-swarms through another. We shall, therefore, have various bodies in the heavens, suddenly or gradually increasing or decreasing in brightness, quite irregularly, unlike those other bodies where we get a periodical variation in consequence of the revolution of one of them round the other. Hence, as Norman Lockyer expresses it clearly, "it cannot be too strongly insisted upon that the chief among the new ideas introduced by the recent work is that a great many stars are not stars like the sun, but simply collections of meteorites, the particles of which may be probably thirty, forty, or fifty miles apart."

The swarms of dust referred to above undergo condensation by attraction or gravitation; they will become hotter and brighter as their volume decreases, and we shall pass from the nebulæ to what we call true stars.

The author of the paper I am quoting from imagines such condensed masses of meteoric dust being pelted or bombarded by meteoric material, producing heat and light, which effect will continue so long as the pelting is kept up. To this circumstance is due the formation of stars like suns. Our earth originally belonged to that class of heavenly bodies, but from a subsequent process of cooling assumed its present character.

While apologizing for this digression into extra-atmospheric dust, I shall propose to divide atmospheric dust into *organic*, or *combustible*, and *mineral*, or *incombustible*. The dust scattered everywhere in the atmosphere, and which is lighted up in

[1] An Address delivered to the Royal Meteorological Society, January 15, 1890, by Dr. William Marcet, F.R.S., President.

a sunbeam, or a ray from the electric lamp, is of an organic nature. It is seen to consist of countless motes, rising, falling, or gyrating, although it is impossible to follow any of them with the eye for longer than a fraction of a second. We conclude that their weight exceeds but very slightly that of the air, and moreover, that the atmosphere is the seat of multitudes of minute currents, assuming all kinds of directions. Similar currents, though on a much larger scale, are also met with in the air. One day last June, from the top of Eiffel's Tower in Paris, I amused myself throwing an unfolded newspaper over the rail carried round the summmit of the tower. At first it fell slowly, carried away by a light breeze, but presently it rose, and, describing a curve, began again to fall. As it was vanishing from sight, the paper seemed to me as if arrested now and then in its descent, perhaps undergoing again a slight upheaval. Here was, indeed, a gigantic mote floating in the atmosphere, and subject to the same physical laws, though on a larger scale, as those delicate filaments of dust we see dancing merrily in a sunbeam.

I recollect witnessing at one of the Friday evening lectures of the Royal Institution in the year 1870 the following beautiful experiment of Dr. Tyndall, illustrative of the properties of atmospheric dust:—If we place the flame of a spirit-lamp or a red-hot metal ball in the track of a beam of light, there will be seen masses of dark shadows resembling smoke emitted in all directions from the source of heat. At first sight this appears as if due to the dust-particles being burnt into smoke; but by substituting for the spirit-flame or red-hot metal ball an object heated to a temperature too low to burn the motes, the same appearance of smoke is observed, hence the phenomenon is not owing to the combustion of the dust. The explanation, however, is obvious. The source of heat, by warming the air in its contact, and immediate proximity, made the air lighter and the motes relatively heavier, consequently they fell, and left spaces free from dust. These spaces in the track of the electric ray appeared dark, or looked as if full of a dense smoke, because the light of the ray could no longer be scattered in them from the absence of dust.

The motes were next examined by Tyndall, to determine whether they were organic or mineral. This was done by driving a slow current of air through a platinum tube heated to redness, and examining this air afterwards in a beam of light; it was then found to darken the ray, having lost the power of scattering light; therefore the dust had been destroyed or burnt by passing through the red-hot platinum tube, clearly showing its organic nature.

We breathe into our lungs day and night this very finely-divided dust, and yet it produces no ill effect, no bronchial irritation. Tyndall has again shown by the analytical power of a ray of light what becomes of the motes we inhale.

Allow me to return to the experiment with the red-hot metal ball placed in the beam of the electric light. Should a person breathe on the heated ball, the dark smoke hovering around it will at first disappear, but it will reappear in the last portions of the air expired. What does this mean? It means that the first portions of air expired from the lungs contain the atmospheric motes inhaled, but that the last portions, after reaching the deepest recesses in the organs of respiration, have deposited there the dust they contained.

It is difficult to say how much of the dust present in the air may become a source of disease, and how much is innocuous. Many of the motes belong to the class of *micro-organisms*, and the experiment to which we have just referred shows how easily these micro-organisms, or sources of infectious diseases, can reach the lungs and do mischief if they should find a condition of the body on which they are able to thrive and be reproduced. Atmospheric motes, although it has been shown that they are really deposited in the respiratory organs, do not accumulate in the lungs and air-passages, but undergo decomposition and disappear in the circulation. Smoke, which is finely-divided coal-dust, is clearly subjected to such a destructive process; otherwise the smoky atmosphere of many of our towns would soon prove fatal, and tobacco smoke would leave a deposit interfering seriously after a very short time with the phenomena of respiration.

Dust, however, in its physical aspect is far from being always innocuous, and, as you are aware, many trades are liable to suffer from it. The cutting of chaff, for horses' food, is one of the most pernicious occupations, as it generates clouds of dust of an essentially penetrating character. Those engaged in needle manufactures and steel-grinders suffer much from the dust of metallic particles. Stone-cutters, and workmen in plaster of Paris, coal-heavers, cotton and hemp spinners are also engaged in trades injurious to health because of the dust these men unavoidably work in. Those engaged in cigar and rope manufactures, or in flour-mills, hat and carpet manufacturers, are also liable to suffer for the same reason. A number of methods have been adopted, more or less successfully, to rid these trades of the danger due to the presence of dust. I shall not detain you on this subject, which would carry me too far, but merely bring to your notice the fact I observed many years ago, that charcoal has the power of retaining dust in a remarkable degree. I had charcoal respirators made of such a form as to cover both the mouth and nose, and containing about ½-inch thick of charcoal in a granular state. I could breathe through such a respirator in the thickest cloud of dust made by chaff-cutting without being conscious of inhaling any of the dust.

The subject of micro-organisms belongs to the science known as micro-biology. As meteorologists we are chiefly concerned with their distribution in the atmosphere. Micro-organisms are dust-like particles capable of cultivation or reproduction in certain media and at certain temperatures. If a particle of matter known to contain micro-organisms, also called *bacilli*, be placed on a clear surface of gelatine and maintained at a temperature favourable to its development, in a short time the gelatine will be found to contain a colony of those same *bacilli*. A fact so often stated as to become a medical truism is that there can be no infectious disease without the presence of the micro-organism special to that disease. Open cesspools, putrid meat or vegetable matter, accumulations of refuse, have no ill effects on health unless the micro-organisms of a certain disease, as those of typhoid fever or cholera, be present. On such foul decomposing matters these organisms thrive. They are reproduced with great activity, and become virulent in their effects.

Micro-organisms are scattered everywhere in the atmosphere. Dr. Miguel, at the Montsouris Observatory at Paris, has made an extensive inquiry into their distribution in air and water. In this country Dr. Percy Frankland has, with praiseworthy labour and perseverance, investigated the subject of micro-organisms, and ascertained their number in various localities. The result of his inquiry is that in cold weather, especially when the ground is covered with snow, the number of organisms in the air is very much reduced, and presents a very striking contrast with that found in warmer weather. The experiments made on March 9 show that during cold and dry weather, with a strong east wind blowing over London, a large number of micro-organisms may still be present in the air. It is particularly noticeable that even after an exceedingly heavy rain, and within a few hours afterwards, the number of micro-organisms in the air should be as abundant as usual. Taking an average of the experiments made on the roof of the Science Schools of the South Kensington Museum, the mean number of organisms found in 10 litres of air amounted to 35, while an average of 279 fell on one square foot in one minute. Other experiments made near Reigate and in the vicinity of Norwich present a marked contrast with those undertaken in the South Kensington Museum. There was a remarkable freedom from micro-organisms of the air collected on the heath near Norwich during the comparatively warm April weather, when the ground was dry. The air in gardens at Norwich and Reigate was richer in micro-organisms than that of the open country. Again, the number of organisms found in the air of Kensington Gardens, Hyde Park, and Primrose Hill was less than in that taken from the roof of South Kensington, but greater than in the country.

Experiments made in inclosed places, where there is little or no aërial motion, show the number of suspended organisms to be very moderate, but as soon as any disturbance in the air occurs, from draughts or people moving about, the number rapidly increases and may become very great. Experiments made in a railway carriage afford a striking example of the enormous number of micro-organisms which become suspended in the air when many persons are brought together.

Micro-organisms being slightly heavier than air, have an invariable tendency to fall, and on that account frequently collect on the surface of water; hence rivers, lakes, and ponds are constantly being thus contaminated. Micro-organisms in very pure water are not readily disposed to multiply, but traces of decomposing organic matter will induce their reproduction. One remarkable case occurs to me illustrating this fact. In 1884 a severe epidemic of typhoid fever broke out in the town of

Geneva, in Switzerland. The water of the lake in the harbour, which is surrounded by houses on three sides, was then examined by a distinguished micro-biologist, M. Fol, who discovered it to be full of micro-organisms; the water supplied to the town for drinking-purposes was taken from the River Rhone immediately as it flowed out of the harbour. The inquiry was pursued further, and it was found that just outside the harbour, on the surface of the water, there were still a number of micro-organisms, though less than in the harbour; but a few feet below the surface, say 3 or 4 feet, they had greatly diminished in number, indeed to such an extent that there were very few present. The obvious remedy was at once carried out. A wooden aqueduct was constructed, opening into the lake about 150 yards outside the harbour, and some 3 or 4 feet under the surface. As stated by Dr. Dunant, a Geneva physician who has given a very interesting account of this epidemic,[1] eighteen days after the source of the water-supply had thus been altered, a marked decline took place in the epidemic, and it was clearly being mastered. A similar epidemic due to a like cause occurred about the same time at Zurich.

There is one point connected with the properties of dust of organic origin which I think cannot fail to be of interest on the present occasion. I mean its inflammability, and its liability to explode when mixed with air. By *explosion* is meant that the propagation of flame by a very finely-divided material, such as coal-dust, mixed in due proportion with air, may proceed with a rapidity approaching the transmission of explosion by a gaseous mixture.

An interesting lecture was delivered on this subject at the Royal Institution, in April 1882, by Sir Frederick Abel, entitled "Some of the Dangerous Properties of Dust." The lecturer refers to instances of explosions in flour-mills, due in all probability to a spark from the grinding mill-stones, occurring in consequence of a deficient supply of grain to the stones.

Messrs. Franklin and Macadam, who investigated the subject, found that accidents of this nature were of frequent occurrence. In May 1878 a flour-mill explosion, quite unparalleled for its destructive effects, occurred at Minneapolis, Minnesota. Eighteen lives were lost, and six distinct corn-mills were destroyed. Persons who were near the scene of the calamity heard a succession of sharp hissing sounds, doubtless caused by the very rapid spread of flame through the dust-laden air of the passages inside the mill. The nearest mill to that first fired was 25 feet distance, and exploded as soon as the flames burst through the first mill. The explosion of the third mill, 25 feet from the second, followed almost immediately; and the other three mills, about 150 feet distance in another direction, were at once fired. The fire was attributed to a spark from friction of the mill-stones.

Coal-dust in coal-mines is a cause of accident from explosions, which has been closely investigated in this country, in Germany, and other mining districts. Sir Frederick Abel has given this subject especial attention, and brings it prominently forward in his valuable and exhaustive paper on "Accidents in Mines," read to the Institution of Civil Engineers in 1888. Some mines are, of course, more dusty than others, and coal-dusts are not all equally inflammable. That which is deposited upon the sides, top timbers, and ledges in a dry, dusty mine-way is much finer and more inflammable than the coarser dust which covers the floors. The lecture I have referred to alludes to the case of a considerable quantity of coal-dust accidentally thrown over some screens at a pit mouth bursting into flame as the dust cloud came into contact with a neighbouring fire, and burning a man very severely. There appears good ground for believing that fire may travel to a considerable extent through the workings of a mine from the ignition of coal-dust, as will be seen in the following account, extracted from Messrs. W. W. and J. B. Atkinson's book on "Explosions in Mines":—"An appalling accident happened at the Seaham Colliery, in the county of Durham, on September 8, 1880, at 2.20 a.m., causing the death of 24 men. An explosion occurred in the mine, and a loud report was heard at the surface, accompanied with a cloud of dust from the shaft, but no fire was seen. Owing to damage to the shaft it was more than twelve hours before a descent could be effected, and then a scene of destruction was witnessed by the explorers. Doors and air-crossings destroyed; tubs broken to pieces, and hurled one over the other; timber blown out, attended with heavy falls from the roof; and the bodies of men and horses in many cases terribly mutilated. The explosion was found to have extended over roads of an aggregate length of about 7500 yards, the greatest distance between the extreme points reached being about 3800 yards."

When discussing the cause of this terrible accident, Messrs. Atkinson remark that it was apparently impossible to account for the effects of the explosion on the assumption that it was due to fire-damp, as the presence of fire-damp was most unlikely to occur at any part at which the explosion could have happened; and therefore attention must be turned to coal-dust. There was coal-dust on all the roads traversed by the explosion, and there was coal-dust at the supposed point of origin. These facts are of striking significance. After the explosion, all parts of the mine in which its effects could be traced were covered on the bottom and on flat surfaces with a coating of fine dust, which, when examined under the microscope, appeared to have been acted on by great heat. This fine dust covered the surface for a depth of from $\frac{1}{8}$ to $\frac{1}{2}$ an inch and under. Dust of this kind was entirely absent on those roads over which the explosion had not extended. With reference to the original ignition, a shot had been fired apparently simultaneously with the explosion. The road at the place was of stone, and would probably be coated with the finest coal-dust; and, moreover, just above the spot where the fatal shot was fired were large baulks of timber, on which dust was plentifully stored. The shock caused by the explosion would throw the dust into the air, and the flame set fire to it. Thus initiated, the flame would extend through all the roads on which there was an uninterrupted supply of coal-dust to support it.

The second part of this address relates to inorganic or mineral dust. When on the Peak of Tenerife in 1878, engaged in a pursuit mostly of a physiological kind, I had occasion to use a very delicate chemical balance. My object was to determine the amount of aqueous vapour given out of the lungs while in the shallow crater at the summit of the Peak, 12,200 feet above the sea. The heat was intense, as the sun shed its nearly vertical rays at midday on the fine white volcanic sand spread over the floor of the crater. At various places rocks projected, covered here and there with crystals of sulphur, and so hot that the hand could scarcely bear coming in contact with them. Anticipating some difficulty in the use of the balance from the action of the wind, I had brought up with me a hamper and a blanket. After placing the hamper sideways, with the lid off, I proceeded, though not without some little trouble, to dispose the balance satisfactorily inside the basket; then, having thrown the blanket over the hamper, I stretched out at full length on the burning sand, nestling under the blanket, much as a photographer would cover himself and camera with a dark cloth. On trying to use the balance, it refused to act; its beam would not oscillate. A careful examination showed the instrument to be apparently in perfect order, when it occurred to me to wipe the knife-edges at the points of suspension of the beam and pans. The balance then worked quite well, though but for a few minutes only, again most provokingly declining to oscillate; indeed, it was only by constant wiping of the knife-edges that I succeeded with my experiment. The cause of my trouble was clearly the presence of very fine mineral dust in the air, of which my senses were utterly unconscious. Hence it is that extremely fine particles of mineral dust may exist in the atmosphere, while escaping detection by our senses, and such an occurrence is probably more frequent than generally thought.

Prof. Piazzi Smyth, while on the Peak of Tenerife, witnessed strata of dust rising to a height of nearly a mile, reaching out to the horizon in every direction, and so dense as to hide frequently the neighbouring hills. The Report of the Krakatão Commission of the Royal Society contains the following interesting account, p. 421 (Mr. Douglas Archibald's contribution to the Report):—"In 1881, Prof. S. P. Langley ascended Mount Whitney, in Southern California, with an expedition from the Alleghany Observatory; at an altitude of 15,000 feet his view extended over one of the most barren regions in the world. Immediately at the foot of the mountain is the *Inyo Desert*, and in the east a range of mountains parallel to the Sierra Nevada, but only about 10,000 feet in height. From the valley the atmosphere had appeared beautifully clear, but, as stated in Prof. Langley's own words, 'from this aërial height we looked down upon what seemed a kind of level dust ocean, invisible from below, but whose depth was six or seven thousand feet, as the upper portion only of the opposite mountain range rose clearly out of it. The colour of the light reflected to us from

[1] "Epidémie de fièvre typhoide à Genève en 1884," par P. L. Dunant, *Revue Médicale de la Suisse Romande*, 1887.

this dust ocean was clearly red, and it stretched in every direction as far as the eye could reach, although there was no special wind or local cause for it. It was evidently like the dust seen in mid-ocean from the Peak of Tenerife—something present all the time, and a permanent ingredient of the earthy atmosphere."

Dust Storms.—These storms, as suggested by Dr. Henry Cook, from whose paper to the Quarterly Journal of the Royal Meteorological Society, in 1880, I am now quoting, may be considered under three heads, according to their intensity—atmospheric dust, dust columns, and dust storms. Dr. Cook, alluding to occurrences in India, observes that there are some days on which, however hard and violently the wind may blow, little or no dust accompanies it; while on others, every little puff of air or current of wind forms or carries with it clouds of dust. If the wind which raises the dust is strong, nothing will be visible at the distance of a few yards, the sun at noon being obscured. The dust penetrates everywhere, and cannot be excluded from houses, boxes, and even watches, however carefully guarded. The individual particles of sand appear to be in such an electrical condition that they are ever ready to repel each other, and are consequently disturbed from their position and carried up into the air.

Dust columns are considered by Dr. Cook as due to electrical causes. On calm, quiet days, when hardly a breath of air is stirring, and the sun pours down its heated rays with full force, little eddies arise in the atmosphere near the surface of the ground. These increase in force and diameter, catching up and whirling round bits of sticks, grass, dust, and, lastly, sand, until a column is formed of great height and considerable diameter, which usually, after remaining stationary for some time, sweeps away across country at great speed. Ultimately it loses gradually the velocity of its circular movement and disappears. In the valley of Mingochar, which is only a few miles in width, and surrounded by high hills, Dr. Cook, on a day when not a breath of air stirred, counted upwards of twenty of these columns. They seldom changed their places, and, when they did so, moved but slowly across the level tract. They never interfered with each other, and appeared to have an entirely independent existence.

Dr. Cook describes as follows a dust storm which took place at Jacobabad:—"The weather had been hot and oppressive, with little or no breeze, and a tendency for dust to accumulate in the atmosphere. On the evening of the storm heavy clouds gathered and covered the sky. About 9 p.m. the sky had cleared somewhat, and the moon shone. A breeze sprang up from the west, which increased and bore along with it light clouds of sand. At 9.30 p.m. the storm commenced in all its fury. Vast bodies of sand were drifted violently along. The stars and moon were totally obscured. It became pitch dark, and it was impossible to see the hand held close to the face. The wind blew furiously in gusts, and heaped the sand on the windward side of obstacles in its course. Lightning and thunder accompanied it, and were succeeded by heavy rain. The storm lasted about an hour, when the dust gradually subsided. The sky again became clear, and the moon shone brightly. The storm appeared to have entirely relieved the electrical condition of the atmosphere. A pleasant freshness followed, and the oppressive sensation before mentioned was no longer experienced. This, indeed, is the general effect of storms in Upper Scind. The air is cooled, the atmosphere cleared, and the dusty condition of the atmosphere which usually precedes them for several days completely disappears."

In the case of a memorable sand storm which occurred at Aden on July 16, 1878, and recorded by Lieutenant Herbert Russell, there was a remarkable play of light on the objects which remained within sight. The sudden darkness from the storm gave a peculiar and ghastly tint to the white sand and neighbouring plain, while the curling masses of sand drifted before the gale, resembling a dark yellow smoke. The varied lights, quickly changing, were curious and most grand; the sea a clear green, and Slave Island and Shum-Shum, usually of an arid brown colour, became of an ashy white.

In a dust storm I experienced myself at Luxor, on the Nile, the suffocating effect of the sand as it drove into the lungs and air passages was very trying. People rushed to the immediate river side, where some relief was found.

A book on "Whirlwinds and Dust Storms in India," by P. L. H. Baddeley, Surgeon, Bengal Army, 1860, gives some interesting information on the electrical character of dust storms and dust pillars. When at Lahore in 1847, this gentleman was desirous of experimenting on the electrical state of the atmosphere in a dust storm, and with this object he projected into the air, on the top of his house, an insulated copper wire fixed to a bamboo; the wire was brought through the roof into his room, and connected with a gold-leaf electrometer, a detached wire communicating with the earth. A day or two after, during the passage of a small dust storm, he observed the occurrence of vivid sparks from one wire to the other, and, of course, strongly affecting the electrometer. He subsequently witnessed at least sixty dust storms of various sizes, all presenting the same kind of phenomena.

Volcanic Dust.—This dust consists mainly of powdered vitrified substances, produced by the action of intense heat. It is interesting in many respects. The so-called ashes or scories shot out in a volcanic eruption are mostly pounded pumice, but they also originate from stones and fragments of rocks which, striking against each other, are reduced into powder or dust. Volcanic dust has a whitish-grey colour, and is sometimes nearly quite white. Thus it is that, in summer, the terminal cone of the Peak of Tenerife appears from a distance as if covered with snow; but there is no snow on the mountain at that season of the year; the white cap on the Peak is entirely due to pumice ejected centuries ago. It is probably to this circumstance that the island and Peak owe their name, as in the Guelph language the words *Tener Ifa* mean *white mountain*.

The friction caused by volcanic stones and rocks as they are crushed in their collision develops a mass of electricity which shows itself in brilliant displays of branch lightning darting from the edges of the dense ascending column. During the great eruption of Vesuvius, in 1822, they were continually visible, and added much to the grandeur of the spectacle. It not unfrequently happens that dust emitted from Vesuvius falls into the streets of Naples; but this is nothing in comparison with the mass of finely-powdered material which covered and buried the towns of Pompeii, Herculaneum, and Stabiæ in the year 79.

On this occasion, according to the younger Pliny, total darkness from the clouds of volcanic ashes continued for three days, during which time ashes fell like a mantle of snow all over the surrounding country. When the darkness cleared away, the calamity was revealed in all its awful extent, the three towns having disappeared under the showers of dust.

The eruption of Krakatão, a mountain situated on an island in the Straits of Sunda, exceeded, in all probability, in its deadly effects, and as a wonderful phenomenon of Nature, the outburst of Vesuvius in the year 69. The Krakatão Committee of the Royal Society have collected and published in their interesting Report particulars of that memorable eruption, all of them thoroughly authenticated and reliable. The following is extracted from a communication to the Report by Prof. Judd:—"On August 26, 1883, it was evident that the long-continued moderate eruptions of Krakatão had passed into the paroxysmal stage. That day, about 1 p.m., the detonations caused by the explosive action attained such a violence as to be heard at Batavia and Buitzsenborg, about 100 English miles away. At 2 p.m. Captain Thompson, of the *Medea*, then sailing at a point 76 miles east-north-east of Krakatão, saw a black mass like smoke rising into the clouds to an altitude which has been estimated at no less than seventeen miles (nearly six times the height of Mont Blanc)."

If this surmise be correct, some idea of the violence of the outburst can be formed from the fact that during the eruption of Vesuvius in 1872 the column of steam and dust was propelled to a height of from 4 to 5 miles only.

At 3 p.m. the explosions were loud enough to be heard 150 miles away. At Batavia and Buitzsenborg the noise is described as being like the discharge of artillery close at hand. Windows rattled, pictures shook, but there was nothing in the nature of earthquake shocks—only strong air vibrations.

Captain Wooldridge, of the *Sir R. Sale*, viewing the volcano at sunset on the 26th, describes the sky as presenting a most terrible appearance, the dense mass of cloud of a murky tinge being rent with fierce flashes of lightning. At 7 p.m., when from the vapour and dust clouds intense darkness prevailed, the whole scene was lighted up by electrical discharges, and at one time the cloud above the mountain presented the appearance of an immense pine-tree, with the stem and branches formed of volcanic lightning. The air was loaded with excessively fine ashes, and there was a strong sulphurous smell. The steamer *G. G. Loudon*, within 20 or 30 miles of the eruption, passed through a rain of ashes and small bits of stone.

Captain Watson, of the ship *Charles Bal*, at a spot about a dozen miles off the island, records the phenomena of chains of fire appearing to ascend between the volcano and the sky, while on the south side there seemed to be a "continual roll of balls of white fire." These appearances were doubtless caused by the discharge of white-hot fragments of lava rolling down the sides of the mountain. From midnight till 4 a.m. explosions continually took place, the sky one second being intense blackness, the next a blaze of fire.

All the eye-witnesses agree as to the splendour of the electrical phenomena. Captain Woolridge, viewing the eruption from a distance of 40 miles, speaks of the great vapour cloud resembling an immense wall, with outbursts of fork lightning, like large luminous serpents, rushing through the air. After sunset, this dark wall assumed the appearance of a blood-red curtain, with the edges of all the shades of yellow—the whole of a murky tinge, and attended with fierce flashes of lightning. It was reported from the *Loudon* that lightning struck the mast-head conductor five or six times, and that the mud-rain which covered the masts, rigging, and decks was phosphorescent. The rigging presented the appearance of St. Elmo's fire, which the native sailors were busily engaged putting out with their hands, alleging that, if any portion found its way below, a hole would burst in the ship; not that they feared the ship taking fire, but they thought the light was the work of evil spirits, and that if it penetrated the hold of the vessel, the evil spirits would triumph in their design to scuttle the ship.

By these grand explosive outbursts the old crater of Krakatão was completely eviscerated, and a cavity formed more than 1000 feet in depth; while the solid materials thrown out from the crater were spread over the flanks of the volcano, forming considerable alterations in their forms.

The sea disturbance which accompanied the eruption of Krakatão was carefully investigated by Captain Wharton, Hydrographer to the Admiralty:—"The rush of the great sea wave over the land, caused by the violent abrasion in the crater, aided by the action on the water of enormous masses of fallen material, caused great destruction of life and poverty in the Straits of Sunda. By the inrush of these waves on land, all vessels near the shore were stranded, the towns and villages near the coast devastated, two of the lighthouses were swept away, and the lives of 36,380 of the inhabitants sacrificed. It was estimated that the wave was about 50 feet in height when it broke on the shore."

On the morning of the 27th, between 10 and 11 a.m., three vessels at the eastern entrance of the Straits encountered the fall of mingled dust and water, which soon darkened the air, and covered their decks and sails with a thick coating of mud. Some of the pieces of pumice falling on the *Sir R. Sale* were said to have been of the size of a pumpkin. All day on the 27th, the three vessels were beating about in darkness, pumice-dust falling upon them in such quantities as to employ the crew for hours in shovelling it from the decks and in beating it from the sails and rigging. At Batavia, 100 miles from Krakatão, the sky was clear at 7 a.m., but at 11 a.m. there fell a regular dust-rain; at 11.20 complete darkness pervaded the city. The rain of dust continued till 1, and afterwards less heavily till 3 p.m.

The speed and distance attained by the pumice ejected from the volcano may be conceived from the fact stated in Mr. Douglas Archibald's contribution to the Report, that dust fell on September 8, more than 3700 English miles from the seat of the eruption.

The great mass of the pumice thrown out during the eruption presented a dirty greyish-white tint, being very irregular in size. It was undoubtedly due to the collision of fragments of pumice as they were violently ejected from the crater; the noise produced was even more striking than the sound of the explosion.

The dust ejected from Krakatão did not all fall back at the same time upon the sea and earth; as the lightest portions formed into a haze, which was propagated mostly westward. Mr. Archibald states in the Report that most observers agree upon considering this haze as the proximate cause of the twilight glows, coloured suns, and large corona, which were seen for a considerable time after the eruption. The haze was densest in the Indian Ocean and along the equatorial belt, and was often thick enough to hide the sun entirely when within a few degrees from the horizon.

And now, ladies and gentlemen, I must bring this address to a conclusion, and thank you for having followed me over a long, dusty track. I hope I have succeeded in showing that infinitely small objects, no larger than particles of dust, act important parts in the physical phenomena of Nature, just as small and apparently unimportant events occasionally lead to others of the greatest magnitude.

SOCIETIES AND ACADEMIES.

London.

Royal Society, March 6.—"The Cranial Nerves of the Torpedo" (Preliminary Note). By J. C. Ewart, M.D. Communicated by Prof. M. Foster, Sec. R.S.

The cranial nerves of the torpedo agree in their general arrangement with those of the skate.[1] The ophthalmicus profundus occupies the usual position, but its ganglion lies in close contact with the Gasserian, and not on a level with the ciliary ganglion. The trigeminus has the usual distribution, for, notwithstanding the statements in the most recent text-books,[2] the trigeminus sends no branch to the electric organ. The facial complex includes the superficial ophthalmic, the buccal, and the hyomandibular nerves, all of which have the same distribution as the corresponding nerves in the skate; but the hyomandibular includes or is accompanied by a large bundle of nerve fibres which supply the anterior and inner portion of the electric organ. This large nerve cord (the first electric nerve) has hitherto almost invariably[3] been described as a branch of the trigeminus. When traced backwards, it is found to spring from the anterior portion of the electric lobe.

The glossopharyngeus, a slender nerve in the skate, is represented in the torpedo by a thick cord which escapes by a large foramen in the outer wall of the auditory capsule. This large nerve consists of two portions, one of which is small and completely covered by the large superficial division. The small deep division, which in its course and distribution closely resembles the glossopharyngeal in the skate, presents on leaving the auditory capsule a distinct ganglionic swelling, beyond which it breaks up into the branchial and other branches. The large superficial division emanates from the electric lobe behind the origin of the first electric nerve, and at once runs outwards to reach and supply the majority of the columns of the anterior half of the electric organ.

The vagus complex consists of the nervus lateralis, the nervus intestinalis, and of five branchial nerves, of which the two anterior are accompanied by the third and fourth electric nerves. The nervus lateralis, lying superficial to all the other nerves, arises on a level with the root of the glossopharyngeus, and then curves backwards dorsal to the posterior electric nerve to reach the canal of the lateral line. Shortly after leaving the cranium it presents a distinct ganglionic swelling, which is crowded with large cells. The four branchial nerves for the four vagus branchiæ, the slender filament which represents a sixth branchial nerve, and the intestinal nerve lie at first in contact with each other under cover of the third and fourth electric nerves. When the branchial and intestinal nerves are carefully examined, they are found to present four, sometimes five, ganglionic enlargements, and in addition ganglionic cells can sometimes be detected at the proximal end of the slender sixth branchial nerve. The third and fourth electric nerves lie over and are especially related to the second and third branchial nerves. These large electric nerves spring from the posterior half of the electric lobe, and find their way outwards partly behind and partly under the auditory capsule, to terminate in the posterior half of the electric organ.

It thus appears that all the electric nerves spring from the electric lobe, that the first accompanies the hyomandibular division of the facial complex, the second the glossopharyngeus, and the third and fourth the first two branchial nerves of the vagus complex. It remains to be seen whether the electric nerves have been derived from motor branches of the nerves with which they are respectively associated by an enormous increase in the number of their fibres, as the muscular fibres were gradually transformed into electric plates.

Physical Society, Feb. 21.—Prof. G. Carey Foster, F.R.S., Past-President, in the chair.—The following communications were read:—On a carbon deposit in a Blake telephone trans-

[1] Ewart, "On the Cranial Nerves of Elasmobranch Fishes," Roy. Soc. Proc., vol. 45, 1889.

[2] *E.g.*, McKendrick, "Text-book of Physiology," 1888, and Wiedersheim, "Grundriss der vergleichenden Anatomie," 1888.

[3] Fritsch is the only author I am acquainted with who does not describe the first electric nerve as a branch of the trigeminus, "Untersuchungen ueber den feineren Bau des Fischgehirns," Berlin, 1878.

mitter, by Mr. F. B. Hawes. The author exhibited photographs of the interior portions of the transmitter on which the deposit had taken place. These portions consist of a metal diaphragm, a highly-polished carbon button, and a platinum contact piece carried by a German silver spring placed between them. The diaphragm presented a mottled appearance due to the deposit, but the part which had been behind the German silver spring seemed comparatively clean. The deposits on the carbon button and German silver spring were much less dense than that on the exposed parts of the diaphragm, and the space near the point of contact between the platinum and carbon was free from deposit. The deposit was fairly adherent, some rubbing being necessary to remove it, and on examination under the microscope particles of copper and metallic crystals could be seen. The author believes the deposit due to some kind of bombardment of carbon particles, but was unable to say why it should occur, or why the varnished diaphragm should receive the greater deposit although it was further from the carbon than the German silver spring. Mr. C. V. Boys said the photographs reminded him of a phenomenon he observed some time ago on a glass sheet against which one terminal of a dry pile had been resting for some weeks. Just as on the carbon button, the glass near the point of contact was clean and had a comet-shaped deposit formed around it. He could offer no explanation of the appearance.—The geometrical construction of direct-reading scales for reflecting galvanometers, by Mr. A. P. Trotter. In a recent paper on galvanometers, by Prof. W. E. Ayrton, F.R.S., T. Mather, and Dr. W. E. Sumpner, read before the Society, the opinion was expressed that proportionality of scale reading to current was very desirable, and the present paper shows how to bend a scale of equal divisions so as to give the required proportionality. Suppose the currents required to produce several deflections have been experimentally determined. A full-size plan of the scale is then drawn, and radial lines from the points on the scale at which the observations were taken are drawn towards the centre of the mirror. Let these radii be numbered 0, 1, 2, 3, &c., commencing from zero azimuth. According to the procedure recommended, distances proportional to the several current strengths are marked off along the edge of a strip of paper, a few inches being left over at each end. Call the marks a, b, c, d, &c., a being the zero point. Two points on the radii 0, 1, and equidistant from the mirror are now found such that the distance between them is equal to that between a and b on the strip, and the points marked by fine needles stuck in the board. The zero end of the strip is now fixed so that the marks a and b lie against the needles, and the strip is swept round until the mark c coincides with the radius 2, where also a needle is placed. Repeating the process gives a series of points which on being joined form part of a polygon. A line can then be drawn between the inscribed and circumscribing curves which has the same length as the sum of the straight lines, and this is the curve to which the original scale may be bent so as to give proportional readings. Diagrams showing such curves, constructed from the calibrations of instruments given in the paper above referred to, accompany the paper. The author showed that a family of curves may be drawn, each of which satisfies the required condition. Of the two limiting curves, one is tangential to the usual scale line at zero azimuth, and the other passes through the vertical axis of the mirror. The flattest of the various curves is generally the most convenient. Mr. J. Swinburne asked whether good definition could be obtained when such curved scales not equidistant from the mirror were used, and also whether it was not easier to divide a flat scale unequally so that the readings are proportional to the current. Mr. Trotter, in reply, said Dr. Sumpner thought there would be no difficulty as regards definition with the flat curves shown. He (Mr. Trotter) also added that a curved scale might be advantageous in reading the deflections from one side of a table, as the more distant part of the scale could be more nearly perpendicular to the line of sight. For such an arrangement, however, a parallel beam of light would be required.—A parallel motion suitable for recording instruments, by Mr. A. P. Trotter. This is a modification of Watt's parallel motion, in which the two fixed centres are on the same side of the line described by the "parallel point." The arrangement consists of two vibrating arms, one of which is twice the length of the other, and whose outer ends are jointed respectively to the middle and end of a short lever; the free end of the latter describes an approximate straight line. The motion was arrived at by considering the curve traced out by a point on the radius of a circle, such that its distance from the circumference measured towards the centre is equal to the radial intercept between the circle and a tangent line. The equation to the curve is $r = 2 - \sec\theta$ (conchoid of Nicomedes) and the radius of the osculating circle at the point where the intercept is zero is given as half that of the initial circle. This osculatory circle, the author finds, practically coincides with the curve over a considerable angle (40°), and thus may replace this part of the curve; hence the motion. The author thinks the motion will be useful for recording barometers, ammeters, and voltmeters, as it is more compact than that of Watt, and needs no fixed point beyond the straight path.—Owing to the absence of Prof. S. P. Thompson, his paper on Bertrand's refractometer was not read.

Linnean Society, March 6.—Mr. Carruthers, F.R.S., President, in the chair.—Mr. Thomas Christy exhibited a dried specimen of *Picramnia antidesma*, the plant from the bark of which a medicine, known as *cascara amarya*, a useful alterative in diseases of the blood and skin, is believed to be prepared.—Mr. J. E. Harting exhibited a series of horns of the American Prongbuck (*Antilocapra americana*), to illustrate the mode in which the shedding and new growth of horn is effected in this animal.—A paper was read by Mr. D. Morris, on the production of seed in certain varieties of the sugar-cane (*Saccharum officinarum*). It was pointed out that, although well known as a cultivated plant, the sugar-cane had nowhere been found wild; nor had the seed (*caryopsis*) been figured or described; it being the generally received opinion that, having been propagated entirely by slips, or cuttings, it had lost the power of producing seed. Spikelets, however, received at Kew, had been carefully examined, and the seed found, which was now for the first time exhibited by Mr. Morris. He anticipated that, by cross-fertilization and selection of seedlings, the sugar-cane might be greatly improved, and much importance was attached to the subject, as it opened up a new field of investigation in regard to sugar-cane cultivation. Mr. J. G. Baker and Mr. Christy concurred.—A paper was then read by Mr. Spencer Moore, on the true nature of *callus*; Part 1, the vegetable-marrow and *Ballia callitricha*. It was shown that the *callus* of sieve-tubes of the vegetable-marrow gives marked proteid reactions; and since it is dissolved in a peptonizing fluid there can be no doubt of its being a true proteid, and not a kind of starchy mucilage, as is usually supposed. The "stoppers" of *Ballia* also yield proteid reactions, but inasmuch as they resist gastric digestion, the substance cannot be a true proteid, and may perhaps be allied to lardacein. Mr. Moore maintained the view of Russow, Strassburger, and others—that *callus* is deposited upon the sieve—to be correct in the case of the vegetable-marrow; since a peptonizing fluid clears the sieve-plates and leaves them in their pristine condition, which would not be the case if *callus* were formed by a swelling up of the sieves. A discussion followed, in which Dr. F. W. Oliver, Dr. D. H. Scott, Prof. Reynolds Green, and Mr. George Murray took part.

Zoological Society, March 4.—Prof. W. H. Flower, F.R.S., President, in the chair.—The Secretary read a report on the additions that had been made to the Society's Menagerie during the month of February 1890.—Mr. F. E. Beddard exhibited and made remarks on some living specimens of an Indian Earthworm (*Perichæta indica*), obtained from a greenhouse in Scotland.—Mr. A. Thomson exhibited a series of insects reared in the Insect House in the Society's Gardens during the past year, and read a report on the subject. Particular attention was called to specimens of a South African Mantis (*Harpax ocellata*) and of a Canadian Stick Insect (*Diaphemora femorata*).—Mr. Henry Seebohm read a paper on the classification of birds, being an attempt to diagnose the sub-classes, orders, sub-orders, and some of the families of existing birds. The characters upon which the diagnoses were based were almost entirely derived from points in the osteology, myology, and the pterylosis of the groups diagnosed.—A communication was read from Mr. T. D. A. Cockerell, describing some Galls from Colorado, of which specimens were transmitted for exhibition.

EDINBURGH.

Royal Society, February 28.—Sir Douglas Maclagan, Vice-President, in the chair.—Prof. Rutherford communicated a paper on the structure and contraction of striped muscular fibre of crab and lobster.—Prof. Haycraft read a paper on the histology, functions, and development of the carapace of the Chelonia, and also another paper on the rate at which muscles contract when the motor paths are stimulated by interrupted electrical currents.

March 3.—Sir W. Thomson, President, in the chair.—Prof. Tait communicated a note on ripples in a viscous liquid. He investigates in it the motion of a continuous set of ripples, and discusses the effects of gravity, surface-tension, surface-stiffness, and viscosity.—Dr. Thomas Muir communicated a paper by Mr. D. Maver, on a geometrical method based on the principle of translation.—Prof. J. Stuart Blackie read a paper on the phases of the living Greek language.

PARIS.

Academy of Sciences, March 10.—M. Hermite in the chair.—Note on the life and works of George Henry Halphen, by M. Emile Picard.—On the phenomena seen about the sun on March 3, 1890, by M. A. Cornu. Halos and parhelia were seen about the sun on this date, and observations of the aqueous bands of the solar spectrum made at the time when the first halo of 22° appeared, showed that warm and moist currents existed in the higher regions of the atmosphere in spite of the exceptional cold (−11° C.) at Paris.—Thermal researches on the allotropic modifications of arsenic, by MM. Berthelot and Engel. The amount of heat evolved on treatment with bromine and water was found to be nearly the same in both the forms; arsenic, in this respect, behaving like carbon.—Second note on the absorption of atmospheric ammonia by soils, by M. H. Schlœsing. From the experiments described in this and the previous note, the author finds that calcareous, acid or neutral, dry or wet soils, absorb atmospheric ammonia. Moist earth, however, favours the fixation of ammonia, and dry earth retards it.—The muscular and elastic elements of the retrolingual membrane of the frog, by M. L. Ranvier. The problems investigated are: the attachment of the elastic fibres to the muscular bundles, and whether a fibril terminates in a thick or thin disc or a clear space, all of which occur in the muscular bundles.—On the microbes of acute osteomyelites called infectious, by MM. Lannelongue and Achard.—Study of the errors of observation, by M. J. E. Estienne.—Sun-spot in very high latitude, by M. Dierckx. To this note we refer elsewhere (p. 472).—On Stirling's formula, by M. E. Rouché.—On regular surfaces which pass through a given curve, by M. Ch. Bioche.—On the compounds of phosphoretted hydrogen and ammonia with boron chloride and silicon hexachloride, by M. A. Besson.—Note on the compounds of the metals of the alkalies with ammonia, by M. J. Moutier.—On the estimation of free halogens and of iodides in presence of chlorine and bromine, by M. P. Lebeau. Iodine is estimated by liberation from its compound in aqueous solution by a standard solution of bromine, the iodine being dissolved out from the water by CS_2 as soon as liberated: the end of the reaction is indicated by the decoloration of the supernatant aqueous solution, to which a few drops of indigo solution has been previously added.—On the formation of thiosulphate of lead, note by M. J. Fogh.—Decomposition of thiosulphate of lead by heat. Trithionate of lead, by the same author. It is shown that, by the prolonged action of boiling water, thiosulphate of lead decomposes according to the equation $2PbS_2O_3 = PbS + PbS_3O_6$.—On a new iodide of bismuth and potassium, M. L. Astre.—Note on the molecular increase of dispersion of saline solutions, by MM. Ph. Barbier and L. Roux. If the constant K given in a previous communication be multiplied by the molecular weight of the dissolved salt, what the authors term the molecular increase of dispersion is obtained. MK for chlorides of the type MCl is shown to have the mean value 0·020, for chlorides MCl_2 the mean value is 0·044.—Researches upon the application of measurements of the rotatory power to the determination of compounds resulting from the action of malic acid upon the neutral molybdates of lithium and magnesium, by M. D. Gernez.—The volumetric estimation of tannin, by M. E. Guenez.—Estimation of acetone in methyl alcohol and in the raw methyl alcohol used for methylation, by M. Léo Vignon.—On the diminution of fermenting power of the ellipsoidal wine-yeast, in presence of salts of copper, by M. A. Rommier.—On a Coleopterous insect attacking the vine in Tunis (*Ligniperda francisca*, Fabricius), by M. A. Laboulbène.—The preparation of crystallized basic nitrate of copper and its identification with gerhardtite, by M. L. Bourgeois.

BERLIN.

Meteorological Society, February 11.—Prof. Schwalbe, President, in the chair.—Dr. Danckelmann spoke on the meteorological conditions which exist on the Gold and Slave Coast. General observations had been started in New Guinea, but were soon reduced to observations of rainfall only; during the years 1886 to 1889, they had yielded some interesting results on the connection between rainfall and the direction of the monsoons and trade-winds. No trustworthy data are as yet to hand of the meteorological conditions of Southern Africa, Cameroon, and East Africa, but, on the other hand, there is a mass of material accumulated at many stations on the Guinea coast. From the latter it appears that the atmospheric pressure varies but slightly, and shows a maximum in July and August. In Bismarckburg the wind blows from the north and north-east from the Sahara in December, January, and February; in June, July, and August it blows west and south-west. Variations of temperature are but slight, presenting a maximum in December to February, and a minimum in July and August. The amount of rainfall is very variable, being, in some places, as low as 575 mm. per annum; in others, 1000, 1500, or even 3500. The speaker concluded by describing the climatic conditions of this region, pointing out that they may be explained with reference to the contiguity of the Sahara Desert.—Dr. Eschenhagen gave a detailed description of the Magnetic Observatory at Potsdam, dealing with its structural arrangements and the internal location of the instruments. While exhibiting the photographically recorded curves of the previous fortnight, he dealt with the breaks in these which result from any more than usually severe shock of earthquake. These he attributed to purely mechanical causes rather than to magnetic, basing his views on observations of the movement of the surface of mercury at the time. He pointed out that the opposite view, urged by French meteorologists, as based upon observation of a copper rod with a bifilar suspension, is inconclusively supported by such observations, inasmuch as the equilibrum of a copper rod is relatively stable, while that of a bifilar magnet is unstable.—The President referred, in conclusion, to the loss which meteorology had sustained in the death of Buys Ballot.

Physiological Society, February 14.—Prof. du Bois Reymond, President, in the chair.—Prof. Zuntz gave an account of experiments conducted in his laboratory by Dr. Katzenstein, on the influence of bodily labour on the metabolism of man. After giving an historical *résumé* of previous researches, he described the methods employed in the present research. The experiments were conducted in a very convenient form of respiration-apparatus, the analysis of the gases being made by Hempel's method. Great stress was laid on the accurate determination of the work done; the latter consisted in either turning a wheel against a graduated resistance, or else in motion on either a plane or inclined surface. In the latter form of work an apparatus was used which had previously been employed in experiments on a horse. The oxygen consumed in each experiment was taken as a measure of the metabolism. It was found that this was permissible, from the fact that the respiratory quotient was observed to be constant during the three conditions of rest, walking, and climbing. From this it appeared that the energy required for any given work was the outcome of the union of oxygen and carbon in the formation of carbonic acid gas. The increased respiratory interchange which accompanied any extra work fell to the normal some two or three minutes after the work ceased. In each experiment the distance covered and height through which the body was raised was measured in kilogram-metres; the oxygen simultaneously absorbed was determined, and from this the amount of oxygen which would have been absorbed if no work had been done was subtracted, so that the amount of oxygen required for the given work was obtained. It was found that, as in Smith's experiments, the metabolism might be increased to two or three times the normal during work. The experiment was then repeated, employing a different rate of motion and steepness of ascent, so that it was readily possible to calculate the oxygen, in cubic centimetres, required for a progression of one metre or the raising of one kilogram; the former was then reduced to a unit of one kilogram of body-weight. The result obtained from the person on whom most of the experiments were made was that the moving of one kilogram of body-weight over one metre of space on the level involved a consumption of 1·11 c.c. of oxygen, and for the raising of one kilogram through one metre, a consumption of 1·438 c.c. In conclusion, the speaker drew some interesting comparisons between the results of these experiments and those previously made on a horse.—Dr. Benda exhibited several preparations of sense-organs of mammals; and Dr. Katz showed some specimens of the organ

of Corti.—Dr. Hausemann spoke on unsymmetrical karyokinesis met with in epitheliomata. Ordinarily the chromatin-filaments divide into two equal parts, but in cancer-cells they do not, and from this results the polymorphism of the nuclei.

Physical Society, Febuary 21.—Prof. du Bois-Reymond, President, in the chair.—Prof. von Bezold made a short speech in memory of Buys Ballot, pointing out with chief prominence that he was the first to draw attention to the necessity of co-operation between the meteorologists of different nations, and that he had been chiefly instrumental in establishing the existing International Meteorological Congress. He further showed that Buys Ballot was the first to give a survey of the meteorological conditions existing simultaneously at different places on the earth's surface, the pioneer in the production of the synoptic charts which are now published (see *Poggendorff's Annalen* for 1847), and the first to thoroughly grasp and state with precision the difference between weather and climate.—Dr. E. Pringsheim spoke on Kirchoff's law and gaseous radiation. During the experimental verification of the above, the speaker was chiefly interested in the behaviour of gases and vapours, and selected for his experiments sodium vapour. It was impossible to obtain any answer to the question "Does a gas acquire the power of emitting light-rays when its temperature is raised?" by the mere introduction of sodium or its salts into the non-luminous flame of a Bunsen burner, since it was not possible to exclude the occurrence of chemical changes during such an experiment. Thus he employed rather the method of Lockyer, Liveing, and Dewar, heating the metal in a sealed tube. In this way he verified the appearance of the bright emission-line and of the absorption-line of sodium. The lowest temperature at which they make their appearance was determined and measured thermo-electrically, but the speaker did not deduce any absolute value from his data. He further considered that the radiation of gases when heated is not yet definitely proved, since the nitrogen in which he heated the sodium contained minute traces of oxygen, and the method he employed for closing the ends of his tube permitted of the probable entry of small quantities of air. He had, therefore, additionally made experiments with thallium, and on the introduction of air into the metallic vapours; these experiments yielded a distinctly affirmative answer to the original question, but require further extension. So also do some experiments on the occurrence of a fluted spectrum of sodium, which the speaker had made during the course of the above work.

DIARY OF SOCIETIES.

LONDON.

THURSDAY, MARCH 20.

ROYAL SOCIETY, at 4.30.—The Bakerian Lecture—On the Discharge of Electricity through Gases: Prof. A. Schuster, F.R.S.
LINNEAN SOCIETY, at 8.—The External Morphology of the Lepidopterous Pupæ; Part 2, the Antennæ and Wings: E. B. Poulton, F.R.S.—On the Intestinal Canal of the Ichthyopside with especial Reference to its Arterial Supply: Prof. G. B. Howes.
CHEMICAL SOCIETY, at 8.—The Evidence afforded by Petrographical Research of the Occurrence of Chemical Change under Great Pressures: Prof. Judd, F.R.S.
ZOOLOGICAL SOCIETY, at 4.
INSTITUTION OF ELECTRICAL ENGINEERS, at 8.
ROYAL INSTITUTION, at 3.—The Early Developments of the Forms of Instrumental Music (with Musical Illustrations): Frederick Niecks.

FRIDAY, MARCH 21.

PHYSICAL SOCIETY, at 5.—On the Villari Critical Point of Nickel: Herbert Tomlinson.—On Bertrand's Idiocyclophanous Prism: Prof. Silvanus Thompson.
INSTITUTION OF CIVIL ENGINEERS, at 7.30.—Economy Trials of a Compound Mill-Engine and Lancashire Boilers: L. A. Legros.
ROYAL INSTITUTION, at 9.—Electro-magnetic Radiation: Prof. G. F. Fitzgerald, F.R.S.

SATURDAY, MARCH 22.

SOCIETY OF ARTS, at 3.—The Atmosphere: Prof. Vivian Lewes.
ROYAL BOTANIC SOCIETY, at 3.45.
ROYAL INSTITUTION, at 3.—Electricity and Magnetism: Right Hon. Lord Rayleigh, F.R.S.

MONDAY, MARCH 24.

ROYAL GEOGRAPHICAL SOCIETY, at 8.30.—North American Trans-Continental Pathways, Old and New: Augustus Allen Hayes.
SOCIETY OF ARTS, at 8.—Some Considerations concerning Colour and Colouring: Prof. A. H. Church, F.R.S.

TUESDAY, MARCH 25.

ANTHROPOLOGICAL INSTITUTE, at 8.30.—Exhibition of a Skull, dredged up on the Manchester Ship Canal Works: Isidore Spielman.—The Old British "Pibcorn," or "Hornpipe," and its Affinities: Henry Balfour.—The Ancient Peoples of Ireland and Scotland considered: Hector Maclean.
SOCIETY OF ARTS, at 8.—Engraving in Wood, Old and New: W. J. Linton.
INSTITUTION OF CIVIL ENGINEERS, at 8.—Lough Erne Drainage: James Price, Jun. (Discussion.)—Barry Dock and Railway: John Robinson.
ROYAL INSTITUTION, at 3.—The Post-Darwinian Period: Prof. G. J. Romanes, F.R.S.

WEDNESDAY, MARCH 26.

GEOLOGICAL SOCIETY, at 8.—On a New Species of Cyphaspis from the Carboniferous Rocks of Yorkshire: Miss Coignou. Communicated by Prof. T. McKenny Hughes, F.R.S.—On Composite Spherulites in Obsidian from Hot Springs near Little Lake, California: F. Rutley.—A Monograph of the Bryozoa (Polyzoa) of the Hunstanton Red Chalk; G. R. Vine. Communicated by Prof. P. Martin Duncan, F.R.S.—Evidence furnished by Quaternary Glacial-Epoch Morainic Deposits of Pennsylvania, U.S.A., for a Similar Mode of Formation of the Permian Breccias of Leicestershire and South Derbyshire: W. S. Gresley.
SOCIETY OF ARTS, at 8.—Carriage-Building and Street Traffic in England and France: G. N. Hooper.

THURSDAY, MARCH 27.

ROYAL SOCIETY, at 4.30.—The following papers will *probably* be read:—On Black Soap-films: Profs. Reinold and Rücker, F.R.S.—The Variability of the Temperature of the British Isles, 1869–83 inclusive: R. H. Scott, F.R.S.—Preliminary Note on Supplementary Magnetic Surveys of Special Districts in the British Isles: Profs. Rücker and Thorpe, F.R.S.—The Rupture of Steel by Longitudinal Stress: C. A. Carus-Wilson.—Measurements of the Amount of Oil necessary in order to check the Motion of Camphor upon Water: Lord Rayleigh, Sec.R.S.—On the Stability of a Rotating Spheroid of Perfect Liquid: G. H. Bryan.—A Determination of ν, the Ratio of the Electromagnetic Unit of Electricity to the Electrostatic Unit: Prof. J. J. Thomson, F.R.S., and G. F. C. Searle.
CHEMICAL SOCIETY, at 4.—Anniversary Meeting.—Election of Office-Bearers and Council.
INSTITUTION OF ELECTRICAL ENGINEERS, at 8.
ROYAL INSTITUTION, at 3.—The Early Development of the Forms of Instrumental Music (with Musical Illustrations): Frederick Niecks.

FRIDAY, MARCH 28.

INSTITUTION OF CIVIL ENGINEERS, at 7.30.—Deflection of Spiral Springs: Alfred E. Young.
ROYAL INSTITUTION, at 9.—Foam: Right Hon. Lord Rayleigh, F.R.S.

SATURDAY, MARCH 29.

SOCIETY OF ARTS, at 3.—The Atmosphere: Prof. Vivian Lewes.
ROYAL INSTITUTION, at 3.—Electricity and Magnetism: Right Hon. Lord Rayleigh, F.R.S.

CONTENTS.

THURSDAY, MARCH 27, 1890.

A SOUTH LONDON POLYTECHNIC.[1]

SOME little time ago we expressed our views on the general scheme put forward by the Charity Commissioners for the establishment of Polytechnics (we must use the word, however inapplicable) in various parts of London. Since then we have received a copy of the architect's report on the requirements of a Technical Institute for Battersea. It may be well to recall to the minds of readers the main features of the proposed scheme. The Polytechnic in Regent Street, and the People's Palace at Mile End, are to receive large endowments to enable them to continue and develop the work on which they are already engaged, a large sum is to be given to found a City Polytechnic, and series of three new Institutes are to be established in various parts of South London; whilst others, at present more or less shadowy and prospective, are talked of for other parts of the metropolis.

Of the three new Institutes, the plans for which may be said to be in an advanced condition, two will be housed in buildings already established. The Goldsmiths' Company have bought the Royal Naval School at New Cross, and are adapting and altering it so as to be ready to be opened for its new purpose in October next. The premises of the Borough Road Training College have been secured for the second of the Institutes, which is probably to be partly endowed by the Ironmongers' Company. The scheme in this case is not, we believe, yet published, and some delay may take place; but, if all goes smoothly, this Institute also may be ready to begin work before very long.

The third of the proposed South London Polytechnics is the Battersea Institute, for which we have received the draft plans. Here there is no existing building to be adapted. Everything must start *de novo*, and only the limits of the funds at their command, and their uncertainty as to the future tastes and wants of the district, need restrict the trustees in their efforts to make the Institute in every way worthy of its purpose.

And here we may at the outset congratulate the trustees on the mode in which they have determined to proceed. They have intrusted to Mr. Rowland Plumbe the task of visiting other technical schools, obtaining necessary information, and preparing a detailed statement of the requirements of the Battersea Institute, and have since circulated his draft Report among various experts, with requests for criticisms and suggestions. The plans with which the Report is illustrated are not intended to be in any way final, but merely to suggest the nature of the requirements of the Institute to the architect, whoever he may be, who is ultimately selected to design the building. It is clear that no stone will be left unturned, so far as the Committee are concerned, to make the Battersea Institute a model Polytechnic.

We may congratulate the Committee on another matter. In our former article we pointed out the inexpediency of attempting too much at once, while the whole question of the future of Polytechnics is in an experimental stage. Since then, Sir Bernhard Samuelson and other members of the Executive Committee of the Technical Association have publicly impressed similar views upon the Vice-President of the Council, into whose hands the Commissioners' scheme has now passed. We are, therefore, glad to see that Mr. Plumbe expressly states that his plans are drawn up so that the proposed building may be gradually constructed as the need arises; and though he does not conceal his own desire to have the whole building erected at once, we are glad to learn that the Committee have decided to let the institution grow as the number of students increases, and not to erect a great shell until they see more clearly the extent of the demand which it is to supply. We gather further that the sum required for the endowment of the Institute is not yet complete, and we may take it for granted that no attempt will be made to start operations until this necessary preliminary step is completed. Thus those who are anxious that the whole scheme for Polytechnics should not be imperilled by hastily founding too many at once before one new Institute has been made a success, may feel assured that the necessary interval which must elapse before the foundation-stone of the Battersea Institute can be laid will give some further opportunity to the promoters to profit by the experience which accumulates every day of the working of similar institutions elsewhere.

To quote Mr. Plumbe's Report, "The combined form of Institute . . . is a growth almost of the present day, and the subject as now presented is, with the hereinafter mentioned exception, comparatively new and *without precedent*." The exception referred to is Mr. Hogg's Polytechnic, and as this is the product of the gradual growth of seventeen years, the argument for going "slow and sure" is irresistible. The promoters of the Goldsmiths' Institute at New Cross are, we understand, equally alive to this necessity.

Mr. Plumbe has made inquiries, for the purpose of his Report, into the nature of the industries of Battersea, and has visited several of the chief Technical Institutes in London, from the Bow and Bromley Institute up to the Central Institution of the City and Guilds Institute. He might, perhaps, with advantage have extended his visit to some of the more important provincial centres, which in some ways offer examples which are not to be found in the metropolis of the kind of equipment required for a popular technical school. London has long been behindhand in the matter, except for the higher Colleges at South Kensington, which are intended to serve a purpose so different that their example may be disregarded. There are, indeed, the two existing Polytechnic Institutes, and apparently Mr. Plumbe has derived from them almost all his information as to the requirements of the Battersea Institute. The Regent Street Polytechnic he considers "most undoubtedly must serve as a model to all succeeding institutions." He presumes that the Committee will "follow to some extent the curriculum of study adopted at Mr. Quintin Hogg's Polytechnic and the People's Palace."

Without in any way challenging these conclusions, it is only fair to point out that the first-hand inquiries on which they are based are mostly derived from these very

[1] "South London Polytechnic Institutes—Report on Requirements for the Battersea Institute." By Rowland Plumbe, F.R.I.B.A.

institutions. Now it is important that in a new departure like that which it is proposed to make at Battersea we should not blindly follow in the rut of any one existing institution, and the only way to avoid this is to profit by the experience of other technical institutes in various parts of the country. Mr. Plumbe quotes the Report (now nearly six years old) of the Royal Commission on Technical Instruction, but many of the more important provincial schools have sprung up since that date, and the Commission on Elementary Education to which he refers only dealt with elementary schools. He is consequently led to the very doubtful conclusion that provincial schools offer no example for London because of the "thoroughness and great cost of the education given (which further required the whole time of the pupils for a number of years)." "I have not," he continues, "thought it necessary to spend any further time on the examination of buildings of this character, particularly as I found those of most experience with whom I conferred on the subject were distinctly of my opinion."

Who these experts were we are not told, but the above remarks are scarcely applicable to such technical schools as those at Bradford, Huddersfield, Keighley, Manchester, Bristol, and other large centres, which are doing for the artisan population of those districts much the same service as is expected from the Battersea Institute.

Whether instruction be elementary or advanced, whether it be intended for masters or for workmen, it ought to be "thorough," and thoroughness implies to some extent costliness. "To educate the industrial classes on a large scale at a comparatively nominal cost" is an attempt which looks better on paper than in practice.

And this brings us to the question of the financial aspect of the scheme. Mr. Plumbe states that his estimate of the cost of a given amount of accommodation is based on a memorandum by Mr. H. Cunynghame, in which he calculates that the building, including land, &c., ought to be erected and fitted up for £11 per student or member and that the cost of annual maintenance, in addition to fees and grants, will amount to 15*s*. per head per annum. This estimate is naturally considered by Mr. Plumbe to be "moderate in the extreme." It is much to be desired that the basis of Mr. Cunynghame's calculation should be made public, so that the materials should exist for the formation of a sound judgment thereon.

As regards the cost of building, all depends of course on the kind of building proposed; but it would be melancholy, indeed, if an institution directly designed to elevate the ideas and refine the taste of the population of dismal and ugly South London, were to be housed in a building "of the plainest and most utilitarian character"—to say nothing of the quality of materials used in its construction.

But from an educational point of view an even more important consideration is the necessary amount of endowment. The allowance of 15*s*. a head, "including repairs and maintenance," seems very meagre, if fees are to be low, and at the same time first-class teaching power and management are to be secured, *and paid for*. To base an estimate on the current expenses of the Regent Street Polytechnic is to run the risk of serious error, for it is well-known that much of the work of organization and direction has there been performed gratis, or at far below market value, thanks to the enthusiasm of a few devoted workers. Can the Committees of the new Institutes call into existence a similar amount of enthusiasm among men of leisure and means in connection with each of the proposed Institutes (not, be it remembered, of a religious character), which will justify them in relying on being permanently saved the bulk of the expenses of management? If not, it is clear that a good deal will have to be added to the estimate of 15*s*. a head.

Another matter which is of importance from a financial point of view is the question of the position to be occupied by the day-school with respect to other sections of the new Institute. On this point, the language of the Commissioners' scheme is vague almost to the point of unintelligibility. There are evident advantages in utilizing the Polytechnic buildings in the day-time for the purpose of a school which may afterwards serve as a feeder to the evening classes. But it should be an organic part of the Institute; not a mere appendage, the existence of which may be tolerated so long as it interferes with no other department of work and claims no share in the endowment. Yet such seems to be the present intention of the Charity Commission, so far as we can gather from their published statements. The language of Mr. Plumbe's Report confirms this conclusion, against which it is time to record an emphatic protest. In our opinion, the day-school, if properly conducted, should ultimately become the corner-stone of the whole educational work of the Institute, for much more systematic teaching can be done in the case of boys working all their time than can be hoped for with students devoting a couple of evenings a week to instruction and recreation. Doubtless, in Regent Street a secondary school can be made self-supporting, and even profitable, by its fees; but such an attempt would be undesirable, and indeed impossible, in the case of a school for the "poorer classes" in a poor district. A high-fee'd school might perhaps fill itself at the expense of emptying other schools in the neighbourhood, but it would not fill the gap which wants filling. Under these circumstances, to condemn the day-school to pay its way is to condemn it to become a mere grant-earning machine, neglecting all subjects which do not pay, and constructing its curriculum strictly on the lines of the South Kensington Directory. What is wanted is a good modern school with a low fee, and a large number of scholarships for competition among the scholars of elementary schools. But such a school cannot be made self-supporting, and the Battersea Committee would do well to induce the Charity Commissioners, before it is too late, to recognize this fact frankly in the scheme which they are about to draw.

Again, we should be glad to know how wide a margin Mr. Cunynghame's estimate allows for the cost of what we may term "local adaptation." For example, in Mr. Plumbe's list of local industries we find chemical works, match factories, and gas-works. From this it would seem that there is room for the teaching of chemistry in its application to various industries. But such instruction, though it is one of the chief objects with which the technical side of the Institute is started, must involve extra cost, for it will not produce grant; and Mr. Plumbe's conclusion from his inquiry, that the "science and art

classes should be carried on so that the Government grant be earned," is a *non sequitur;* at all events until the Science and Art Department award grants for distinctively technical subjects under the new Technical Instruction Act.

We cannot help thinking that if due weight is allowed to these considerations the estimate of 15*s.* a head will be largely raised (unless compensation be sought by cutting down some of the more expensive trade classes); and as we suppose the endowment cannot be much increased, the number of students to be provided for must be necessarily diminished. In fact, the whole scale on which Mr. Plumbe has calculated the requirements of the Institute may have to be somewhat revised. To those who consider large numbers all-important, this may seem deplorable, but we are convinced that the Committee of the South London Polytechnic will prefer the interests of efficiency to those of temporary display.

One other matter which we notice with some surprise and regret is the apparent omission in the plans to provide committee-rooms and other accommodation which can be utilized by local working men's organizations. We referred in our former article to the importance of making the Institutes real working-class centres, and the reply of the Charity Commissioners to the deputation from the London Trades Council on the subject was supposed to be favourable to the provision in connection with each Institute of rooms which could be utilized on moderate payment by various working-class societies which now too often have to meet in public-houses. The omission of any such provision in the plans for Battersea is a serious blemish on the scheme, which, however, can easily be corrected, as soon as pointed out.

The Committee will have a great opportunity, which it is to be hoped they will use aright, of providing the inhabitants of South London with a technical and recreative Institute, which in its close adaptation to local needs may serve as model for all such Institutes in the future.

A GEOLOGICAL MAP OF THE ALPINE CHAIN.

Geologische Übersichtskarte der Alpen. Entworfen von Dr. Franz Noë. Mit einem Begleitworte. (Wien: Ed. Hölzel, 1890.)

GOOD, and in some cases even elaborate, geological maps exist for parts of the Alps; but one to exhibit the chain as a whole, without being on a scale so large as to be unwieldy or so small as to be indistinct, has been hitherto a desideratum. This has now been supplied by Dr. Noë. The scale adopted is 1 in 1,000,000, or about 16 miles to the inch, which very well satisfies both the above conditions. A glance at the list of authorities which have been consulted indicates that Dr. Noë has had no easy task; for in Alpine geology there are indeed consellors enough, but their multitude is not strength, for they are so often at variance.

At the present stage of knowledge, the chartographer must be content, in dealing with the crystalline schists (using that term in a rather wide sense), to colour his map petrographically—that is to say, he must, as far as possible, record facts and avoid theories. Dr. Noë has endeavoured, though not with complete success, to render his maps petrographical in the parts where doubt might arise, viz. those occupied by that crystalline series which, whatever may be its age, in the Alps always underlies any sedimentary rock to which a date can be assigned. The principle of coloration agrees very nearly with that suggested by the International Geological Congress at Bologna. Crimson denotes the deep-seated igneous rocks of the more acid type, dull green the more basic; two slightly different shades of red represent respectively the older (and in most cases more acid) volcanics and the newer volcanics. Four colours are employed to express the "crystalline schist" series: one, for the Central gneiss and some of the oldest mica-schists; another, for the less coarsely crystalline (and probably newer) mica-schists, together with calc-schists, chlorite-schists, &c.; a third, for certain crystalline schists, phyllites, and clay-slates of uncertain geological age; and marbles are indicated by a deep blue. Palæozoic rocks (exclusive of Permian) are coloured purple, the different series being distinguished by symbols; pale brown denotes Permian; tints of blue represent the Triassic and Jurassic strata; green signifies Neocomian and Cretaceous; orange the older Tertiary, flysch having a separate tint; one shade of yellow is used for Miocene and Pliocene; another for Diluvial and Alluvial deposits—the former a word of misleading origin, which ought to have long since disappeared from geological nomenclature.

Very wisely, Dr. Noë has included in his map something more than the Alps. Not only do we find the Jura, but also this region is extended far enough in the direction of Dole to exhibit the remarkable exposure of the old crystalline floor, north of that town. On the right bank of the Rhine, in the neighbourhood of Sackingen, a considerable strip of crystalline rock is shown, the end of the great Schwarzwald *massif;* and north of the Eastern Alps we find the crystalline rocks indicated as they uprise from beneath the Miocene on the left bank of the Danube, as, for example, near Linz, and again at Pressburg. The geological colours also are carried down the east coast of the Adriatic as far as Spalato, so that the connection of the Istrian and Dalmatian Alps with the main chain is made perfectly clear. Unfortunately, however, Dr. Noë has not applied the same treatment to the Apennines, though their connection with the Alpine chain cannot be of less geological importance, for he brings the colours to an abrupt end a few miles west of Savona.

In one or two respects the above system of coloration seems open to criticism. The tint and the lines used to indicate mountain land are productive of some confusion, and increase the difficulty of identifying the colours, without, as we think, producing a compensating advantage. The use of three colours for the Trias-Rhætic seems a disproportionate subdivision when only one is allotted to Neocomian-Cretaceous. We are, however, disposed to differ more seriously—though only occasionally—from Dr. Noë as to his use of the colours for the divisions of the crystalline schists. One of these is made too inclusive, because it is applied to clay-slates and phyllites as well as to rocks which must be admitted to be crystalline schists. Granted that there is sometimes a difficulty in separating these in the field, we fail to see the propriety of deliberately effacing the distinction. Fortunately, however, this confusion, owing to the scale of the map, does not

seriously mislead the student, but we are more perplexed to discover the reasons which have led in some cases to the separation of the crystalline members of this group from certain of those in the other, and presumably older group, which is defined as consisting of "mica-schists calc-mica-schists, chlorite-schist, &c. To the latter are referred the schists—calcareous, micaceous, and chloritic—near Windisch-Matrei; to the former the great belts north and south of the Tauern range, which, for instance, occur respectively near Mittersill and Lienz. We cannot understand on what grounds these are distinguished. Further, the great group of schists which sweeps along on the eastern flank of the watershed of the Franco-Italian Alps, as, for example, near the Mont Genèvre, has the same colour as those of Windisch-Matrei; but petrographically they appear to us inseparable from the other group. By some geologists, as is well known, the "lustrous schists" have even been mapped (erroneously no doubt) as altered Trias.

Still, though we venture to dissent occasionally from Dr. Noë, and think that in all probability a wider personal knowledge of the Alps would have led him occasionally to modify a conclusion and to avoid some slight inconsistencies, we cannot conclude this notice without expressing our sense of the very great value of his work. He has placed a really good general map of the Alps within the reach of all students, for the price at which it is sold is surprisingly low. The map is accompanied by a useful descriptive pamphlet, to which Prof. Suess has written a short preface.

T. G. BONNEY.

OLD AGE.

Old Age. By George Murray Humphry, M.D., F.R.S, (Cambridge: Macmillan and Bowes, 1889.)

IN spite of pessimistic philosophies, man still regards life as worth living, and trusts to attain to a good old age, however miserable his life may seem to impartial critics. This desire, of course, is a necessary condition of human existence, and the destruction of it would entail the extinction of the human race—a contingency, however, which is never likely to arise. Hence, we have no doubt that this volume will be eagerly scanned by innocent persons who are still in hopes of finding some panacea which will enable them to attain the desired length of days.

But, alas, the number of their somatic cell generations is already fore-ordained in the germ from which they were developed; and no rule of life can increase this. No man by taking much thought can add a cubit to his stature, nor a decade to the predestined span of his existence. Yet the facts gathered together in this book may afford some hints as to the best way of attaining just this limit.

On p. 135, *et seq.*, Prof. Humphry reviews the chief characteristics in the mode of life of the favoured subjects of the work. He begins by saying that the results of the collective investigation respecting old age, "have not been such as to evolve anything very novel or startling, or to give rise to any fresh theories with regard to longevity and the means of attaining it," but only to "show that the maxims and laws which common-sense would dictate hold good, that the real *elixir vitæ* is to be found in the observance of them, and that, as a general rule, those persons live the longest who might be expected to do so."

The author also emphasizes the fact of the all-importance of inherited predisposition among the factors that tend towards producing longevity, and shows that nearly all the subjects of the returns came of a long-lived stock. In most of them, too, the body was well-proportioned and developed, brain development fair, and there was a remarkable absence of degenerative changes in the arteries and cartilages. According to the author, their essential characteristic is that all parts of the body are so well balanced, that the senile decay of function goes on in them all simultaneously, and at an equal rate, so that, *e.g.*, the vascular system is not overloaded and overworked by a too vigorous digestive apparatus, nor the vessels worn out by an over-excitable nervous and cardiac mechanism, so that if we could induce all our organs

> "to arrange
> This not to be avoided change,
> So as to change together,"

we should have gone far towards attaining the secret of long life.

Most of the persons described were temperate, taking little alcohol and meat, and lived active open-air lives. There are one or two startling exceptions to the former rule, however; such as the centenarian who "drank like a fish all his life," and several others who had always indulged pretty freely in stimulants.

Another point that Prof. Humphry lays stress on is the fact that most of these people were early risers, and could do with little sleep. It seems that the anabolic processes are more complete and regular when they are accomplished quickly. *Apropos* of this, he quotes with approval the dictum of the Duke of Wellington: "When one turns in bed, it is time to turn out."

In discussing the general aspects of his subject, he shows that old age may be said to be a product of civilization, the law of the "weakest to the wall" being altered by the growth of sympathy, and of love for others. But the continued existence of old people among communities may (partly, at all events) be accounted for on more utilitarian principles. Weismann remarks:—

> "It [old age] is obviously of use to man, for it enables the old to care for their children, and is also advantageous in enabling the older individuals to participate in human affairs, and to exercise an influence upon the advancement of intellectual powers, and thus to influence indirectly the maintenance of the race."

Thus we see the production of old age could be accounted for simply on the laws of natural selection among nations.

The fertility of these long-lived individuals is also above the normal (the average of children born to each, whether man or woman, being six), and many of them seem to have borne or begotten children to an advanced age. This, again, is in accordance with the view advocated by the biologist just quoted—viz. that a lengthening of life is connected with the increase in the duration of

reproduction. The effects of this fertility of long-lived people must give their stock an advantage in the race for existence, so that one would expect their number, in proportion to the rest of the population, gradually to increase.

The last chapter gives a short account of the maladies of old people, and is chiefly of medical interest.

Besides the general account of the subject, Prof. Humphry gives all the analyses of the British Medical Association returns, which furnish the material for the book. There are several good photographic illustrations: the frontispiece, portraits of a man and his wife (both over 101 years), and others, representing sections through the neck of the thigh-bone, and the jaw of old people. With regard to the femur, Prof. Humphry points out that there is no foundation for the generally accepted idea that the head in old people sinks to or below the level of the great trochanter, and the illustration certainly bears out his criticism.

Perhaps the happiest feature of the book is its optimism. "It is satisfactory to note how many of the very aged are in good possession of their mental faculties —taking a keen interest in passing events, forming a clear judgment upon passing events, and full of thoughts for the present and future welfare of others."

An old age like this is worth striving to attain, although one may never be free from the dread of dying "from the head downwards," and so lingering on in

> "Second childishness and mere oblivion,
> Sans teeth, sans eyes, sans taste, sans everything."

E. H. S.

THE ELEMENTS OF ASTRONOMY.

The Elements of Astronomy. By Prof. C. A. Young, Ph.D., LL.D. (Boston and London: Ginn and Co. 1890.)

THIS is a valuable addition to the existing text-books of astronomy for the use of those who intend to study the subject seriously. It has much in common with the same author's larger work on "General Astronomy" (see NATURE, vol. xxxix. p. 386), but we are assured that it is not merely an abridgment, but has been worked over with special reference to a high-school course. It is assumed that the students have mastered the ordinary elementary subjects, and are acquainted with elementary algebra and geometry.

The book covers quite as much ground as can be expected for an elementary course, although many of the subjects are merely glanced at. Practically everything, with the exception of the more difficult problems of mathematical astronomy, is considered more or less. The opening chapters deal with definitions, the geometry of the sphere, and the determination of latitude and longitude. Chapters on the earth's dimensions and motions, the moon, sun, planets, comets, stars, and nebulæ, then follow. An appendix includes topics which might be considered beyond an elementary book, but are still of sufficient importance to form part of a high-school course.

Astronomical physics receives a fair share of attention, but here the book is necessarily more open to criticism than in the parts dealing with well-established facts and principles. There are few general text-books which treat this important branch of astronomy in a satisfactory manner, and it is perhaps not to be wondered at, as the constantly increasing number of new observations necessitate considerable changes in our ideas. As far as a consideration of the facts is concerned, however, Prof. Young has done his work admirably, but this cannot be said of his treatment of the various conclusions which have been drawn from them. In his introduction, Prof. Young tells us that he has tried to treat every subject in such a way as "to discourage narrow and one-sided ways of looking at things, and to awaken a desire for further acquisition." However he may succeed with his readers, it does not seem that he has altogether taken this lesson to heart himself, for we find him dismissing suggestions without a complete hearing. For instance, in connection with the theory that sun-spots are formed by the down-rush of cool materials into the photosphere (p. 130), he states that it is not easy to reconcile this view with the distribution of the spots over the sun's surface. Further enquiry on his part, however, would have shown him that the theory in its extended form suggests that the spot-forming material is mainly formed of vapours which have condensed in the cool outer layers of the sun's atmosphere (in the same way as water-vapour condenses in our own), and also gives an explanation of the way in which the material may be localized over the spot-zones. The author is notably cautious with regard to new things, but we are surprised to find that he continues to adopt Secchi's classification of star spectra (p. 317), seeing that it does not satisfactorily treat bright-line stars like γ Cassiopeiæ, and those of Orion which give almost continuous spectra. The classifications suggested by Vogel and Lockyer both have the advantage of detail, and the latter is certainly the most philosophical. On p. 318 it is stated that stars of Secchi's fourth type usually "show a few bright lines," in addition to the carbon absorption bands, an idea of Secchi's which was shown to be erroneous several years ago.

The book is abundantly illustrated, and most of the diagrams are excellent. Fig. 119, however, gives a very bad impression of the spectrum of a nebula, the three bright green lines being represented as almost equidistant, whereas they practically form a triplet. A useful "Uranography" is given at the end. This embraces the more important celestial objects in the northern hemisphere and some degrees south, and is accompanied by a series of star maps. In the maps a convenient system of indicating magnitudes is adopted, but it has the disadvantage of destroying the appearances of the constellations for rapid identification.

A. F.

OUR BOOK SHELF.

Physiology of Bodily Exercise. By Fernand Lagrange, M.D. (London: Kegan Paul, Trench, and Co., 1889.)

THIS book at first sight reminds one of the saying that a German takes a year to make a research, and a week to write an account of it, while a Frenchman takes a year to write a book on one week's work. The only original part consists of a few experiments on the influence of fatigue in producing increased excretion of urates in the urine. The author ascribes most of the ill effects of

fatigue to the presence of uric acid in the blood—in fact, considers a fatigued man to be in exactly the same condition as a gouty man. His observations, however, seem to have been very few in number, and the analyses were all made for him by a friendly chemist. Still, it is unfair to the book to regard it as a contribution to the advance of physiological science. It is really an excellent little account of the physiology of bodily exercise, and its *rôle* in the maintenance of health, by a medical practitioner. It seems to be chiefly culled from the standard French works on general physiology, and on the physiology of movement. The author has digested his materials well, and so produced a very readable and lucid account of his subject. For a book of its class, it is remarkably free from mistakes, though physiologists might not agree with him in his account of the production of breathlessness or the causation of gout.

The style is simple, and the book is well adapted for popular use, and ought to find favour with our exercise-loving countrymen. E. H. S.

Boilers—Marine and Land. By Thomas W. Traill, F.E.R.N., M.Inst.C.E. Second Edition. (London: Charles Griffin and Co., 1890.)

THIS volume is a second edition of a work noticed in these columns last year. It was then a pleasure to express the opinion that the work would be useful to all connected with this particular branch of mechanical engineering. The author has found it necessary to extend the tables of scantlings, &c., from 160 to 200 pounds pressure per square inch. This in itself is sufficient evidence of the continued increase of steam pressures used in marine and stationary engines—probably the only practicable direction in which greater economy of fuel is to be obtained. These increased steam pressures have also the advantage of diminishing the gross weight of machinery on board ship.

The greater use made of mild steel by engineers generally is interesting, considering the fight the steel manufacturers had a few years ago to get it used at all in place of iron for many purposes. Mr. Traill observes that, "notwithstanding the peculiarities of mild steel, it is a material which may be used with safety and advantage, if proper precautions be taken and due consideration given to these peculiarities; possibly it has fewer infirmities than iron; and there can be no doubt that it is a better and more serviceable material for general use in the construction of boilers." This is the experience of most engineers intimate with the general behaviour of the material when being worked up into boilers and other constructions. To the many tests and safeguards specified to prevent the use of a brittle and bad steel in any erection is due the present excellence of this material, nor should they now be in any way relaxed, for to accept material, either iron or steel, on any particular brand is a mistake.

The general utility of the work has been increased by the addition of other matter and tables. The volume cannot fail to be of very great use to engineers. It is nicely printed, got up in a handy size, and strongly yet pliably bound. N. J. L.

The History and Pathology of Vaccination. Edited by Edgar M. Crookshank, M.B. Two Vols. (London: H. K. Lewis, 1889.)

THE arguments adopted in this work belong to a mental attitude identical with that displayed by anti-vaccinators in their clamorous treatment of the subject. They are sophistical from beginning to end, and even as a book of reference the volumes are not without drawbacks.

Firstly, the argument is that cow-pox is to be regarded as akin to syphilis rather than to small-pox, and that therefore cow-pox is no protection against small-pox. On this hypothesis ulcerated arms sometimes occurring after vaccination are to be regarded as reversions to type, rather than as due to the ill-treatment by over-anxious mothers not content to let Nature alone in her progress towards recovery. Having assumed that vaccination is no protection against small-pox, the book goes on to show that the only means we have of controlling the devastations of this disease is by attention to sanitary arrangements and by isolation, perhaps combined with judicious inoculation. The latter, the book assures us, is a more scientific procedure than the inoculation of cow-pox. Next, the author is very angry with Jenner for calling vaccinia, "cow-pox" or "variola vaccinia." To this stroke of dexterity by Jenner is to be attributed, says Prof. Crookshank, all the credit that vaccination has attained; thus for a single happy thought Parliament gave Jenner £30,000 as a consequence of his conceit, and England has been made to submit to the most tyrannical of laws.

This carping at the pioneer of new knowledge, and more especially at those forecasts of his which necessarily could only be verified by the lapse of time, is certainly not calculated to shake the faith of those who now fully comprehend not only the immense value of vaccination, but also the small amount of mischief which it has ever done.

The best that can be said for Prof. Crookshank's work is that it is well published. The printing is bold and clear, and the lithographs, such as they are, well reproduced.

Vol. ii. contains reproductions of original papers, most if not all of which are out of print, and cannot now be obtained except at fancy prices.

Had Prof. Crookshank been satisfied with editing these, and had he refrained from expressing his opinions, we should have been grateful to him. The book does not pretend to be a practical work on the subject of which it treats; and for the rest it might have been compiled by the average anti-vaccinator. ROBERT CORY.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Transmission of Acquired Characters, and Panmixia.

I SUPPOSE that a correspondent has no claim to limit the scope of a discussion in such a journal as NATURE. At the same time I feel it to be a rather severe burden when I am called upon to expound, in answer to one letter after another, the merest common-places of the subject under discussion, and to retail in this place the substance of books like Weismann's "Essays" and Wallace's "Darwinism" (to which the attention of your readers has been already drawn by reviews), not to mention the "Philosophie Zoologique" and the "Origin of Species." It seems to me that there might be interest and profit in opening your columns to the statement of newly observed cases which seem to tell in favour of either the Lamarckian or the anti-Lamarckian theories, or to novel criticisms of any cases which have already been discussed elsewhere; but surely the repeated citation of familiar exploded "cases," and the reiteration of arguments and beliefs which have long since received attention, is not fair to the writers who have dealt with these cases and these arguments in admirable treatises which are well known (I am happy to think) to nearly all serious students of these questions.

When I saw the distinguished name of Mr. Herbert Spencer at the end of a letter in your issue of March 6, I anticipated some real contribution to the discussion as to whether acquired characters are transmitted or not. Mr. Spencer some few years ago expounded his convictions in favour of Lamarck in one of the monthly reviews. His present letter is not only disappointing, but is unfortunately likely to mislead the uninformed. Mr. Spencer states what we all know, viz. that Mr. Darwin considered that the effects of habit and of use and

disuse are transmitted from the affected generation to its offspring. He refers by chapter and page to the instances which Mr. Darwin considered as examples of the transmission of the effects of habit or of use and disuse. He then says: "Clearly the first thing to be done by those who deny the inheritance of acquired characters is to show that the evidence Mr. Darwin has furnished by these numerous instances is all worthless." I entirely disagree with this way of putting the matter. It is not necessary to show that anything Mr. Darwin wrote was "worthless," but it is necessary to show that certain facts cited by Mr. Darwin admit of another interpretation or explanation than that which he gave to them. Naturally those who have taken up the anti-Lamarckian position have done long ago what Mr. Herbert Spencer says is the first thing for them to do. Of course the cases cited by Darwin were the first to be dealt with. It is extremely unfortunate that Mr. Spencer has not come across the work in which this is done. Otherwise, instead of a well-meant direction from Mr. Spencer as to what we ought to do, we might have the advantage of reading what he has to say after considering what has been done. It is seven years since Prof. Weismann published his essay on heredity; last spring this and other essays appeared in English under the auspices of the Clarendon Press. In that particular essay Darwin's cases are dealt with at length. Am I to reproduce Prof. Weismann's essay or a *précis* of it in this letter? Will not Mr. Spencer and others who are interested in these matters read Weismann's "Essays"? I think that those who will take the trouble to do so will see that Mr. Spencer's injunction was superfluous.

It is, however, apart from other branches of the question, important that a correct appreciation of Mr. Darwin's position in this matter of the "transmission of acquired characters" should be arrived at. Mr. Herbert Spencer's letter is, I think, likely to produce an erroneous conception on this matter. We know from his letters published since his death that Darwin held the "Philosophie Zoologique" to be "veritable rubbish"—"extremely poor; I got not a fact nor an idea from it." The notion that his own view was a modification of Lamarck's appeared to Darwin absurd. The "obvious view" was propounded by Lamarck, he says, "that if species were not created separately they must have descended from other species, and I can see nothing else in common between the 'Origin' and Lamarck." This was Mr. Darwin's attitude of mind to Lamarck's theory, and the cases in which he attributes importance to the effects of use and of disuse, and to acquired habit, and consequently to the Lamarckian principle of the transmission of acquired characters, are clearly to be regarded as concessions or admissions on his part, given with increasing generosity in the later editions of the "Origin"; but always treated as of quite subordinate importance. It is not going too far to say that Mr. Darwin never troubled himself very much with the question as to whether acquired characters are transmitted or not. It was the object of his works to show that the main effective principle in the origin of species is the natural selection in the struggle for existence of congenital characters. He explicitly states that he believes other causes to be at work; one of which at least, viz. sexual selection, he himself investigated at length. It must be remembered that no evolutionist in Darwin's life-time had prominently challenged the truth of the Lamarckian assumption that acquired characters are transmitted. For Darwin it was sufficient to show that, granting such a process to take place, it would not account for much; he was content to accept it as a subordinate factor. His view is best stated in his own words in the "Origin of Species": "On the whole we may conclude that habit, or use and disuse, have, in some cases, played a considerable part in the modification of the constitution and structure."

Whilst it is true that Mr. Darwin in various parts of his works alludes to cases which he interprets as due to the transmission of characters acquired by parents through habit, use, or disuse, it is obvious, when we read what he has to say in each case (as in the examples cited by Mr. Herbert Spencer), that he preferred, where it occurred to him another interpretation. Thus, after referring to the wings of the logger-headed duck and the domestic Aylesbury duck as dwindled by the transmission in successive generations of the effects of disuse, he interposes his own explanation by natural selection of the wingless beetles of Madeira, prefaced by the words: "In some cases we might easily put down to disuse modifications of structure which are wholly or mainly due to natural selection." He refuses to regard the defective anterior tarsi of dung-beetles as due to inherited mutilation, though he supposes they may have become deficient through disuse. He regards the defective eyes of cave-animals as due to the inheritance of the effects of disuse. I can scarcely doubt that, had it occurred to him, he would have preferred an explanation similar to that given by him of the wingless island beetles, viz. that a natural selection of animals with defective eyes takes place in a cave; since ultimately only those remain in a cave and breed in it which, in the course of their wanderings, are unable to see the faint light which penetrates to a great distance from the mouth, and must guide all those but the congenitally blind or weak-sighted to the exterior. The defective eyes of moles are ascribed by him not merely to disuse but to the selective action of inflammation. The case of the silkworm caterpillars with defective instincts (which is one of those given by Mr. Spencer) does not appear to me to bear on the present question. Of acquired characters, other than those due to disuse, Mr. Darwin accepts very few as being transmitted. He accepts the statements of Brown-Séquard as to the transmission of the effects of mutilations of guinea-pigs only so far as to "make us cautious in denying such transmission." He regards the dislocation of the eye of flat-fishes as due to the inheritance in successive generations of an increasing displacement caused by muscular effort. Besides these two instances (noted by Mr. Spencer) there is one other prominent passage in which Darwin asserts his belief in the inheritance of an acquired character which is not merely the result of disuse. I am anxious to separate those cases which Darwin speaks of as "due to the effects of disuse," for a reason which will appear below. The additional passage not noted by Mr. Spencer is this ("Origin of Species," p. 206, sixth edition):—"If we suppose any habitual action to become inherited—and it can be shown that this does sometimes happen—then the resemblance between what originally was a habit and an instinct becomes so close as not to be distinguished. If Mozart, instead of playing the pianoforte at three years' old with wonderfully little practice, had played a tune with no practice at all, he might be truly said to have done so instinctively. But it would be a serious error to suppose that the greater number of instincts have been acquired by habit in one generation and then transmitted by inheritance to succeeding generations. It can be clearly shown that the most wonderful instincts with which we are acquainted—namely, those of the hive bee and of many ants—could not possibly have been acquired by habit."

The cases of the epileptic guinea-pigs, the eyes of flat-fishes, and of some acquired habits, have been discussed by Weismann and by Wallace. I will not now allude further to those classes of cases. But I am anxious to draw attention to the special subject of the "effects of disuse" as set forth by Mr. Darwin. This phrase is not only used by him in regard to special instances, but, in treating of the large subject of rudimentary organs, he frequently refers to the "effects of disuse." He says, "It appears probable that disuse has been the main agent in rendering organs rudimentary" ("Origin," p. 401).

Now I am anxious to point out three things in regard to the "effects of disuse." (1) There are other possible effects of *disuse* of an organ than the dwindling of that organ in one generation, and the inheritance of the organ in a diminished size by the next generation. (2) The anti-Lamarckians attribute a very great effect to disuse, although they do not attribute to it the particular result which Lamarck did. (3) The particular way in which, according to the anti-Lamarckians, disuse acts so as to lead to the dwindling or complete loss of the disused organ has been called by Weismann by a convenient name—"panmixia." The doctrine of panmixia is already indicated by Darwin himself, and in view of this fact we must suppose that, when he attributed the loss or dwindling of an organ to "disuse" or the "effects of disuse," he did not necessarily (though probably he frequently did) refer to the Lamarckian *modus operandi* of disuse, but may very well have had in mind the results which are attributed to disuse by the anti-Lamarckian doctrine of panmixia.

The doctrine of panmixia is this. When there is no longer, owing to changed conditions of life, any use for an organ, it will cease to be the subject of natural selection. Consequently all possible variations of the organ will have (so far as the now lapsed use of the organ is concerned) an equal chance. Amongst the possible variations there will be the variation in the direction of increased size, and its exact complement—the variation in the direction of diminished size. Prof. Weismann has stated briefly that this equal survival of all possible variations must lead to the

dwindling and ultimate loss of the organ. I would, however, venture to supplement what he has said by the following: viz., given the state of panmixia, it is apparent that variations in the direction of excessive size will be injurious—both as taxing the nutriment of the organism, and often as mechanical encumbrance. On the other hand, variations in the direction of greatly diminished size will be advantageous, as causing a diminished tax on the resources of the organism. Now it is a demonstrable fact that excessive variations in both directions *do* naturally though rarely occur—probably more often than is supposed, since we do not see all the young born. If the variations in the direction of excessive diminution of a useless organ (as, for instance, tailless cats or hornless sheep) survive as being less taxed—whilst the complementary variations in the direction of excessive size tend in the struggle to die without reproducing, owing to their awkwardness and their relatively greater burden in life—then it is clear that panmixia may lead rapidly to the dwindling and eventual extinction of a disused organ without any transmission of *acquired* parental character. The fact that there is no use for an organ—or, in other words, the "effect of disuse"—is that the congenitally small varieties of the organ survive, and are even favoured in the struggle for existence.

Whilst Weismann has the merit of having insisted on a form of his doctrine as the effective reply to those who argue in favour of Lamarck's theory of the transmission of acquired qualities from instances of "disuse," it is yet the fact that Mr. Darwin himself recognized and formulated the doctrine of panmixia in the last (sixth) edition of the "Origin of Species," published in 1872; and he even went further than Weismann, for he associated the principle of the economy of material with the principle of the cessation of selection. It is therefore, it seems to me, not at all improbable that when Darwin refers, here and there throughout his works, to a reduced or rudimentary condition of an organ as "due to disuse," or "explained by the effects of disuse," he does not *necessarily* mean such effects as the Lamarckian second law asserted and assumed (though often he does appear to mean such); but he may mean, and probably had in his mind, the effects of disuse as worked out through panmixia and economy of growth.

The passages in Darwin which seem to me to have been missed or neglected by those who think panmixia altogether a new idea are as follows:—

(1) "If under changed conditions of life a structure before useful, becomes less useful, its diminution will be favoured for it will profit the individual not to have its nutriment wasted in building up a useless structure." After an example in point from the group of the Cirripedia, Darwin continues: "Thus, as I believe, natural selection will tend in the long run to reduce any part of the organization as soon as it becomes, through changed habits, superfluous, without by any means causing some other part to be largely developed in a corresponding degree" ("Origin of Species," sixth edition, p. 118).

(2) "Organs, originally formed by the aid of natural selection, when rendered useless, may well be variable, for their variations can no longer be checked by natural selection. . . . It is scarcely possible that disuse can go on producing any further effect after the organ has once been rendered functionless. Some additional explanation is here requisite, which I cannot give. If, for instance, it could be proved that every part of the organization tends to vary in a greater degree towards diminution than towards augmentation of size, then we should be able to understand how an organ which has become useless would be rendered, independently of the effects of disuse, rudimentary, and would at last be wholly suppressed; for the variations towards diminished size would no longer be checked by natural selection. The principle of the economy of growth explained in a former chapter [cited in quotation No. 1], by which the materials forming any part, if not useful to the possessor, are saved as far as possible, will perhaps come into play in rendering a useless part rudimentary" ("Origin of Species," sixth edition, pp. 401–402).

I had written thus far, and intended to finish this letter by asking if the anti-Lamarckians are not really carrying out the spirit of Darwin's doctrines, although not the absolute letter, when I received your issue of March 13, containing a long letter from Mr. George Romanes, headed "Panmixia." In that letter Mr. Romanes, whilst amending (as I have done above) Prof. Weismann's statement of the principle of panmixia, makes the definite assertion that "it is remarkably strange that this principle should have been overlooked by Mr. Darwin."

Probably your readers will be as much astonished as I was when they read the extracts I have above given from the "Origin of Species" by the side of Mr. Romanes's letter.

After dismissing Mr. Darwin, Mr. Romanes proceeds to say: "In this connection, however, it requires to be stated that the idea first of all occurred to myself, unfortunately just after the appearance of his last edition of the 'Origin of Species.'"

Now, inasmuch as the idea in question is (as I have shown above) formulated in the last edition of the "Origin of Species," I confess that I do not think it requires to be stated that the idea occurred to Mr. Romanes shortly after the publication of that work. What more natural? The idea occurred to me also shortly after the passages above quoted from Mr. Darwin were published. It certainly never appeared to me "unfortunate" that this was the case, and I cannot see where the misfortune comes in in regard to Mr. Romanes. As soon as the matter had taken root in his mind, Mr. Romanes published in NATURE, March 12, April 7, and July 2, 1874, an exposition of the importance of the principle of cessation of selection as a commentary upon a letter by Mr. Darwin himself (NATURE, vol. viii. pp. 432, 505) in which Mr. Darwin had suggested that, with organisms subjected to unfavourable conditions, all the parts would tend towards reduction. Mr. Darwin, with his usual kindly manner towards the suggestions of a young writer, gives at p. 309 of vol. ii. of "Animals and Plants under Domestication" (second edition), Mr. Romanes's view, "as far as it can be given in a few words." The view, as it there appears in Mr. Darwin's words, is certainly *not* the same as that which Mr. Romanes has expounded in NATURE of March 13, 1890 (p. 437), and since it represents what Mr. Darwin had been able to gather from Mr. Romanes's letters to NATURE of 1874, it is not at all surprising that Mr. Darwin did not recognize any resemblance between it and his own statement, viz. that "the materials forming any part, if not useful to the possessor, are saved as far as possible," thus "rendering a useless part rudimentary." Whether this is, or was, Mr. Romanes's view or not, it is Darwin's, and is the essence of the anti-Lamarckian view of the effects of disuse.

March 15. E. RAY LANKESTER.

Exact Thermometry.

SHORTLY after the publication of my second letter on this subject (NATURE, January 23, p. 271) I received a letter from M. Guillaume, who very kindly called my attention to a paper by Prof. J. M. Crafts (*Comptes rendus*, xci. p. 370), in which the "plastic theory" is discussed. Prof. Crafts states that he has subjected thermometers to prolonged heating at 355° C., under various conditions as regards pressure, the internal pressure being in many cases considerably greater than the external, but that there was invariably a rise of the zero-point. The experiments were carried out in very much the same manner as that described in my first letter (NATURE, December 19, 1889, p. 152), and had I known at the time of the earlier work of Prof. Crafts, I should of course have referred to it. Prof. Crafts also describes and quotes experiments with air-thermometers, the temperature in one determination by Regnault being as high as 511° C., and the internal greater than the external pressure; in every case the bulb diminished in volume. From these results, Prof. Crafts concludes that it is not proved that pressure plays any part in the contraction of the glass.

My experiments can therefore be regarded as little more than confirmatory of the earlier work of Prof. Crafts and others, but as such it may be worth while to give the results. The method adopted was fully described in my first letter, and it is therefore only necessary to repeat that in thermometer A the external pressure exceeded the internal, while in thermometer C there was considerable internal pressure, but no external. According to the plastic theory, therefore, the zero-point of A should have risen, while that of C should have fallen. The results previously described were regarded as insufficient by Prof. Mills, and I have therefore continued the heating for a much longer time.

I have also made similar experiments with two other thermometers belonging to the same batch, at a temperature of about 356°, the thermometers being heated in the vapour of boiling mercury. During the first three hours, the two thermometers *a* and *b* were treated in precisely the same manner, as regards pressure, as A and C, and it will be seen that the zero-point of *b* showed a slightly greater rise than that of *a*. Afterwards, air was admitted into thermometer *a*, so that there was an excess of internal over external pressure in both thermometers, but the excess was greater by one atmosphere in *b* than in *a*.

The results obtained are given in the following table:—

Temperature 280°.

Total time in hours.	Duration of each heating.	Zero-point of A.	Rise of zero.	Zero-point of C.	Rise of zero.	Mean rise of zero per hour.
		°	°	°	°	°
0	—	0·15	—	−0·1	—	—
2	2	0·5	0·35	+0·3	0·4	0·187
7·5	5·5	1·3	0·8	1·1	0·8	0·145
12	4·5	2·0	0·7	1·8	0·7	0·156
17	5	2·3	0·3	2·05	0·25	0·055
22·5	5·5	2·6	0·3	2·15	0·1	0·036
29	6·5	2·95	0·35	2·5	0·35	0·054
35	6	3·15	0·2	2·8	0·3	0·042
86	51	4·1	0·95	3·95	1·15	0·021
133	47	4·8	0·7	4·9	0·95	0·018
201	68	5·25	0·45	5·5	0·6	0·008
369	168	6·5	1·25	6·8	1·3	0·008

Temperature 356°.

		a		*b*		
		°	°	°	°	°
0	—	0·4	—	0·05	—	—
3	3	6·0	5·6	6·1	6·05	1·942
6	3	8·0	2·0	8·1	2·0	0·667
12·5	6·5	10·3	2·3	10·35	2·25	0·350
15	2·5	10·95	0·65	11·1	0·75	0·280
66	51	16·1	5·15	16·1	5·0	0·100
113	47	18·45	2·35	18·3	2·2	0·048
181	68	20·1	1·65	20·0	1·7	0·025
205·5	24·5	20·75	0·65	20·6	0·6	0·025
221·5	16	20·9	0·15	20·7	0·1	0·008
292	70·5	21·8	0·9	21·7	1·0	0·013

The last result at 356° is a little uncertain, owing to a breakage of the apparatus.

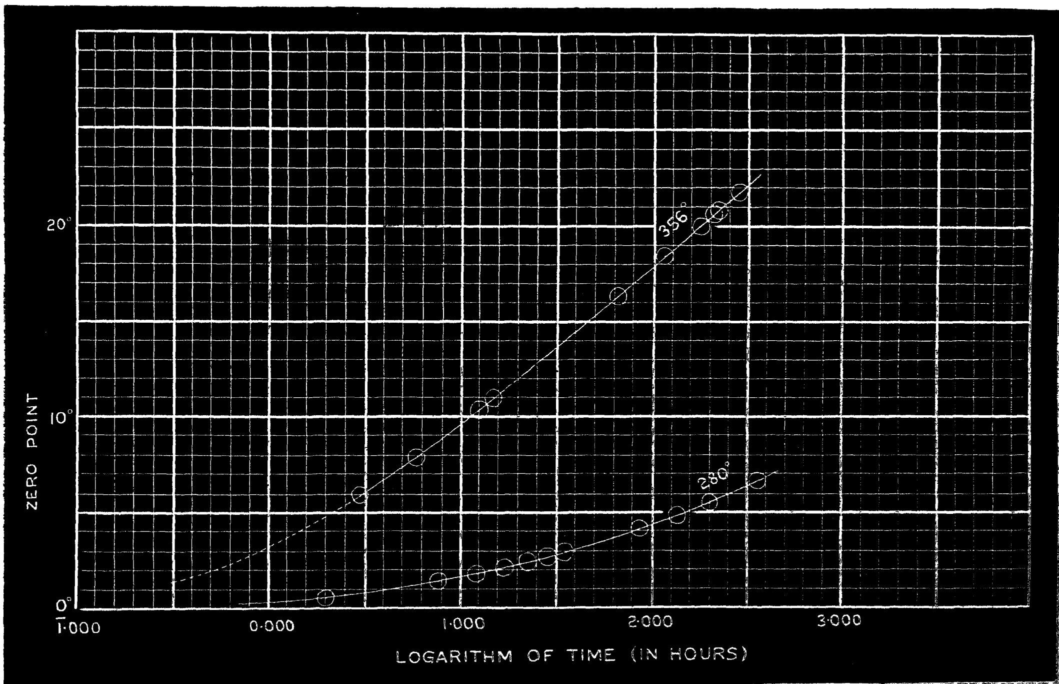
I may also mention that M. Guillaume has informed me that M. Tonnelot has heated several thermometers to 450°, and that, notwithstanding a considerable internal pressure, a rise of the zero-point was observed in every case.

All these results seem to lead unmistakably to the conclusion that pressure has little or no effect on the rise of the zero-point.

Three questions remain to be discussed—

(1) Would the total rise of the zero-point be different if two similar thermometers were subjected to sufficiently prolonged heating at different temperatures? At first sight, it would certainly appear that at 356° the total rise with my thermometers must be greater than at 280°, but I do not feel satisfied that the proof is sufficient. If we map the observations of zero-point against the time of heating, curves are obtained which appear as if they might become horizontal after a few weeks or, possibly, months; but if, instead of the actual times, we take their logarithms—as in the diagram—as abscissæ, there is no appearance of an approach to the final state at either temperature. But while at 356° the curve has become almost a straight line, at 280° there appears to be an increasing tendency towards the vertical direction. I do not for a moment argue that the curves indicate that the maximum rise would be the same at both temperatures if the experiments were carried on for a sufficiently long time; but, at the same time, I do not think that they afford any convincing proof that the total rise would be different. The results merely tend to increase my scepticism as to the value of the determination of the maximum rise at 0° obtained by extrapolation of the curve constructed from observations at that temperature. It does not appear to me that it would be justifiable to extrapolate these curves at all, and I am afraid that they do not throw much light on the total rise of zero-point at either temperature. Very much more prolonged heating would be necessary before arriving at a definite conclusion.

(2) With regard to the causes of the contraction of the bulb, I have no hesitation in admitting that—as shown by M. Guillaume—the removal of the condition of strain caused by the more rapid cooling of the *outer parts* of the glass, is insufficient to account for the results. No doubt we must also take into account the too rapid cooling of the glass *as a whole*, which prevents the molecules from assuming the position of greatest stability, perhaps in the same sort of way that the assumption by sulphur of the monoclinic or the more stable rhombic form depends on the rate at which solidification takes place. That there are other causes besides these two does not, at present appear to me to be proved.



(3) Lastly, there is the question raised by Mr. Tomlinson, as to whether repeated heating and cooling between wide limits of temperature is more effective in raising the zero-point than prolonged heating at the higher temperature. The points representing the individual observations fall very fairly on the curves constructed from them, and do not seem to indicate any noticeable difference in the effect of long or short heating. The results can hardly, however, be regarded as decisive.

University College, Bristol, March 1. SYDNEY YOUNG.

Foreign Substances attached to Crabs.

SINCE *Hyas* is one of the most abundant Crustaceans found off the east coast of Scotland, Mr. Holt must adduce considerably more than two instances before it can be admitted that the attachment of Simple Ascidians to this crab is at all a usual occurrence. If it is, I should still be anxious to inquire whether the crab does not—in spite of the apparent difficulty of the operation—place the Ascidians upon its back with its own nippers. I may cite Gosse's well-known experiment with *Pagurus prideauxii* and *Adamsia palliata*, described in his "Year at the Shore," for the purpose of analogy. But Mr. Holt will find a case, probably quite similar to that which he mentions, in Bell's "Stalk-eyed Crustacea." Two specimens of *Hyas araneus* were found with oysters attached to their backs, that on the larger crab being three inches in length, and five or six years old, probably a much more "serious incubus" than Mr. Holt's Tunicates. The crab's carapace was but two and a quarter inches in length. Hence, despite the "world of weight upon its shoulders," Mr. Thompson concluded that "the presence of this oyster affords interesting evidence that the *Hyas* lived several years after attaining its full growth." Probably the larvæ of the oysters, and of the Ascidians also, happened to alight upon the crabs at the end of their free-swimming existence, although six or seven years seems to me to be a remarkably long age for a *Hyas*.

Barnacles upon the backs of *Maia*, *Carcinus*, &c., are also due to the same, as it were, accidental cause.

But, whatever the explanation, these exceptional cases do not alter the fact that the foreign bodies found upon *Hyas* are usually fixed there by the crab itself. The specimens I have seen have been covered with fragments—not living colonies—of Algæ, Hydroids and Polyzoa, which are fastened by the hairs of the crab's carapace and legs exactly as in *Stenorhynchus*, and in this crab the process of attachment has been frequently observed here and accurately recorded.

At the same time I by no means hold that the two groups which were defined in my previous letter are absolutely marked off from one another. The hermit crabs make use of both methods of protection. Bits of Sponges may frequently be seen upon the carapace of *Maia*, *Stenorhynchus*, and *Inachus*, and I have occasionally found colonies of *Leptoclinum* and *Didemnum* upon both *Maia* and *Inachus*. In these cases the inconspicuous appearance is not lost, but the attachment of small Sponges and Didemnids is probably an additional protection against the numerous night-feeding fishes, which hunt their prey by the senses of smell and touch.

As to the inedibility of *Tunicata*, I did not—as Mr. Holt states—"assume" it. I have experimentally found it to be a fact (as I stated in my letter) that the odour and taste of "*Tunicata*, and especially Compound *Tunicata*," are almost invariably sufficient to prevent fishes from eating them. Exceptions do not disprove the rule, and it is quite possible that *Pelonaia* is not distasteful. But this is not established by a few specimens having been taken on one or two occasions from the stomachs of Cod, Haddock, and Dab; and although Mr. Holt quotes Prof. McIntosh as speaking of the "abundant" occurrence of *Molgula arenosa* in the stomachs of Cod and Haddock, he will find upon reading Prof. McIntosh's words again, that they are open to a different interpretation.

In my previous letter I omitted to mention that a species of hermit crab also, *Eupagurus lucasii*, takes advantage (regularly?) of the distastefulness of Compound Ascidians. Mr. Harmer has, with much kindness, examined for me a specimen in the Cambridge Museum. The crab inhabits a univalve which is covered with *Distaplia magnilarva*.

Mr. Holt's statement that "*Actinia mesembryanthemum* is certainly a favourite food of the Cod" is so astonishing that I hope he will adduce the evidence for his assertion. Mr. Brook had not found this to be so when he reported upon the food of this fish for the Scottish Fishery Board, and indeed only the youngest Cod ever frequent the tidal waters to which *A. mesembryanthemum* is confined. Further, although *Pagurus bernhardus*, when *not* associated with an Anemone, is very frequently found in the stomachs of Cod and Haddock, I do not know a single instance of its having been found in the stomachs of the same fish when associated with one.

I am informed by Mr. Poulton that, in a work which is shortly to appear, he has included such animals as *Stenorhynchus* and *Caddis worms*, which disguise their appearance with foreign bodies simply in order to escape identification by enemies, in a group to which he gives the very convenient name "allocryptic." Animals which trust rather to the offensive than to the inconspicuous character of the foreign bodies with which they associate themselves he terms "allosematic" (σῆμα, a sign).

It is obvious that the allosematic method of protection is all but perfect, since it is largely free from the loss due to experimental tasting attendant upon the method of a purely warning appearance ("autosematic"). WALTER GARSTANG.

Plymouth, March 21.

Sea-bird Shooting.

Is it not time that something more was done to stop the wholesale slaughter of our sea-birds? During the past winter the havoc has been terrible, and unless some restraint is imposed we may expect before long to find our shores denuded of their white wings. When the birds had no value, there was a limit, though a wide one, to their destruction, because of the cost of killing them; but recently a large demand has sprung up for their skins, and an organized traffic is now carried on in the carcases.

The shooter gets from threepence to sixpence per bird from the amateur dealer, and for the sake of this paltry sum (surely the birds are worth more to us alive than this!) there is not a sporting lounger on the coast who can possess himself of a gun who does not kill every bird which can be reached either from the shore or from a boat. The gulls are pursued, I am told, even as far as the Dogger Bank.

The beautiful kittiwake is the greatest sufferer. One of the dealers boasted to me the other day that he had passed "nearer ten than nine thousand dead birds through his hands this season, chiefly kittiwakes." He added that he had got *804 carcases in one batch from one sportsman.*

From inquiries, I judge that this person's trade represents about one-third of the dead birds which have been sent away from our little town this season. I know the traffic is carried on at other points, and no doubt this is but an example of what is going on all round our coast. When we consider that the carcases which can be secured represent only a fraction of the birds killed or injured, we gain some idea of the extent of the mischief. Indeed, during the past month it has been possible to take a long walk along our shore without seeing a single sea-gull. Who wishes to see a blank seascape?

Now, surely, we all have equal rights in these graceful birds, and the numerous class who love to see them alive deserve as much consideration as the mischievous minority whose pleasure it is to destroy them! It is not as though these latter were worthy persons, compelled to a cruel employment for their daily bread: they are, on the contrary, nearly all of a class who deserve no sympathy—of a comfortable class who, I verily believe, would shoot their next-door neighbours if they could do so with impunity, and could dispose of the carcases! Just imagine the new variety of "sport" which one of them described to me not long ago! He said you could catch the gulls at sea by baiting a floating fishing-line with liver, and in this way, though you did not get quite so many as with a gun, you had far better *fun*, especially from the kittiwakes, as they are wonderfully "game," and, when they feel the hook, "flacker about and scream like a child"!

Is it too much to ask that our Legislature, which has spent so much time in the past on laws in the interests of the so-called "preservers" of game, will do something, and that speedily, in the interests of those who would fain be truly preservers of the sea-birds? At least they should extend the protection afforded to "game" to these noble birds, and order that those who shoot them shall pay a heavy license for their despicable sport, and those who deal in the dead carcases a still heavier.

And nothing in this matter must be left to local authorities. In seaside places self-interest vitiates the sentiment on this question. The fisherman finds it easier to earn money by letting his boat to the "sportsman" than by his legitimate productive industry; the tradesman fears to lose these men's custom; and the gentry, mostly supporters of "sport," are perhaps not sorry to have such an excellent safety-valve for guns which might otherwise poach on their preserves; and besides, there is in Yorkshire a semi-political aspect to the matter. Thus it has happened that of late years the clause in the (so far as it goes) excellent "Sea-birds Preservation Act" of 1869, which permits a lengthening of the close-time under certain conditions, has been rendered

nugatory through the action of our county magistrates, who have refused to present the requisite petition to the Home Office. They must have been aware that their action doomed innumerable young birds to death by starvation, since the cliff-climbers collect the eggs until July (a perfectly legitimate industry, by the way, carried on by hard-working men, and producing valuable food), and thus render it impossible for the majority of the birds to get their young reared by the 1st of August.

And, in consequence, whenever during August I go on the shore under the great cliffs where the birds breed, my ears are filled with the melancholy "piping" of the starving helpless young, dying slowly on the ledges, whose parents have been shot—for sport, or threepence. G. W. LAMPLUGH.

Bridlington Quay.

Locusts.

WITH reference to the flight of locusts which passed over the steam-ships *Golconda* and *Clyde* in the Red Sea about November 25 last, it would be interesting to ascertain to what species they belong. The past year, 1889–90, has been marked in India by the invasion of locusts belonging to the species *Acridium peregrinum*, which, starting, it is believed, about the end of the hot weather (May or June), from the sand-hills of Western Rajputana, have, during the past six months, spread in vast numbers over the whole of Sind, Rajputana, the Punjab, North-West Provinces, and Oudh, besides penetrating sporadically into Guzerat, Ahmedabad, Baroda, Khandesh, and parts of Central India, a stray flight even appearing in the Kistna district of the Madras Presidency.

This insect, which is supposed to be the *locust* of the Bible, and which is undoubtedly the one that periodically invades Algeria from the Sahara, though it is altogether distinct from the locust *Stauronotus maroccanus*, of which so much has been heard in Algeria during the past two years, is likely to be the species which was observed in the Red Sea. To ascertain the point, however, with certainty, it is essential that specimens, which I am told fell upon the deck of the ship *Clyde* in considerable numbers, should be examined and determined entomologically, and my object therefore in addressing you is to endeavour to obtain some of the specimens for comparison with those which have invaded India.

It is worthy of notice that in 1869 when Rajputana suffered considerably from locusts, vast swarms were also observed by ships passing through the Red Sea, and it would therefore be interesting to learn to what extent 1869 and 1889 were years of locust invasion in the intervening countries of Arabia, Persia, and Biluchistan. It is much to be regretted that in 1869 neither the locusts found in Rajputana nor in the Red Sea appear to have been preserved or determined, and their identity therefore cannot be definitely established. E. C. COTES.

Indian Museum, Calcutta, February 28.

THE ROYAL METEOROLOGICAL SOCIETY'S EXHIBITION.

THE eleventh Annual Exhibition of the Royal Meteorological Society was held at the Institution of Civil Engineers on March 18 and three following days. Each Annual Exhibition is devoted to some special branch of meteorology, which is illustrated by specimens of all known instruments (or drawings and descriptions of the same) that have been employed in its investigation. This year's Exhibition was illustrative of the application of photography to meteorology. Photographic meteorological instruments are not numerous, and those used for recording the indications of the barometer, thermometer, and electrometer are very costly and delicate, and are only made to order. The number of instruments in the Exhibition was consequently less than in previous years, but this deficiency was fully made up by the large and highly interesting collection of photographs of meteorological phenomena.

The earliest application of photography for the continuous registration of the barometer, &c., was made by Mr. T. B. Jordan, of Falmouth, in 1838. His plan was to furnish each instrument with one or more cylinders containing scrolls of photographic paper. These cylinders were made to revolve slowly by a very simple connection with a clock, so as to give the paper a progressive movement behind the index of the instrument, the place of which was registered by the representation of its own image.

In 1846, Mr. Charles Brooke and Sir Francis Ronalds each brought forward a method for the registration of magnetic and meteorological instruments by means of photography. The methods are those now in use, the former at the Royal Observatory, Greenwich, and the latter at the Observatories of the Meteorological Office.

Although these instruments were not shown, they were fully illustrated by photographs and drawings. A number of the barograms and thermograms were exhibited by the Astronomer-Royal and the Meteorological Council, showing the passage of storm centres, and sudden changes of temperature and humidity. A set of barograms from various parts of the world was exhibited by the Meteorological Council, showing the barometric oscillation due to the Krakatão eruption, August 1883. The thermogram at Kew on May 8, 1871, showed a fall of about 20° of temperature during a thunderstorm at 4 p.m.

Mr. Symons exhibited a photographic scale showing the intensity of sunlight during the solar eclipse of July 18, 1860; and the Kew Committee showed the chemical photometer devised by Sir H. Roscoe in 1863. Mr. J. B. Jordan exhibited his experimental instrument for recording the intensity of daylight, and also the three patterns of his sunshine recorder. Similar instruments designed by Dr. Maurer, of Zürich, and Prof. McLeod, were also shown. Prof. Pickering sent a photograph of his Pole-star recorder, in use at the Harvard College Observatory, U.S.A., for registering the cloudiness during the night. This instrument consists of a telescopic objective attached to a photographic camera and directed to the Pole-star; the camera is provided with very sensitive plates which are inserted in the evening, and a shutter, worked by an alarm clock, is closed before dawn. If the sky be clear during the night, the plate, after development, shows a semicircle traced by the revolution of the star around the North Pole, but if clouds have passed across the star, the trace is broken.

The photo-nephograph designed by Captain Abney for the registration of the velocity and direction of motion of clouds was exhibited by the Meteorological Council, as well as a model showing the manner in which the pair of photo-nephographs are mounted for use at the Kew Observatory. One of the instruments is placed on the roof of the Observatory, the other being at a distance of 800 yards; the observers at each end are in telephonic communication. Both cameras being oriented with reference to the same point of the horizon, the distant observer is instructed as to the direction and elevation of his instrument. The chief observer controls the exposure, both cameras being exposed simultaneously; another pair of plates are exposed after an interval of one minute. A slide rule designed by General R. Strachey for obtaining the height and distance of clouds from the pictures yielded by the cloud cameras was also exhibited, as well as photographs of an experimental apparatus designed by Mr. G. M. Whipple for the same purpose.

The Exhibition included a large and interesting collection of photographs of clouds. Padre F. Denza sent a set of 80 cloud photographs which had been taken during the past twelve months at the Specula Vaticana, Rome. M. Paul Garnier exhibited a magnificent set of 17 large photographs of clouds taken at his observatory, Boulogne-sur-Seine, Paris. These are the best photographs of clouds that have been seen in this country, and they were consequently very much admired. M. Garnier has not yet explained the method he adopts for obtaining such beautiful pictures. Dr. Riggen-

bach, of Basle, showed some photographs of cirrus clouds taken by reflection from the surface of the Lake of Sarnen. In this case the surface of the water acts like a polarizing mirror, and extinguishes the sky light. Photographs of clouds were also exhibited by Mr. Clayden, Dr. Drewitt, Dr. Green, Mr. Gwilliam, Mr. Harrison, Mr. McKean, Messrs. Norman May and Co., Mr. H. C. Russell, and Mr. Symons. Mr. H. P. Curtis, of Boston, U.S.A., sent a valuable and highly interesting collection of photographs, showing the devastation caused by the tornadoes at Rochester, Minnesota, on August 21, 1883, and at Grinnell, Iowa, on June 17, 1884. After seeing these photographs, some idea can be formed of the immense destruction wrought by these terrible scourges, which so frequently visit various parts of the United States. Mr. Curtis also exhibited three photographs of the tornado cloud; two of these were taken at Jamestown, Dakota, on June 6, 1887, when the cloud funnel was 12 miles to the north; the third, which was taken in New Hampshire, during the storm on June 22, 1888, shows the spiral-shaped funnel trailing at a considerable altitude in the air.

Many interesting photographs illustrating meteorological phenomena were exhibited. These included floods, snow-drifts, hoar-frost, frozen waterfalls, &c. A large number of photographs of flashes of lightning taken during the last twelve months were also shown, as well as some photographs of electric sparks, taken by Mr. Clayden and Mr. Bidwell, which explain the formation of dark images of lightning-flashes.

Mr. Clayden exhibited a very interesting and instructive working model, showing the connection between the monsoons and the currents of the Arabian Sea and the Bay of Bengal.

Mr. Dines showed a model of the whirling machine used by him at Hersham for testing anemometers and for experiments on wind-pressure; he also exhibited a remarkable curve showing the normal component of the wind-pressure upon a sloping surface 1 foot square, the normal pressure being taken as 100, and the pressure at various angles of inclination being expressed proportionately. Mr. Munro sent two instruments which he has recently constructed in conjunction with Mr. Dines. The first is for showing the velocity of the wind. The shaft of an anemometer is connected with the shaft of the instrument, and in turning works a small centrifugal pump, thus raising the level of the mercury in the long cistern. The deflection of the pendulum from the vertical position is proportional to the rate of turning, and thus gives a uniform scale. The second instrument is for showing the pressure of the wind from a velocity anemometer. The arrangement is the same as in the preceding instrument, but the fall of the float in the small circular cistern is proportional to the square of the velocity and therefore to the wind-pressure, thus giving a scale of pressure with the divisions at uniform distances.

Mr. Hicks exhibited Draper's self-recording metallic thermometer; a mercurial minimum thermometer with lens front; and a radial scale thermometer. Mr. Long showed Trotter's compensating thermometer for taking temperatures at any distance; and Mr. Denton exhibited his clinical thermometer case with new spring-catch.

WILLIAM MARRIOTT.

THE ORIGIN AND COMPOSITION OF THE FLORA OF THE KEELING ISLANDS.

AT intervals I have contributed to NATURE the results of the more recent investigations of insular floras, more especially in relation to the dispersal of plants by ocean currents, birds, and winds; and now, through the courtesy of the author and Captain Petrie, Honorary Secretary of the Victoria Institute, I am able to furnish a commentative summary of a lecture[1] by Dr. H. B. Guppy, on the flora of the Keeling Islands.

It is hardly necessary to mention that Darwin visited these islands in 1836, except in connection with the fact that Dr. Guppy's visit was in a measure an outcome of that event. In 1878, Mr. H. O. Forbes spent some time there, and extended our knowledge of the flora. Primarily, no doubt, the coral-reef question took Dr. Guppy to the scene of Darwin's early labours, though he was probably not less interested in the flora, having been stimulated by practical botanizing in the Solomon Islands a few years previously; and a stay of nearly ten weeks enabled him to elucidate many points that were either obscure or conjectural.

Mr. John Murray, of the *Challenger* Expedition, found funds for Dr. Guppy's mission, and he presented to the Kew Herbarium the collections made of dried plants and drifted seeds and fruits; and there, such of them as were not already familiar to Dr. Guppy, and of which the material was sufficient, were named, and a set incorporated.

For the sake of brevity it will be better to describe what Dr. Guppy has accomplished, rather than follow him through his account of it.

Specimens were taken of all the different species of plants found in a wild state in the islands; notes made of the conditions under which they occurred, of their relative frequency, of their chances of propagation, and of their natural enemies, besides other particulars. In addition to seeds, or fruits containing the seeds, of the plants actually established on the islands, many others were picked up on the beach, where they had been deposited by the waves. Whilst most of these were in various stages of decay, others were actually germinating, and the question arose, Why had they not succeeded in obtaining a footing? As we shall presently learn, this question was easily answered.

Another point on which we had little trustworthy information was the length of time various seeds of essentially littoral and insular plants would bear immersion, or, rather, flotation, in sea-water without losing their vitality. With the exception of a few isolated instances of seeds having germinated after having been carried across the Atlantic to the western coast of Europe, very little was known, because the majority of the seeds experimented with by botanists at home did not belong to this class of widely-spread plants. Dr. Guppy instituted experiments on the spot, and although his time was too short to determine the extreme limits of endurance of the various seeds, he was able to prove that certain kinds germinated freely after being thirty, forty, or fifty days in sea-water. Again, he observed that some seeds that do not readily float, or only for quite short periods, are conveyed hither and thither in a variety of ways—such as in the cavities of pumice-stone, and in the crevices of driftwood.

From all available evidence, it is almost absolutely certain that there were no permanent inhabitants of the Keeling Islands till about the end of the first quarter of the present century; and from the most trustworthy accounts the islands were covered with vegetation, the coco-nut largely preponderating in the arboreous element. Indeed, as the outer part was almost entirely coco-nut, it seemed, as Darwin says, at first glance to compose the whole wood. But there is evidence that there were large "forests" in the interior of the islands, consisting mainly of the iron-wood, *Cordia subcordata*. The largest island is said to be only about five miles long; and the group is between 600 and 700 miles from the nearest land, excluding the small Christmas Island.

Already at the time of Darwin's visit in 1836, the islands were in the possession of Captain Ross, the

[1] "The Dispersal of Plants, as illustrated by the Flora of the Keeling or Cocos Islands." A Paper read at a meeting of the Victoria Institute on Monday, February 3, 1890, by Dr. H. B. Guppy.

grandfather of the present proprietor, and coco-nut planting was progressing. Since then most of the available ground has been cleared of other vegetation and planted with coco-nut trees, so that the wild vegetation is nearly limited to an external fringe, and this often broken. In North Keeling, about fourteen miles distant from the main group, which was not visited either by Darwin or Forbes, there was still sufficient of the original vegetation left for Dr. Guppy to form an idea of what it was generally before it was cleared away for cultivation. Darwin's investigations had the effect of arousing the interest of Captain Ross in the natural history of the group, and this interest has been inherited by his descendants, who have greatly aided subsequent travellers by their hospitality and by their knowledge of local phenomena. Darwin collected or noted about a score of different species of wild plants, and this number has now been doubled by Forbes and Guppy.

This brings us to the results of Guppy's own investigations, the most interesting and important being those relating to the capabilities of certain plants, notably the coco-nut, to establish themselves on coral islands, as some writers of repute have strongly contested the possibility of it, and there can be little doubt that the coco-nut and other plants having large seeds obtain a footing only under exceptional circumstances, such as being buried by the sands washed over them in heavy gales.

Foreign coco-nuts are frequently cast ashore on the Keeling Islands, where they sometimes germinate, but the crabs invariably destroy the sprouting nut. Suppose, however, a period when crabs were less numerous, and the chances are not so very remote of some of the growing nuts escaping them. Again, Mr. Forbes cites an instance in which the crabs may even facilitate the establishment of the coco-nut, for he observed that the crabs sometimes burrow so near the surface that the nuts occasionally break through and find favourable conditions for growth. Should they escape the crabs in their earliest infancy, they are safe. Many other plants are now prevented by the crabs from establishing themselves on the Keeling Islands. Dr. Guppy says:—

"I have been informed by the proprietor that sometimes when a large amount of vegetable drift has been stranded on the beach, a line of sprouting plants may be shortly observed just above the usual high-tide mark; but the tender shoots are soon eaten by the crabs, and in a little time every plant is gone. Many of the seeds that germinate on the beach are beans, varying in size from those of *Entada scandens* downward. They form one-third of the vegetable drift."

Indeed, the crabs are so numerous that Mr. Ross has failed in many attempts to raise plants of some of these things in his garden. One flourishing *Entada scandens* and a sickly *Calophyllum Inophyllum* were all the reward of much trouble in this direction. The huge square fruits of *Barringtonia speciosa* are often thrown up, and the seed germinates, but very few escape the crabs. This tree had not established itself in North Keeling, though in August 1888, Dr. Guppy observed two seedlings about eighteen inches high, and they owed their preservation, it was supposed, to the circumstance of the fruits having been concealed when the seeds germinated by the bed of fine drift pumice that had been deposited on the shores of the lagoon after the Krakatão eruption.

Particulars are given of the incipient germination and early destruction of *Carapa*, *Nipa*, *Cycas*, and other seeds. Of course, the clearing of the original vegetation and subsequent cultivation, and the incidental or intentional introduction of various birds and animals, and the migration of the myriads of sea-birds that formerly inhabited the islands must all be taken into consideration. Yet no species of plant ever known to grow wild there has become quite extinct, an evidence of their tenacity of life under unfavourable conditions.

Dr. Guppy's additions to the Keeling flora include the following plants, which he regards as having formed part of the original vegetation, judging from the conditions under which he found them: *Calophyllum Inophyllum*, *Thespesia populnea*, *Triumfetta subpalmata*, *Suriana maritima*, *Canavalia obtusifolia*, *Terminalia Catappa*, *Barringtonia speciosa*, *Sesuvium Portulacastrum*, *Ipomœa grandiflora*, *I. biloba* (*I. pes-capræ*), *Premna obtusifolia*, and *Hernandia peltata*. Their general distribution fully justifies this deduction.

The experiments on the vitality of seeds after forty to fifty days in sea-water were necessarily of a limited character, but they established the fact that the following germinated: *Cordia subcordata*, *Hernandia peltata*, *Guettarda speciosa*, *Thespesia populnea*, *Scævola Kœnigii*, *Morinda citrifolia*, and *Tournefortia argentea*. Every seed of the last named germinated after forty days', and half of the seeds of *Morinda* after fifty-three days' immersion. Dr. Guppy calculates that a surface current of only one knot an hour would convey drift a distance of 1000 to 1200 miles during these periods. From the fact that almost all the drift is thrown up on the eastern and southern coasts, it is assumed that the bulk of it comes from the Malay Archipelago, and perhaps some from the north-west coast of Australia. This is borne out by the general distribution of the established Keeling plants, as well as by the other seeds and fruits that are stranded there.

Among the latter may be mentioned *Pangium edule*, *Heritiera littoralis*, *Erythrina indica*, *Mucuna* spp., *Dioclea reflexa*, *Cæsalpinia Bonducella*, *Cerbera Odollam*, *Quercus* spp., and *Caryota*.

Carpophagous pigeons have played no recognizable part in the flora of the Keeling Islands.

In his forthcoming book Dr. Guppy will doubtless give all the details of his observations in a more connected and systematic form.

W. BOTTING HEMSLEY.

NOTES.

TO-DAY the honorary freedom and livery of the Turners Company are to be conferred on Sir John Fowler, K.C.M.G., and Sir Benjamin Baker, K.C.M.G., "in recognition of their distinction and eminence as engineers, earned by many great works at home and abroad, especially the design and construction of the Forth Bridge, one of the greatest triumphs of British engineering in the Victorian age."

SIR JOHN KIRK, F.R.S., AND SIR WILLIAM TURNER, F.R.S., Professor of Anatomy in the University of Edinburgh, have been elected members of the Athenæum Club, under the rule which provides for the annual election of a certain number of persons of distinguished eminence in science, literature, or the arts, or for public services.

MR. T. KIRKE ROSE, Associate of the Royal School of Mines, has obtained the appointment of Assistant Assayer at the Royal Mint, by competition among selected candidates. It is a post of some importance, and the salary rises from £350 to £450, with an official residence in the Mint. After an unusually brilliant career at the Royal School of Mines, Mr. Rose was engaged as metallurgist and assayer to the Colorado Gold and Silver Extraction Company in Denver. It is to be hoped that he will afford valuable assistance to Prof. Roberts-Austen in preserving the standard fineness of our coinage with the remarkable degree of accuracy that generations of assay masters have attained.

SIR HENRY ROSCOE has introduced into the House of Commons a Technical Education Bill, which is intended to clear up any doubt as to the legality of the provision of technical

and manual instruction in public elementary schools. The following are the provisions of the measure:—(1) The managers of any public elementary school may provide technical or manual instruction for the scholars in that school, either on the school premises or in any other place approved by the inspector, and attendance by the scholars of the school at such instruction shall be deemed to be attendance at the public elementary school. (2) The conditions on which Parliamentary grants shall be made in aid of technical or manual instruction in public elementary schools, shall be those contained in the Minutes of the Education Department and of the Science and Art Department in force for the time being. (3) The expression "technical instruction" and "manual instruction" shall have the same meaning as in the Technical Instruction Act (1889).

LAST week Dr. Farquharson asked the President of the Board of Trade whether he was aware that much dissatisfaction existed among scientific men as to the sufficiency of the tests used in the mercantile marine for the detection of colour-blindness, and whether he would appoint a committee of experts to advise the Government on this important question. In reply, Sir Michael Hicks-Beach said he was sensible of the importance of the matter, and had been in communication with the Royal Society upon the subject; and he was happy to state that "that valuable institution had appointed a committee to consider the whole question of colour-blindness."

THE meetings of the Institution of Naval Architects are now being held in the hall of the Society of Arts; the chair being occupied by Lord Ravensworth, the President of the Institution. The following is the programme of proceedings:—Wednesday, March 26, morning meeting, at 12 o'clock: (1) Annual Report of Council; (2) election of Officers and the Council; (3) alteration of rules relating to election of Vice-Presidents; (4) Address by the President; the following papers were then to be read and discussed—notes on recent naval manœuvres, by W. H. White, F.R.S., Director of Naval Construction, Vice-President; the Maritime Conference, by Rear-Admiral P. H. Colomb, R.N. Thursday, March 27, morning meeting, at 12 o'clock: on leak-stopping in steel ships, by Captain C. C. Penrose Fitzgerald, R.N.; strength of ships, with special reference to distribution of shearing stress over transverse section, by Prof. P. Jenkins; steatite as a pigment for anti-corrosive paints, by Frank C. Goodall. Evening meeting at 7 oclock: on the evaporative efficiency in boilers, by C. E. Stromeyer; on the application of a system of combined steam and hydraulic machinery to the loading, discharging, and steering of steam-ships, by A. Betts Brown; the revolving engine applied on board ship, by Arthur Rigg. Friday, March 28, morning meeting, at 12 oclock: on the variation of the stresses on vessels at sea due to wave-motion, by T. C. Read; spontaneous combustion in coal ships, by Prof. Vivian Lewes. Evening meeting, at 7 o'clock: on the screw propeller, by James Howden; experiments with life-boat models, by J. Corbett.

THE Geologists' Association have made arrangements for an Easter excursion to North Staffordshire. It will last from April 3 to 8, and the head-quarters will be the North Staffordshire Hotel, Stoke-on-Trent, except on Saturday and Sunday nights, when the Association will stay at the Red Lion, Leek.

A CONFERENCE of the Camera Club, under the presidency of Captain de W. Abney, was held last week at the Society of Arts. Lord Rayleigh gave an account of instantaneous photography by the light of the electric spark. He stated that he had been experimentalizing in taking photographs of minute jets of water as from a bottle. He exhibited on the sheet, by means of the electric light, photographs of jets of water taken in less than the 100,000th part of a second. In the course of the discussion following the demonstration and explanations by Lord Rayleigh, Mr. Trueman Wood spoke of the new application of electricity to the photographic art in fixing for study natural phenomena. The chairman, in giving the thanks of the meeting to Lord Rayleigh, referred to some photographs taken in less than the 100,000th part of a second under the name of a "photographic untruth." Captain Abney dealt with the untruth of form, which photography gave when judged by light and shade, a subject which could only be explained by series of drawings on the black-board and shadows cast upon the sheet.

THE Royal Microscopical Society has received from Dr. E. Abbe, of Jena, one of the new apochromatic $\frac{1}{10}$th microscope objectives recently produced at Zeiss's optical works, Jena, under Dr. Abbe's superintendence. The aperture is the highest hitherto attained, being 1·6 N.A., whereas the highest point previously reached by Dr. Zeiss was 1·4 N.A., so that the clear gain of aperture is 20 per cent. The advantage of this increase is shown by the perfection of the images obtained in photomicrographs produced by the new objective in the hands of Dr. Henri Van Hewick, Director of the Jardin Botanique, Antwerp, specimens of whose work were exhibited at the last meeting of the Royal Microscopical Society. At this meeting it was announced that Dr. Dallinger, F.R.S., had consented to join a committee appointed by the Council of the Royal Microscopical Society, to make a special report on the new objective.

AT the fortnightly meeting of the Royal Horticultural Society, on Tuesday, M. Henri de Vilmorin, President of the Botanical Society of France, delivered a lecture on salads, mentioning that in England we neither eat nor grow so many plants for salad as in France. He dwelt upon the nutritive value of salads due to the potash salts, which, though present in vegetables generally, are eliminated in the process of cooking. He then enumerated the various plants which are used in salads in France—namely, the leaves of lettuce, corn-salad, common chicory, barbe de capucin, curled and Batavian endives, dandelion in its several forms of green, blanched, and half-blanched, water-cresses, purslane in small quantities, blanched salsify-tops of a pleasant nutty flavour, witloof or Brussels chicory, the roots of celeriac, rampion, and radish, the bulbs of stachys, the stalks celery, the flowers of nasturtium and yucca, the fruit of capsicum and tomato, and, in the south of France, rocket, picridium, and Spanish onions. Various herbs are added to a French salad to flavour or garnish it, such as chervil, chives, shallot, and borage flowers. In addition, many boiled vegetables are dressed with vinegar and oil. M. de Vilmorin then showed specimens of dandelion, barbe de capucin, and witloof, both varieties of chicory, which he recommended to the notice of English gardeners as most useful and palatable. He mentioned that from a ton to a ton and a half of witloof is daily brought to the Paris market from Brussels, where it is grown in the greatest perfection. Specimens of English salads grown in the month of March, and consisting of corn-salad, lettuce, and blanched chicory, were sent from the Marquis of Salisbury's gardens at Hatfield. Among the other exhibits was a quaint orchid (*Cœlogyne pandurata*), a native of Borneo, sent from Kew Gardens. The flower is bright green, like the colour of forced lilac-leaves, with a dull jet-black blotch and lines on the lip.

AT the meeting of the Royal Botanic Society on Saturday, it was announced that the donations received included an interesting collection of seeds from the gardens of Mr. Thomas Hanbury, at Mortola, on the coast near Ventimiglia, Italy, with printed catalogues of the great variety of plants and trees from all climes growing in the garden—more than 4000 named species.

BARON DE LISSA, the pioneer planter of British North Borneo, arrived at Sandakan in January last. The official *Gazette* of British North Borneo says that the Royal Geographical

Society of Australia have forwarded to the Baron a draft for £100 towards the expenses of obtaining some information regarding the fauna and flora of Kina Balu and its neighbourhood. Baron de Lissa has placed himself in communication with the Governor on the subject, and is endeavouring to secure the services of a well-known geologist and naturalist who is residing at Sandakan.

THE following science lectures will be delivered at the Royal Victoria Hall:—April 1, an hour with the telescope, by J. D. McClure; April 15, the colours of a soap bubble, by John Cox.

IT is pleasant to turn over the pages of the handsome new edition of Darwin's famous "Voyage of a Naturalist" (Murray). The text is well printed, and no one can fail to enjoy the admirable illustrations contributed by Mr. R. T. Pritchett. In a prefatory note Mr. Murray explains that most of the views given in the work are from sketches made on the spot by Mr. Pritchett, with Mr. Darwin's book by his hand.

IN a few days the first part of a new work on the theory of determinants, by Dr. Muir, of Glasgow, will be published by Messrs. Macmillan and Co. It presents the subject in the historical order of its development, beginning with the brilliant but unfruitful conceptions of Leibnitz in 1693, and carrying the record forward to 1841, the year of the appearance of Cayley's first paper.

MR. H. A. MIERS, of the Natural History Museum, is engaged upon a text-book of mineralogy, which will be published by Messrs. Macmillan and Co.

LAST week (p. 478) we noted that at the meeting of the Royal Society of Edinburgh, on February 28, Dr. John Berry Haycraft had communicated the results of some recent investigations on voluntary muscular contraction. Dr. Haycraft's observations are interesting both to physiologists and to physicists. Where a muscle is stimulated by an electrical shock, all the fibres of the nerve receive the same stimulus, and all the fibres of the muscle to which the nerve passes contract together, and in the same way. This is not the case when a muscle contracts on receiving a natural nerve stimulation, starting either as a result of volition or of reflex action. The central nervous system seems unable to affect all the fibres of a muscle, through the numerous nerve fibres passing to it, in such a manner that they all shall contract exactly in the same way. The reason for supposing this to be the case is the fact, observed by the author, that fascicular movements are always present within a muscle during a voluntary or a reflex contraction, so that tracings taken from different parts of the same muscle invariably differ from each other. The experiments were conducted both upon the human masseter and the gastrocnemius muscle of the frog. These fascicular movements occurring within it will prevent any muscle from pulling with perfect steadiness on any lever or other registering apparatus, and the tracings taken by means of such apparatus will show oscillatory waves, often very rhythmical in their appearance. Many observers have concluded from an examination of these tracings that they indicate that the central nervous system discharges impulses into the muscle at a rate corresponding with that of the oscillations observed. Thus some observers find 20, others 10 oscillations per second in the muscle curve, and they consider that the nervous system discharges into the muscle at these rates. The author finds that the fascicular movements just described as occurring within the muscle itself account fully for the oscillations seen, the irregular aperiodic movements of the muscle compounding themselves with the period of oscillation proper to the registering apparatus itself, for by varying the instruments used, the resultant curves may be varied at will, slow oscillations appearing when using instruments of slow period, quick oscillations when using instruments of quick period. The author suggests that these fascicular movements probably account for the production of the muscle sound, which Helmholtz long ago pointed out was chiefly an ear-resonance sound. This, of course, could readily be evoked by any slow aperiodic movement, and the fascicular movements within the muscle must at any rate assist in producing it. These fascicular movements may, perhaps, account for the results obtained by Lovén, with the capillary electrometer, for it is more probable that he was registering the period of his own instrument than that the muscles were twitching at the slow rate of 8 times per second. If these conclusions are correct, there remains little to be said in support of the theory generally accepted that the nervous system normally discharges nerve impulses into the muscles like shots quickly fired from a revolver. It may be that this is the case, but the subject requires more extended investigation before any definite conclusions can be arrived at.

THE St. Petersburg Academy of Sciences has issued the Report for 1889, which was read at the annual meeting on January 12. The Report contains a valuable analysis of the scientific work done by the members during the year. In mathematics, Prof. Tchebysheff's applications of simple fractions to the investigation of the approximate value of the square root, and M. Ishmenetsky's work on the integration of symmetrical differential equations, are especially worthy of note. In astronomy, we notice O. A. Backlund's researches on the influence of temperature upon refraction. In physics, M. Khwolson made an attempt at a mathematical investigation of the extremely complicated laws of dispersion of light in milk-coloured glasses. The exploration of earth magnetism has made marked progress, both as regards the theory of diurnal variations and the measurement of magnetical elements in Caucasia and Siberia. Besides theoretical work in meteorology, the Central Physical Observatory has extended its system of weather-forecasts. Much interesting work has been accomplished in geology, Baron Toll having brought out the first volume of the geological part of the work of the expedition to the New Siberia Islands. In the botanical department the chief event was the publication of two parts of Prof. Maximowicz's description of the plants brought from Central Asia by Przewalsky, as well as the flora of Western China, as represented in the valuable collections brought by M. Potanin. Highly interesting work was done in zoology by Prof. Famintzyn.

WHEN the sun sets in the sea, a curious appearance, as of a bluish-green flame, is sometimes observed. This has been thought to be due to the light passing through the crests of waves. But Prof. Sohncke (*Met. Zeits.*) considers this view disproved by such an observation as that recently made by Prof. Lange at a watering-place on the Baltic. Shortly before sunset, the disk was divided in two by a thin strip of cloud; and just as the upper part disappeared under the cloud, the blue flame was observed. Thus the cause appears to be in the air, not in the sea. It is a case of atmospheric refraction. And as a planet, seen near the horizon with a good telescope, appears drawn out into a spectrum, with the more refracted blue-violet end higher than the red, so the last visible part of the sun furnishes the blue-violet end of a spectrum. But it would be interesting, Herr Sohncke remarks, to determine more precisely the conditions of this not very frequent phenomenon. Perhaps it requires merely great transparency of air, as only in this case would the last ray be able to give a spectrum sufficiently intense in its blue region.

THE Report of the Meteorological Council for the year ending March 31, 1889, has been published, and describes the work of the Office under three heads. (1) Ocean Meteorology. The

number of logs received from ships was 189; of these 80 per cent. were classed as "excellent," being a greater percentage of excellence than has been reported for some years. The discussion of the meteorology of the Red Sea is still in progress, and the work is well advanced. Charts of barometrical pressure for four representative months for the various oceans have been issued, together with charts showing the mean barometrical pressure for the year, and the extent of range of irregular fluctuations, and considerable progress has been made in the construction of the current charts for the various oceans. As these works are cleared off, it is intended to undertake a discussion of the meteorology of the region from the Cape of Good Hope to New Zealand. (2) Weather Telegraphy. The work of this branch continues to increase, and the Daily and Weekly Weather Reports, in particular, have been extended and improved. Forecasts continue to be prepared three times daily, and special forecasts were issued during the hay-making season; the highest percentage of success of the latter was in the southern part of England, and the lowest in the north-east district. Storm warnings are issued to those places on the coast that desire to receive them. (3) Land Meteorology of the British Isles. The records from the Observatories and Stations of the Second Order are discussed and published. The Council have continued the annual grant of £100 towards the expenses of the Ben Nevis Observatory, and have received copies of the observations made there. They have also agreed to allow £250 a year to the proposed Observatory at Fort William, for five years, and to supply an outfit of an Observatory of the First Order, to be equipped with self-recording instruments. The Report also contains interesting notes on some results of an examination of the Atlantic charts published by the Office, and on the measurement of squalls shown on the traces of Robinson's anemometers.

A NEW alkaloid, to which the name taxine is applied, has been extracted and isolated by Drs. Hilger and Brande, of Erlangen, from the leaves, seeds, and young shoots of the yew tree (*Taxus baccata*). Lucas some time ago pointed out the existence of a narcotic partaking of the nature of an alkaloid in the yew tree, and Marmé has since described a mode of extracting it. Drs. Hilger and Brande have lately prepared large quantities of this alkaloid, and have at length satisfactorily determined its composition and its more important chemical properties. The leaves and seeds were first repeatedly treated with ether in order to extract as much of the alkaloid as possible. The extract was then subjected to distillation to remove the ether, and the residue agitated with water acidified by a little sulphuric acid. The acid washings were noticed to be strongly coloured, and this was found to be due to the high tinctorial power of a compound of taxine with sulphuric acid. The acid solution was then rendered alkaline by ammonia, and the precipitated alkaloid dried over sulphuric acid. After dissolving in ether, re-washing with acid and precipitating with ammonia several times, the alkaloid was obtained as a perfectly white powder of extremely bitter taste, and melting at 82° C. On heating in a glass tube the melted taxine partly sublimes as a white cloud which condenses in the colder part of the tube in the form of drops of oil which solidify on cooling. At the same time it evolves a most characteristic odour. It is very difficultly soluble in water, chloroform, or benzene, but readily in alcohol and ether. Concentrated sulphuric acid produces an intense purple coloration. Dilute acid solutions give precipitates with gold chloride, platinum chloride, and picric acid, and also even in very dilute solutions yield precipitates on the addition of caustic alkalies or ammonia insoluble in excess. Analyses show that the formula of taxine is most probably $C_{37}H_{52}O_{10}N$. It forms with acids salts readily soluble in water. The hydrochloride, sulphate, acetate, oxalate, and tartrate, have been prepared, likewise the double salts with the chlorides of platinum and gold. The hydrochloride is best obtained by passing hydrochloric acid gas through a solution of taxine in anhydrous ether, when the salt is at once deposited in good crystals. Analysis indicates the formula $C_{37}H_{52}O_{10}N.HCl$. The sulphate possesses the composition $(C_{37}H_{52}O_{10}N)_2H_2SO_4$, the platinochloride $(C_{37}H_{52}O_{10}N.HCl)_2PtCl_4$, and the aurochloride $(C_{37}H_{52}O_{10}N.HCl)AuCl_3$. A compound of taxine with ethyl iodide, of the composition $C_{37}H_{52}O_{10}N.C_2H_5I$, was also obtained by heating equal molecules of the alkaloid and ethyl iodide to 100° C. under pressure. This compound is also a crystalline solid soluble in water. As regards the constitution of the alkaloid, which from its high molecular weight must of necessity be extremely complex, it has only yet been ascertained that it belongs to the class of nitrile bases. The leaves of the yew tree were found to contain the largest quantity of taxine, the seeds containing a smaller but still by no means inconsiderable quantity of the alkaloid.

THE additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus rhesus* ♀) from India, presented by Mr. McDowall Currie; a Ring-necked Parrakeet (*Palæornis torquatus* ♂) from India, presented by Miss Thornton Smith; two West African Love Birds (*Agapornis pullaria* ♂ ♀) from West Africa, presented by Mrs. Cyril Tatham; a Black-necked Stork (*Xenorhynchus australis*) from Malacca, two Peacock Pheasants (*Polyplectron chinquis* ♂ ♀) from Burmah, purchased.

OUR ASTRONOMICAL COLUMN.

OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at Greenwich at 10 p.*m*. on March 27 = 10h. 21m. 7s.

Name.	Mag.	Colour.	R A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G.C. 2102	—	Blue.	10 19 29	−18 5
(2) 37 Leonis	5·7	Yellowish-red.	10 10 47	+14 17
(3) γ Leonis...	2	Yellowish-white.	10 13 54	+20 24
(4) α Leonis...	1	White.	10 2 30	+12 30
(5) 136 Schj.	6	Very red.	10 46 17	−20 46
(6) X Boötis...	Var.	Dull orange.	14 18 59	+16 49

Remarks.

(1) This is a very bright planetary nebula in the constellation Hydra. From its size and equable light, Smyth compared it to Jupiter. It is about 32″ in diameter, and its spectrum consists of bright lines. In 1868, Dr. Huggins recorded the presence of the three characteristic nebula lines, but Lieutenant Herschel only saw two of them. The spectra of planetary nebulæ are by no means difficult to observe, notwithstanding their generally small diameters. If no cylindrical lens be employed, the lines in some cases are considerably bright, and their shortness is no great drawback. Now that we know that there are a good number of lines in the nebula of Orion, it seems reasonable to expect that a careful search will reveal a greater number in other nebulæ. D_3 and a line about λ 447 are the next in order of brightness to the three chief lines and G in the visible part of the spectrum of the nebula in Orion, and these should therefore be first looked for. It should also be particularly noted whether the brightest line is perfectly sharp on both edges, or otherwise.

(2) This star has a spectrum of the Group II. type. Dunér states that the spectrum is rather feebly developed, all the bands being narrow. The bands 2 and 3 in the red are the strongest. The character of the spectrum indicates that the temperature of the star is probably higher than that of most of the members of the group, the spectrum approaching that of Aldebaran. In that case, a considerable number of lines may be expected. It will be remembered that in Aldebaran there is mainly a line spectrum, together with the remnants of the bands in the red.

(3) A star of the solar type (Gothard). The usual observations are required.

(4) This is a star of Group IV., showing several fine metallic lines in addition to those of hydrogen. The usual observations are required.

(5) The spectrum of this star is a fine one of Group VI. The usual carbon bands are wide and dark, and the subsidiary bands 4 and 5 are perfectly well seen (Dunér). It seems probable that favourable conditions of observation, which, unfortunately, are not common for low stars in our latitude, may reveal other secondary bands.

(6) This is another variable star of which the spectrum has apparently not been recorded. The period as determined by Baxendell is 121·4 days, and the magnitudes at maximum and minimum are 9·2 and 10·2 respectively. The maximum will be reached about April 5. (This is Baxendell's V Boötis.)

A. FOWLER.

CHARLES MARIE VALENTIN MONTIGNY.—It is with regret that we have to announce the death of Prof. Montigny, at Schaerbeek, on the 16th inst. Prof. Montigny was born on January 8, 1819, and was a member of the Royal Academy of Belgium, Astronomical Correspondent of Brussels Observatory, an officer of the Order of Leopold, and decorated with the civil cross of the first class. He is best known for his interesting researches on the scintillation of stars, which form the subject-matter of a series of papers communicated to the Brussels Academy. In the January number of *Himmel und Erde* a long description is given of the results of Montigny's observations, and the instrument he devised and used for the determination of the amount of scintillation on different nights, and for the same stars at different altitudes. It is well known that if a scintillating star is observed by means of an opera-glass or small telescope, and the instrument tapped, the star appears to move and not the instrument; if the instrument is kept vibrating, the star will appear to move in a closed curve, along which different colours repeat themselves. The scintillometer devised by the late Prof. Montigny for investigating these appearances consisted of a small disk which could be whirled round in front of the eye-piece so that the star appeared to describe a circle in the field of the telescope. The circumference of this circle was made up of a regular sequence of colours, of which blue, yellow, and red were predominant. If the rate of motion of the disk be known, then by counting the number of times the colours were repeated the number of changes of colour a second may be found. All the causes affecting the scintillation of stars were investigated, and the relation of the amount to the character of the spectrum, the state of the atmosphere, and the colour of the star, made the subject of inquiry. The results obtained by means of this ingenious instrument are important, and the whole work on scintillation done by the deceased astronomer stands as a fitting monument to his memory.

AN OBSERVATORY AT MADAGASCAR.—A new Observatory has been established at Tananarivo under the direction of the Jesuit fathers, and with the concurrence of the French Government. The site chosen is a hill a short distance to the east of the town, and about 4400 feet above sea-level, making the Observatory one of the highest in the world. It already possesses an equatorial, a meridian instrument, and all necessary apparatus for meteorological observations; and a photographic telescope will shortly be provided for solar observations.

THE ADMINISTRATION OF FOREIGN FISHERIES.

THE following notes[1] were drawn up at the request of the late Lord Dalhousie just before he became seriously ill. The failure of his health and his absence from home—before the sad bereavement and shock which terminated in his death—prevented him perusing them, though the substance of much that appears in the subsequent pages formed the theme of several conversations with him. His familiarity with the sea, his wide knowledge of the fisheries, his upright and generous bearing, and his sound judgment, would undoubtedly, if he had been spared, have been of infinite service to the Department (which, probably, sooner or later, he would have reorganized very thoroughly). No greater loss, indeed, has happened to the fisheries in recent times.

[1] For information on various points relating to the subject, I have to thank Profs. Alex. Agassiz, Hubrecht, Möbius, Lovén, and G. O. Sars, Herr von Behr, Drs. Anton Dohrn, Lindeman, Nansen, and Sauvage; while Mr. Hoyle kindly aided me with the Norwegian statistics.

The United States Fish Commission is managed by a Director, who is more or less autocratic and irresponsible; though in the case of the late Prof. Baird the Americans were extremely fortunate in having a Director possessed of great administrative power and tact, and who never utilized the resources at his disposal for personal display or advancement. However able this Director may be, the system has its disadvantages, and is less suitable than a mixed Commission of men of position, who would have an opportunity of expressing their views as to the work to be carried out. Moreover, the American plan is less safe than a responsible head—that is, a chief under the control of a Board or Commission of those who are not necessarily specially skilled. It is possible, indeed, that, as the fisheries are at present administered in the United States, a considerable expenditure of money and of time annually takes place, which under other methods might be curtailed. The practical advances made by the Americans have in the main been confined to the fresh-water fisheries—that is, the propagation of the salmon-tribe, carp, and other fluviatile and lacustrine forms. The Marine Department has not yet succeeded in making any noteworthy improvement in sea-fisheries, though much money has been spent, and a large Annual Report is regularly issued. This Report contains not only the work accomplished by the staff of the Department, but reprints and translations of papers relating to the fisheries of other countries. There is, therefore, a wide difference between the condition in this country (where the observations connected with the fisheries have often to be published by Societies or independent journals) and the lavish expenditure on the other side of the Atlantic.

In France, again, the management of the fisheries is exclusively vested in the Minister of Marine at the Bureau des Pêches. At the head is a Director charged by the State with the inspection of the fisheries. For the scientific study of the questions pertaining to the marine fisheries the chief station is at Boulogne—though the Minister of Agriculture, under whom the station was constructed, also gave a small subsidy to the Zoological Laboratory at Villefranche (Alpes Maritimes) for the study of diverse questions concerning fishes and oysters—and this was founded by a subsidy from the town and the Chamber of Commerce. The advances made by M. Coste and others in the fresh-water fisheries of France are too well known to need further attention. France is fortunate in having a series of excellent marine laboratories, at which considerable advances have already been made in regard to the food-fishes, and in collateral scientific subjects. The names of MM. Lacaze Duthiers, Giard, Marion, Barrois, Pouchet, Sauvage, and others, are sufficient guarantees that the work of the fisheries and cognate subjects will be worthily carried out.

In Norway there is no special Fishery Board, but the Governmental Department of the Interior manages both the marine and fresh-water fisheries. As yet only a general inspector for the latter has been appointed at a fixed salary. For each of the more important marine fisheries, however, a so-called *opsynschef* is engaged by the Government, to see to the administration of justice during the time the fishery is going on. Moreover, an annual grant of 16,000 kr. is granted to the Society for the Advancement of Norwegian Fisheries in Bergen. The aims of this Society, which has various branches in towns along the coast, are chiefly practical, such as the improvement of fishing implements, the most suitable and successful preparation of the fishery products, and other features. It also has a special department for the artificial hatching of the food-fishes, in connection with the laboratory at Arendal, on the southern coast. The expenses of this establishment are partly borne by the Society just mentioned, and partly by private subscription. It is at this laboratory that M. Dannevig has done so much good work in the artificial rearing of cod, oysters, and lobsters, in the former case having succeeded in keeping the fishes till the end of the second year, and when of considerable size (14–16 inches).

For strictly scientific investigations in connection with the marine fisheries the Storthing grants an annual sum of 4800 kr. These investigations have for many years been chiefly carried out by Prof. G. O. Sars, whose observations on the Lofoten cod-fisheries, and the development of the cod, are well known and justly esteemed, while, as a worthy son of a distinguished father, he has in other departments of zoology contributed largely to our knowledge. Other naturalists have also been engaged in the work, chiefly in regard to the herring-fisheries. Prof. Sars, moreover, with a view of protecting the marine fisheries, has to report on every contrivance proposed,

and in regard to restriction in the use of certain fishing implements, besides giving his advice concerning the regulation of close seasons and similar subjects. He has to present to the Department his opinions on these matters before the proposals are brought in for the Storthing. In 1886 much discussion took place in the latter assembly concerning a more central management of the Fishery Department, and the establishment of a special office for a chief director for all the fisheries, together with a staff of subordinate inspectors. This arrangement is considered in Norway to be of considerable importance, but unfortunately no individual is known who unites in himself all the many qualifications for this important office. The following are the grants sanctioned for the financial year from July 1, 1886, to June 30, 1887, for the Fishery Institutions:—

(1) For practical scientific investigation regarding the sea fisheries, the last Parliament voted 4800 kr.[1]

It is proposed to increase this by 2400 kr., to be given to Hr. Lumholtz.

(2) As a contribution to the Society for the Encouragement of the Norwegian Fisheries, the last Parliament voted 16,000 kr., of which 4000 kr. were to be given to the affiliated Societies of Tromsö, Stift, and 2000 kr. to the Institution for Pisciculture in Arendal.

It is desired to increase this sum to 32,000 kr. for the coming year; the work of the Society depends upon this grant, because the fishermen cannot be expected to contribute much, and the needs of the Society are always increasing. The expenses for the coming year are estimated at 34,910 kr., of which 12,000 kr. will be needed for the regular expenses of the Society. It is proposed that the fisheries should be under a central direction with subordinate officials, and thus the Society would be relieved of a large part of its expenses.

The Department decided, however, that the grant should be retained at its original amount, 16,000 kr.

(3) For inspection and administration of the law at Lofoten cod-fishery, 31,950 kr. were voted.

(4) For increased police inspection of the mackerel-fishery at Uleholmene 200 kr. were voted.

(5) For increased police inspection of the spring cod-fishery in Namdal 1000 kr. were voted.

(6) For increased police inspection of the spring cod-fishery in Finmark 7200 kr. were voted.

(7) For increased police inspection of the spring cod-fishery in Söndmöre 3600 kr. were voted.

(8) For inspection and administration of the law at the herring-fishery 12,000 kr, were voted.

(9) For the encouragement of fresh-water fisheries 24,040 kr. were voted.

This sum it is desired to increase to 31,000 kr.

A. *Expenditure.*

			Kronas.
I. For practical scientific investigations into the sea fisheries, of which 2400 kr. form an honorarium for Hr. Lumholtz			7,200
II. Contribution to the Society for the Encouragement of Norwegian Fisheries			16,000
III. For inspection, &c., of cod-fisheries at Lofoten (1200 kr. only in the event of there being a congregation of fishermen at Raftsund) ...			31,950
IV. For increased police inspection at:—			
(1) Mackerel-fishery at Uleholmene	200		
(2) Spring cod-fishery at Namdal ...	1,000		
(3) ,, ,, Finmark ...	7,600		
(4) ,, ,, Söndmöre	3,200		
			12,000
V. For inspection, &c., at the herring-fishery in 1887			12,000
VI. For the encouragement of fresh-water fisheries:			
(1) To salary and office help for the inspector (400 kr. for personal expenses of present inspector)..	3,640		
(2) To two permanent assistants ...	3,400		
(3) To travelling expenses of the above officials in the fishing districts, and for travelling expenses of temporary assistants.	5,000		
(4) Inspection of salmon-fishery ...	7,600		
(5) For experimental transport of Wener salmon	200		
(6) For experimental marking of salmon and sea-trout	400		
(7) For encouragement of artificial spawning	1,000		
(8) Contribution:—			
a. For erection of salmon ladders at water-falls in accordance with plans given by the inspector in 1884 ... 1,667			
b. For erection of a salmon ladder at Haaelven in accordance, &c. 300			
	1,967		
			23,207
			102,357

[1] About 18 kronas = £1 sterling.

B. *Income.*

Salvage of nets and apparatus at Lofoten 600

In Sweden there is, strictly speaking, no Central Government Office for the fisheries. The fishery laws, and other special measures relating to the fisheries, are decreed by the Governors of the provinces or by the Department of the Interior. Previously, however, to the promulgation of any new law, the Governor must, pursuant to the Royal Ordinance of November 7, 1867, consult the Intendant of the Fisheries, who, conjointly with two assistants and one Instructor in Fish-breeding, are the public functionaries in connection with the fisheries in this country. Before the appointment of these officials, in 1864, there was (from the year 1855) a special Fishery Overseer (*Fiskeritillsyningsman*), or Inspector of the Sea Fisheries, in the province of Gothenburg and Bohus. He receives a salary from the Agricultural Society of that province, with subvention from the Crown, and is subordinate to the Governor of the province. The Intendant of the Fisheries and his assistants are under the control of the Royal Academy of Agriculture in Stockholm.[1]

The duties of the Intendant of the Fisheries are:—

(1) To investigate, with the aid of his assistants, the fisheries of the country.

(2) To propose or examine drafts of fishery laws or other measures for the improvement of the fisheries.

(3) To assist proprietors of fisheries with advice for hatching fishes, or with other measures for a rational management of the fisheries.

(4) To prepare and elaborate the fishery statistics.

(5) To control and direct the labours of the assistants and the fishery overseers.

Persons desiring the assistance of the fishery officials have to lodge intimation with the Royal Academy of Agriculture, and then the Intendant submits to the Academy a plan for the labours and the journeys of the fishery officials for the ensuing year. A fixed sum of 3500 kr. (about £198, or £83 for the Intendant and £55 for each assistant) is assigned for the travelling expenses of the fishery officials. Those requesting assistance have to pay 6*s.* per day.

The Intendant has to present annually a brief report on the labours of the fishery officials, and from time to time more detailed notices of the fisheries of the country. The Inspector of the Sea Fisheries of Gothenburg and Bohus submits an annual report on those fisheries to the Agricultural Society of the province.

The legal proceedings relating to the fisheries are briefly as follow:—If one or more proprietors of fisheries desire new or modified laws for the fisheries in their waters, or the Intendant of the Fisheries proposes such, the matter is submitted to the Governor of the province. The Governor then convokes all persons interested to meet and discuss the question. If the Governor, after having consulted the Intendant of the Fisheries, judges the proposals of the majority of the fishery proprietors suitable for the improvement of the fishery, those proposals are sanctioned, either as they stand, or with the necessary modifications. Anyone who dissents from the judgment may appeal to the Department of the Interior.

[1] The allowances of these officials from the Treasury are as follows:—Intendant, £250; two assistants, respectively, £111 and £83.

Germany, likewise, has no special central or chief authority for the management of the fisheries. The Empire has no right of control or even of cognizance of the fisheries. The State, however, gives annually a small sum to the German Fisheries Union (Fresh-water Fisheries). The control and management of the fisheries is therefore a matter for the different States which form the Empire. All these (Prussia included) have Inspectors of Fisheries (*Oberfischmeister*) and master-fishers (*Fischmeister*), but their duty only relates to the fiscal interests of the States and the rigorous observance of the fishery laws. They also give directions to the fishermen concerning the use of new and suitable fishing apparatus.

The control of the fresh-water fisheries of Prussia is vested in the Minister for Agriculture, Woods, and Forests, but there is no special Board for Fisheries. The various questions are worked up by clerks as they arise, as also is the preparation of Bills for the Prussian Chambers. In like manner the provincial control, the district (*Regierung*) control, and the Kreiss or county control, are carried out respectively by the Oberpräsident, the Regierungs Präsident, and the Landrath.

The Deutsche Fisherei Verein, of which Herr von Behr is chairman, is an independent association. It receives occasionally money grants from the Prussian Minister from a fund voted by the Prussian Chambers, and a regular grant, amounting at present to £1500 a year, from the German Parliament, towards the encouragement of fish-breeding throughout Germany.

Prussia for a series of years has had at Kiel a Commission for scientific researches in the German seas. It consists of four members, viz. a zoologist, a botanist, a physiologist, and a physicist. The present members are Professors in the University of Kiel, and Prof. Möbius (zoologist) is chairman. This Commission is placed under the control of the Ministry of Agriculture, and from that body it receives annually a sum of 9600 marks (£480) for general and personal expenses. The Commission publishes meteorological observations, statistics of the fisheries on the Baltic stations, and reports on scientific researches.

Much valuable work has been accomplished by this Commission in regard to the life-histories and development of fishes and the pelagic animals of the Baltic. Amongst other recent suggestions is one regulating the saleable size of certain fishes in special localities, *e.g.* the salmon and salmon-trout being fixed at 19½ and 11 inches respectively, the flounder at 6 inches, and the plaice at 7.

The Fishery Board of the Netherlands (Collegie voor de Zeevisscherijen) is composed of fifteen members, one of whom is president, and a secretary, who is not actually a member. All are nominated by the Crown, and the president out of a leet of two drawn up by the Board itself. The president and secretary form a kind of standing Committee by whom the every-day business is managed. All important affairs, however, have to come before the meetings of the Board, of which there are at least two yearly, viz. one in summer and one in winter. Very often the meetings are more numerous.

The majority of the members must be free from any direct interest in the fishing trade or the fisheries industries. The minority may, on the contrary, represent such interests. Actually the minority is composed (1) of a specialist for the herring-fishery—a great shareholder and head of a large fishing firm; (2) a member for the line-fishing; (3) one for the oyster industries; (4) one for the salmon and fresh-water fisheries; (5) one for the herring and cod fisheries; and (6) one for the fisheries of the Zuyder Zee.

Further, there are on the Board one shipowner and shipbuilder; one naval officer; several lawyers, several local authorities; and two zoologists.[1]

The members receive no salary—only their travelling expenses. Whenever a question is laid before the Board either by Government or at its own invitation, the President selects a special committee of three or five members to study, discuss it, and to draw up a report, which is then circulated, and afterwards, if necessary, discussed and voted about. All questions concerning fishery legislation are thus brought before the Board, and generally settled according to its advice.

There is a yearly grant (dating back, however, only a few years) of about £250 for experiments on the fishing industries, fish-culture, &c. Another £1000 are yearly devoted to salmon-culture, this sum being disbursed to the most successful fish-culturists at the rate of 5*d.* for a salmon a year and a half old (smolt), and two-fifths of a penny for one a few months old (parr). If the number of parr offered exceeds the sum which is available after the full value has been paid for the smolts, the culturists must either acquiesce in a reduction of price or keep their fishes. One or more members of the Board are always present when the fishes are set free into the rivers.

Since 1881 certain legal restrictions have been made with regard to the fisheries of the Zuyder Zee, and a staff of police organized on the inland sea, the chief officer being directly under the orders of the President of the Board. The same is the case with the police on part of the oyster territories. Those in Zealand have been, since the fresh start in 1870, under a special local Board.

In Italy the affairs relating to the fisheries are managed by the Minister of Agriculture, &c. The Minister nominates a Central Committee of twenty-four members. These consist of scientific men, magistrates, persons industrially interested in the fisheries, and some members of the Legislature (M.P.'s). Twelve members are elected or reappointed every year. The meetings of this Committee do not take place at certain fixed periods, but only by invitation of the Minister, who submits to the Committee the material to be discussed.

Besides the Central Committee there are a series of local Committees throughout the kingdom. These consist of the Captain of the Port, a zoologist, and technically experienced men. Their term of office lasts for three years from the date of appointment. The Regulation is published in the *Annali dell' Industria*, 1882, by the Ministry of Agriculture, Direzione dell' Industria e Commercio.

The duties of these local Committees are as follow:—

(1) To study and to propose all new regulations rendered necessary by experience.

(2) To collect the material for annual statistics.

(3) To give, on the demand of the Government, the Provinces, and the Communes, their opinion on matters directly or indirectly connected with the fisheries.

(4) To further the diffusion of the best methods of fishing and the advancement of the industries connected with them.

(5) To "render popular" the knowledge regarding the production, food, and diffusion of fishes and other useful marine animals.

From a consideration of the foregoing remarks on the Commissions, Boards, or Departments of foreign countries, it would appear that a central authority composed of a single individual, as in America, has certain disadvantages which can only be overcome by a rare combination of scientific eminence, administrative skill, and unbiassed judgment. It has, moreover, been a costly experiment; and it cannot be said that the Americans—even in the case of the cod—have succeeded so well as Dannevig at Arendal, in Norway, with the moderate resources at his disposal. It cannot be questioned, however, that the liberality of the Government of the United States has greatly aided scientific inquiry into marine life in general. Moreover, their efforts to increase the fresh-water fishes are most praiseworthy, and indeed in this they give us a good example, for there are still many fresh-water streams and lochs that would be of great value to the country if scientific fish-culture were put on a proper footing. The instance of the Outer Hebrides, *e.g.* North Uist, is sufficient in our own country. From the top of the Lee Hills the eye rests on a multitude of lochs—fresh-water and salt—which seem to be almost as extensive in superficial area as the shreds of land between them. In many of these, trout, salmon-trout, and salmon are found, so that one familiar with the agricultural poverty of these regions would not hesitate to place the culture of the water far before that of the land in regard to remuneration. A well-organized system of pisciculture in connection with these lochs would effect a revolution in the financial affairs of the people, and greatly supplement the food-supply for the community.

The French system does not seem to offer any suggestion of note in regard to the administration of the marine fisheries. The early labours of M. Coste and others in the culture of trout and salmon have, however, been of great service both to the adjoining Continental States, to us, and to America. It must not be forgotten also that M. Coste was one of those who took much interest in the Stormontfield experimental station on the Tay.

[1] This account does not quite correspond with the view published by the Fishery Board in their Sixth Annual Report, Part III., p. 305, for it is there stated that in Holland "There is a State Commission for Sea Fisheries, chiefly composed of naturalists and scientific men."

and personally, along with Mr. R. Buist, aided its establishment under the Committee of Proprietors.

Much that is useful for the purposes of administration may be learned from Norway, especially in connection with the Society for the Advancement of Norwegian Fisheries in Bergen, a place so classic to marine zoologists, from the days of Michael Sars to those of Fridtjof Nansen. Nowhere in Scotland can we point to a series of open-air reservoirs of pure sea-water, such as at Arendal, in which larval fishes can be raised to post-larval and subsequent stages; though at Stonehaven an enclosure of this kind formerly existed, and was used about thirty years ago in experimenting with young salmon (smolts). Yet no place is better fitted—both scientifically and economically—for such an arrangement than St. Andrews, as has indeed been often pointed out. The Norwegians are also fortunate in having the services of an able and original naturalist—trained from boyhood in marine zoology, besides others of European reputation. Sweden, though rich in names well known wherever zoology is studied, *e.g.* Lovén, places the direction of the fisheries under the Academy of Agriculture, the Governors of the provinces, and the Intendant; while the Inspector of the Sea-fisheries of Gothenburg and Bohus submits a special report to the Academy. The arrangements seem to work fairly, but it is doubtful if any feature of the system would be of advantage to this country.

No central authority for the whole of Germany yet exists, each of the States having Inspectors of Fisheries. Prussia, however, has the Special Commission at Kiel, the scientific work of this body being very much in its own hands. It has done good service in regard to the scientific aspects of the marine fisheries. The encouragement held out by the Deutsche Fisherei Verein to fresh-water fisheries is noteworthy and commendable.

One of the most satisfactory arrangements is seen in the Fishery Board of the Netherlands, in the composition of which all interests have been consulted. Moreover, the recent appointment of a scientific Superintendent of the Fisheries (viz. Dr. P. Hoek, an able zoologist) is important. The names of Hubrecht and Hoffman, who represent scientific zoology on the Board, are a sufficient guarantee that both tact and talent are at the service of the State. The solid scientific work done in the department by Profs. Hubrecht and Hoffman would alone give the Dutch Board a reputation, and when we add the names of other workers who have aided it, the position is considerably enhanced. Further, the mode by which scientific questions are referred to special committees—say of zoologists or physicists—and their reports thereon dispassionately discussed at meetings of the whole Board, obviates the possibility of the mistakes caused by a committee having perhaps only a single head to direct it in a particular inquiry.

The Italian system is satisfactory so far as the composition of the Board goes, though it seems to be a large one for efficiency, and the somewhat irregular nature of the meetings would hardly suit the methodical system generally followed in this country. The short period of office (three years), is perhaps not of much moment if re-election of the right men takes place. The fine Zoological Station at Naples under Dr. Dohrn (who, however, is too closely occupied to serve on the Central Committee of the Fisheries), gives the Italian Government a source of independent and reliable information, and of a different kind from that derived from the servants of a Board. The establishment of hatching stations, and the series of local committees throughout the country are features worthy of note, especially if due care be taken in the composition of the latter, so as to avoid the entrance of those who trade, it may be, on the credulity or ignorance of the fishing population.

W. C. McIntosh.

SCIENTIFIC SERIALS.

L'Anthropologie, paraissant tous les deux mois, tome i. No. 1, 1890 (Paris).—The first number of the new French review of anthropology, formed by the amalgamation of the older *Revue d'Anthropologie* and the *Revue d'Ethnographie*, begins with an article by Dr. Topinard, one of its joint editors, on the skull of Charlotte Corday, which ranked among the most interesting of the curious contents of the anthropological section of the Paris Exhibition, to which it was presented by Prince Roland Bonaparte. The author explains that, in making choice of this special skull, his object is not to compare its craniological characteristics with the moral disposition historically attributed to the individual to whom it had belonged, but simply to make it the text for an exposition, which might serve our own and future students as a lesson for the examination and description of an isolated skull after the precise methods taught by Broca, and having regard to the present condition of our science. In accordance with this object, Dr. Topinard, confining himself almost entirely to craniometrical determinations, of which he gives a most comprehensive series, together with several well-drawn illustrations, only occasionally enters into the comparative relations presented by this cranium to other isolated crania. From this exhaustive lesson in craniometry it would appear that the skull of Charlotte Corday closely accords with the typical form of the female skull, established by Broca as characteristic of Parisian women, deviating only from the normally perfect feminine cranial type in presenting a certain flatness of the frontal region, and some traces of jugular apophysis.—The Bronze Age in Egypt, by M. Montélius. The author, in opposition to the opinions of Lepsius and Maspéro, believes that the use of iron was not known in the valley of the Nile as early as bronze, which was probably fabricated 6000 B.C., and that the use of the former metal was not sufficiently common to justify us in speaking of an Iron Age in Egypt before 2000 B.C. He, moreover, believes that we must consider the era of Egyptian civilization as belonging mainly to the Bronze Age.—A short notice of the works of Alexander Brunias, by Dr. E. T. Hamy.—On the rock-sepulchre of Vaphio, in the Morea, by M. S. Reinach. The exploration of this tumulus was undertaken last year at the cost of the Archæological Society of Athens under the direction of M. Tsountas, and although the contents have not yet been fully examined, there can be no doubt of their extreme importance to archæology, as it has been proved beyond question that this rock-sepulchre had remained intact till the present time. It appears from the report of M. Tsountas that the poniards and other implements, together with many of the numerous funereal objects brought to light by the explorations at Vaphio, are similar to the remains obtained at Mycenæ. Among these finds special interest attaches to two golden goblets carved in strong relief, representing both clothed, and almost nude, figures, engaged in the hunting and taming of wild bulls. M. Reinach proposes in a future number of this journal to discuss the Vaphio tumulus more fully, but in the meanwhile he appeals to English archæologists to test the accuracy of a statement published in 1813 by the German traveller Baron von Stackelberg, that the so-called Treasury of Atreus at Mycenæ had a few years earlier been ransacked by Veli Pasha, who was said to have disposed of part of its treasures to Lord North. Dr. Schliemann questions the truth of this report, but M. Reinach is of opinion that it bears evidence of authenticity, deserving the notice of Englishmen, and he hopes, in the interests of archæological science, that some of these precious objects may yet be found in one or other of the great English collections.—We may remark, in conclusion, that the present review surpasses its predecessors in the excellence of its printing and its illustrations, while it has the great advantage of being edited by MM. Cartailhac, Hamy, and Topinard. In the space allotted to the consideration of the scientific literature of various countries, to which more than half the entire volume is devoted, there are various notices of Russian, Hungarian, and other works, not generally accessible to the ordinary reader; but we trust that in future numbers the reports of English works and memoirs will not, as in the present number, be drawn exclusively from the Quarterly Journal of the Royal Geographical Society of London.

American Journal of Science, March 1890.—Sedgwick and Murchison: Cambrian and Silurian, by Prof. James D. Dana. The relations of these two geologists to one another, and to Cambrian and Silurian geology is given. The full paper appeared in NATURE of March 6 (p. 421).—Notes on the Cretaceous of the British Columbian regions; the Nanaimo group, by George M. Dawson.—Celestite from Mineral County, West Virginia, by George H. Williams. A large number of celestite crystals, from an extensive railroad cutting into a bluff of lower Helderberg limestone, has been investigated.—A method for the determination of iodine in haloid salts, by F. A. Gooch and P. E. Browning.—On the mineral locality at Branchville, Connecticut, fifth paper, by George J. Brush and Edward S. Dana; with analyses of several manganesian phosphates, by Horace L. Wells. A new member of the triphylite group—a sodium-manganese phosphate, which has been called natrophilite—has been found, and the rare mineral hureaulite identified in the Branchville minerals.—A simple interference experiment, by Albert A. Michelson. Two pieces of plane glass, silvered on

the front surfaces, are fixed against a block of wood, so that the angle between the two surfaces is slightly less than 90°. This simple apparatus will give the interference phenomena produced by means of Fresnel's mirror or bi-prism.—An improved wave apparatus, by John T. Stoddard. This is a method of demonstrating to a class the formation of the compound curves representing the combination of two simple sound waves.—On a recent rock-flexure, by Frank Cramer.—On the origin of the rock-pressure of the natural gas of the Trenton limestone of Ohio and Indiana, by Edward Orton. By the rock-pressure of gas is meant the pressure in a well which is locked in so that no gas can escape; and the author concludes that the rock-pressure of the gas of the Trenton limestone is due to the pressure of a water column under which it is held in the arches of the rocks. This explanation seems applicable to all gas fields.

THE *American Meteorological Journal* for January contains a continuation of Faye's theory of storms, and of Ferrel's convectional theory of tornadoes, both of which have been already referred to; the latter paper is concluded in the number for February. Of the other articles in these two months the principal are:—The mathematical elements in the estimation of the Signal Service Reports, by W. S. Nichols. He points out that attempts to measure the accuracy of the daily weather forecasts are liable to give rise to a confusion of ideas, and, confining his attention to rainfall, he lays down certain rules for testing the value of the predictions to the community when judged from the stand-points of quantity and quality, as well as the accuracy of the information.—On the use of the "sling" thermometer in the prediction of frosts, by Prof. H. A. Hazen. With the view of protecting delicate plants from destruction by frost, the author advocates the determination of the dew-point in the evening, and if it is found to be as low as 25°, and the air-temperature at 45° or lower, with a clear sky, frost may be expected, and the plants should be protected by smoke from burning straw, before the early morning.—On globular lightning, by Dr. T. C. Mendenhall. The author quotes many interesting instances of this rare phenomenon, the earliest case recorded being at Stralsund in June 1670; and he describes several instances in which it has been observed at sea. Photographs of the phenomenon are much wanted.—Diminution of temperature with height, by Prof. H. A. Hazen. He has recently spent several weeks on the summit of Mount Washington (6300 feet above sea-level), and finds that the diurnal range of temperature, which is very small, is not due to the heating of the air by the sun, but only to the convection currents caused by the warm rocks. The object of the paper is to endeavour to throw light on the true explanation of storm phenomena.—An interesting summary, by A. L. Rotch, of the Meteorological Conference held at Paris in September last, in connection with the International Exhibition. This is the first general account which has appeared in English.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, March 6.—"On the Development of the Ciliary or Motor Oculi Ganglion." By J. C. Ewart, M.D. Communicated by Prof. M. Foster, Sec. R.S.

The most conflicting views have for some time been held as to the origin, relations, and homology of the ciliary (motor oculi, ophthalmic, or lenticular) ganglion. By Remak, Schwalbe, Marshall, and others, the ganglion of the ophthalmicus profundus has been described as the ciliary ganglion, and this ganglion has frequently been regarded as the ganglion of the motor oculi nerve, and hence as homologous with the Gasserian and other cranial ganglia. The ciliary ganglion having been shown by van Wijhe to be quite distinct from the ganglion of the ophthalmicus profundus, the old view of Arnold has been recently revived, and already van Wijhe, Hoffmann, Onodi, Dohrn, and Beard have indicated that they regard the ciliary as a sympathetic ganglion. Hoffmann bases his belief on certain observations on the development of the ciliary ganglion in reptiles, while Onodi has adopted this view chiefly because in the higher vertebrates the ciliary ganglion receives a communicating branch from the sympathetic. But Beard, while considering the ciliary a sympathetic ganglion, states that in sharks he has seen nothing in support of "the mode of origin for the ciliary ganglion described by Hoffmann," in reptiles.

In studying the ciliary ganglion in Elasmobranchs I have been specially struck with its tendency to vary not only in the same genus or species, but in the same individual. Of the numerous specimens examined, I have only once found the ganglion entirely absent (in an adult *Raia radiata*), while I have occasionally (in *Acanthias*) found two well-developed ganglia on each side. Usually in sharks I found the ganglion lying in connection with the inferior branch of the motor oculi, while in skates it was generally in contact with the ophthalmicus profundus, or lying midway between the motor oculi and the ganglion of the profundus. In form the ganglion varies extremely, rounded or conical in some cases, in others it was represented by two or three groups of cells lying parallel to or in contact with the motor oculi.

In some cases ganglionic cells had wandered from the ganglion a considerable distance along the ciliary nerves towards the eyeball.

Although in sharks the ciliary ganglion often lay in close contact with the motor oculi nerve, no ganglionic cells were ever found either in the trunk of that nerve or on any of its branches. In skates the ganglion was usually more intimately related with the ophthalmicus profundus than the oculo-motor. In all cases the ciliary ganglion had at least two roots, one from the motor oculi, and one or two from the ophthalmicus profundus. In skates the profundus root always proceeded directly from the profundus ganglion, and the profundus ganglion was frequently found to be connected by a communicating branch with the Gasserian ganglion.

Both in sharks and skates, in addition to the ciliary nerves from the ciliary ganglion there were ciliary nerves proceeding from the ganglion and from the trunk of the profundus, and in some cases large ganglionic cells had wandered from the profundus ganglion along the ciliary nerves; occasionally a few large cells had migrated some distance along the main trunk of the profundus. In all cases the majority of the cells of the ciliary ganglion were only about half the size of the cells of the profundus ganglion.

In skate embryos under two inches in length no indication of the ciliary ganglion was discovered, and in shark embryos about ten inches in length the ganglion was frequently represented by small groups of cells in the vicinity of the inferior branch of the oculo-motor nerve. In sharks the first steps in the development of the ganglion were not observed, but in skates it was possible to make out all the stages. The first indication of the ganglion was in the form of a slender outgrowth from the inferior border of the large ophthalmicus profundus ganglion, which met and blended with fibres from the descending branch of the motor oculi. The *outgrowth from the profundus ganglion* was crowded with cells; the *fibres from the motor oculi*, like its root and trunk, were absolutely destitute of cells. At a somewhat later stage the cells had accumulated at the junction of the outgrowth from the profundus ganglion with the fibres from the motor oculi. It looked as if the blending of the two sets of fibres had formed a network which resisted the further migration of the ganglionic cells. In typical cases, at a still later stage, all the ganglionic cells had left the outgrowth from the profundus ganglion to form a rounded mass from which the ciliary nerves took their origin. In some cases some of the fibres which connected the profundus ganglion with the Gasserian seemed to reach and end in the ciliary ganglion. It thus appears that the ciliary ganglion stands in the same relation to one of the cranial nerves (the ophthalmicus profundus) as the sympathetic ganglia of the trunk stand to the spinal nerves, and that the ciliary ganglion may henceforth be considered a sympathetic ganglion. Further investigations may show that the ganglia in connection with the branches of the trigeminus (fifth) nerve may also be considered as belonging to the sympathetic system. In conclusion, I may say that I have found the vestiges of the ophthalmicus profundus ganglion in a five-months human embryo lying under cover of the inner portion of the Gasserian ganglion, and satisfied myself that the ophthalmicus profundus of the Elasmobranch is represented in man, as suggested by several writers, by the so-called nasal branch of the ophthalmic division of the fifth. To as far as possible clear up the confusion that has arisen from mistaking the ophthalmicus profundus nerve for a branch of the oculo-motor or of the trigeminus nerve, and the ganglion of the ophthalmicus profundus for the ciliary ganglion, it might be well in future to speak of the profundus as the *oculo-nasal* nerve and its ganglion as the *oculo-nasal* ganglion.

Chemical Society, February 20.—Dr. W. J. Russell, F.R.S., in the chair.—The following papers were read:—The behaviour of the more stable oxides at high temperatures, by Dr. G. H. Bailey and Mr. W. B. Hopkins. Previous experimenters have found that cuprous oxide is obtained when cupric oxide is heated to redness. The authors find that at higher temperatures a further quantity of oxygen is given off, and an oxide having the composition Cu_3O is formed. This is insoluble in mineral acids and even in aqua-regia, but can be converted into a soluble form on fusion with caustic potash, from which it separates on treatment with water. The oxides of lead and tin seem to behave similarly at high temperatures.—The influence of different oxides on the decomposition of potassium chlorate, by Messrs. G. J. Fowler and J. Grant. The authors have systematically examined the influence of the chief metallic oxides and certain unstable salts on the decomposition of potassium chlorate by heat, and the chief results obtained may be summarized as follows:—(1) Acid oxides, such as V_2O_5, WO_3, and V_3O_8, cause the evolution of oxygen at a much reduced temperature with the formation of a metavanadate, tungstate, or uranate. Chlorine is evolved in large quantity in these cases, but the whole of the oxygen of the chlorate is not liberated, since the compound of K_2O with the oxide is not decomposed by heat or by chlorine—

$$4KClO_3 + 2V_2O_5 = 2K_2O, V_2O_5 + 2Cl_2 + 5O_2.$$

(2) Alumina acts similarly but less energetically. (3) Chromium sesquioxide causes the evolution of oxygen at a lower temperature, chlorine also being liberated—

$$8KClO_3 + 2Cr_2O_3 = 4KCrO_4 + 4Cl_2 + 7O_2.$$

(4) The sesquioxides of iron, cobalt and nickel, cupric oxide, and manganese dioxide cause the evolution of oxygen at a comparatively low temperature accompanied by only a small percentage of chlorine; the oxide is left but little altered at the end of the experiment. The authors find that their results are in harmony with the theory of the action of manganese dioxide advanced by McLeod (Chem. Soc. Trans., 1889, 184). (5) The monoxides of barium, calcium, and lead cause no evolution of oxygen when heated with potassium chlorate, but the latter breaks up below its normal temperature with the formation of potassium chloride and a peroxide. (6) In the presence of such oxides as silver oxide and the peroxides of barium and lead, potassium chlorate acts as a reducing agent. No oxygen is liberated, but a perchlorate is formed. (7) Oxides such as those of zinc and magnesium are completely inactive. The authors find that the physical condition of the oxide is of importance, thus copper oxide prepared in the dry way is almost inactive; and further, that certain substances, as powdered glass, sand, and kaolin, assist the decomposition, although apparently they undergo no chemical change.—The interaction of hypochlorites and ammonium salts; ammonium hypochlorite, by Messrs. C. F. Cross and E. J. Bevan. The authors bring forward evidence of the formation and existence of ammonium hypochlorite in solution, but have failed to isolate the compound when produced by the action of an ammonium salt on a dilute solution of bleaching powder, or by the electrolysis of ammonium chloride solutions. It exhibits curious anomalies in oxidizing properties in comparison with other hypochlorites. It is without action on many colouring matters—for example, those of the vegetable fibre; it does not decolorize a solution of indigo in sulphuric acid, although it at once liberates iodine from potassium iodide, and it does not peroxidize hydrated lead oxide. On the other hand, it oxidizes sulphites and arsenites, and its effect on aniline salts is identical with that of ordinary hypochlorites. In the discussion which followed the reading of the paper, Prof. Armstrong suggested that probably the authors were dealing with a chlorinated derivative of ammonia, *e.g.* NH_2Cl; such compounds, according to Gattermann's experiments, being more stable than is usually supposed.—The action of phosphoric anhydride on stearic acid, by Dr. F. S. Kipping. One of the products of the reaction is stearone, $(C_{17}H_{35})_2CO$, and the yield appears to be as good or better than that obtained when salts of stearic acid are submitted to dry distillation.—Semithiocarbazides, by Prof. A. E. Dixon.—Note on the production of ozone by flames, by Mr. J. T. Cundall. Ilosva (*Ber. der deut. chem. Gesellsch.*, Referate 1889, 791) states that when all the products of combustion of various kinds of flames are collected, they do not exhibit the smell or taste of ozone. This is confirmed by the results of some unpublished experiments made by the author in 1886, but recently he has found that the air aspirated through a tube, 3 mm. in bore, whose mouth is fixed about 5 mm. above the tube, and 5 mm. away from the flame of a Bunsen burner, both tastes and smells strongly of ozone. Similar results were obtained both with luminous and hydrogen flames. It was not found possible to confirm this fact by any other test for ozone, owing to the impossibility of finding any sufficiently sensitive reaction which was not common to dilute nitrogen oxides. The author agrees with Ilosva that the smell and taste of ozone are the only trustworthy tests for it when it is present in small quantities, and that Houzeau's papers (impregnated with red litmus and potassium iodide), which at first sight should give the necessary distinction, since an acid gas would not be expected to give an alkaline product, are useless, inasmuch as nitrogen oxides also turn them blue.

Geological Society, February 26.—Mr. J. W. Hulke, F.R.S., Vice-President, in the chair.—The following communication was read:—On the relation of the Westleton Beds or "Pebbly Sands" of Suffolk to those of Norfolk, and on their extension inland, with some observations on the period of the final elevation and denudation of the Weald and of the Thames Valley; Part 3, on a Southern Drift in the valley of the Thames, with observations on the final elevation and initial sub-aërial denudation of the Weald, and on the genesis of the Thames, by Prof. Joseph Prestwich, F.R.S. In this third part of his paper the author gave a description of the characters of the Southern Drift, showing how it differs from the Westleton Beds in the nature of its included pebbles, which consist of flints from the Chalk with a large proportion of *chert* and *ragstone* from the Lower Greensand, while there is a total absence of the Triassic pebbles and Jurassic *débris* characterizing the Northern Drift. He traced the drift through Kent, Surrey, Berkshire, and Hampshire, and described its mode of occurrence. Another pre-glacial gravel was then discussed under the title of the Brentwood group, and its age was admitted to be doubtful. The author then entered into an inquiry as to the early physiographical conditions of the Wealden area, and gave reasons for supposing that a hill-range of some importance was formed in the Pliocene period after the deposition of the Diestian beds. From the denudation of this ridge, he supposes that the material was furnished for the formation of the Southern Drift, which may have been deposited partly as detrital fans at the northern base of the range. The relation of the Southern Drift to the Westleton Shingle and other pre-glacial gravels was considered, and the Westleton Beds were referred to a period subsequent to that of the formation of the Southern Drift. The influence of the meeting of the earlier Wealden axis with that of the folding which produced the escarpments of central England was discussed, and it was suggested that the result would be the genesis of the Thames valley and river. The following summary gives the results of the author's inquiry as developed in the other parts of the paper. He holds:—(1) That the Westleton Shingle ranges from Suffolk to Oxfordshire and Berkshire, rising gradually from sea-level to 600 feet. (2) That the lower Tertiary strata were co-extensive with this shingle. (3) That the upraising of the Westleton sea-floor, with its shingle, preceded the advance of the Glacial deposits, and that the latter become discordant to the former when traced westward, occupying valleys formed after the rise of the Westleton Beds. (4) That the Tertiary strata and Westleton Beds on the north border of the Chalk basin were continuous until the insetting of the Glacial period, when they were broken through by denuding agencies. (5) That none of the present valleys on the north of the Thames Tertiary basin date back beyond the Pre-glacial period. (6) That the same date may be assigned to the Chalk and probably to the Oolite escarpments. (7) That in the Thames basin, besides the Northern Drift, there is a Southern Drift derived from the Lower Greensand of the Wealden area, and from the Chalk and Tertiary strata formerly extending partly over it. (8) That during the Diestian period the Weald was probably partly or wholly submerged, and that between this and the insetting of the Glacial period, the Wealden area and the Boulonnais underwent upheaval resulting in the formation of an anticlinal range from 2000 to 3000 feet high. (9) That from the slopes of this range the materials of the Southern Drift were derived, and spread over what is now the south side of the Thames basin. (10) That this denudation commenced at the time of the Red Crag, and went on uninterruptedly through successive geological stages. (11) That consequently, though the Southern Drift preceded the Westleton Shingle, the two must at one time have proceeded synchronously. (12) That the valley-system of the Wealden area dates from Pliocene times—

the initial direction of the transverse valleys from pre-Glacial times—and of the longitudinal valleys from Glacial times. (13) That the Thames basin results from the elevation of the Weald and the flexures of the Chalk and Oolites of the Midland counties, and dates from a period subsequent to the Westleton Beds. (14) That the genesis of the Lower Thames similarly dates from early Pleistocene times, whilst its connection with its upper tributaries and the Isis, which possibly flowed previously northeastward, took place at a rather later period. After the reading of the paper there was a discussion, in which the Chairman, Mr. Whitaker, Dr. Irving, Mr. Topley, Dr. Evans, and the author, took part. Dr. Evans congratulated the Society and Prof. Prestwich on his having been able to sum up the results of the observations of so many years in the series of papers which he had lately read.

Entomological Society, March 5.—Captain Henry J. Elwes, Vice-President, in the chair.—Mr. C. G. Barrett exhibited a number of specimens of *Dianthecia carpophaga*, Bork., bred by Mr. W. F. H. Blandford from larvæ collected near Tenby on flowers of *Silene maritima*. He remarked that the series included a number of forms intermediate between *D. carpophaga* and *D. capsophila*, and establish the fact that the latter is only a local variety of the former. Mr. W. H. B. Fletcher, Mr. Blandford, and Mr. McLachlan took part in a discussion as to the identity of the supposed species.—Mr. Barrett further exhibited a specimen of *Dianthecia luteago*, var. *Barrettii*, Db., also bred by Mr. Blandford from a larva found at Tenby, and he remarked that the species had not previously been taken in England; also a long series of forms intermediate between *Catoptria scopoliana*, Hw., and its small variety *parvulana*, Wilk., collected by Mr. E. Bankes, Mr. Fletcher and Mr. Vine, in Sussex, the Isle of Wight, and Pembrokeshire; also a specimen of *Botys mutualis*, Zell.,—a species widely distributed in Asia and Africa,—taken by Mr. C. S. Gregson near Bolton, Lancashire.—Mr. H. Goss exhibited several abnormal specimens of *Arctia caja*, bred last December. The object of the exhibition was to show the effect produced by forcing the larvæ, and subjecting them to unusual conditions. It was stated that the peculiarity of the colour of the hind wings of the female parent had not been transmitted to any of the offspring.—Mr. Blandford referred to two specimens of a species of *Cardiophorus*, from Tenby, which he had exhibited at the August meeting of the Society as *Cardiophorus cinereus*, and stated that subsequent investigation had led him to hand them to Mr. Champion for determination. Mr. Champion was of opinion that they did not belong to the same species; that one of them was *C. asellus*, Er., and the other, probably, *C. equiseti*, Hbst., a species new to this country.—Mr. C. J. Gahan read a paper entitled "New Longicornia from Africa and Madagascar."—Captain Elwes read a paper entitled "On a new species of *Thymara* and other species allied to *Himantopterus fuscinervis*, Wesmael."—Dr. Sharp read a paper entitled "On some Water Beetles from Ceylon."—Mr. J. J. Walker communicated a paper entitled "Notes on Lepidoptera from the Region of the Straits of Gibraltar." Mr. F. Merrifield, Mr. B. G. Nevinson, Captain Elwes, and Mr. G. Lewis took part in the discussion which ensued.—It was announced that papers had also been received from Mr. E. Meyrick, Prof. Westwood, and Mynheer P. C. T. Snellen.

Royal Meteorological Society, March 19.—Mr. H. F. Blanford, F.R.S., Vice-President, in the chair.—The following papers were read:—A brief notice respecting photography in relation to meteorological work, by Mr. G. M. Whipple. The first person to use photography for obtaining meteorological records was Mr. T. B. Jordan, of Falmouth, in 1838. Some years later, Sir F. Ronalds and Mr. C. Brooke devised more complete and elaborate apparatus; the arrangement of the former being now in use at the Observatories of the Meteorological Office, and that of the latter at the Royal Observatory, Greenwich. Reference was also made to Mr. J. B. Jordan's form of sunshine recorder, and to Captain Abney's photo-nephograph. The various photographic processes which have been employed in connection with these instruments were fully described.—Application of photography to meteorological phenomena, by Mr. W. Marriott. The author showed how photography could be most usefully employed for the advancement of meteorological knowledge. Much valuable information had been recently obtained from photographs of lightning and clouds. An interesting collection of such photographs was shown on the screen, together with others illustrating floods, whirlwinds, tornadoes, hailstorms, frost, snow, &c.—After the reading of these papers, the meeting was adjourned to allow the Fellows to inspect the Exhibition of Instruments, &c., an account of which we print elsewhere.

Mathematical Society, March 13.—J. J. Walker, F.R.S. President, in the chair.—The following communications were made:—Perfect numbers, by Major P. A. MacMahon, R.A.—The relation of distortion in prismatic images to dispersion, by Dr. J. Larmor.—On the satellite of a line relatively to a cubic, by the President (Prof. Greenhill, F.R.S., V.P., in the chair).—An approximate relation connecting successive terms of the expansion for tan x, by G. Heppel.

PARIS.

Academy of Sciences, March 17.—M. Hermite in the chair.—M. Maurice Lévy communicated a paper on the application of electro-dynamical laws to planetary motions. In a communication of February 17, M. Tisserand applied Gauss's formula of electro-dynamical attraction to the movement of celestial bodies without at all asserting it to be true. M. Lévy concludes that the formula is contrary to the doctrine of energy and to the facts, and shows that Riemann gave a law which, like that of Weber, is in accord with both.—On the photographic halo, and a method of making it disappear, by M. A. Cornu. The author has investigated the appearance and cause of the halos which surround intense points of light on a photographic plate, and the conditions necessary to remove them.—Under agricultural chemistry, M. Berthelot discusses the facts relating to observations on the reactions between the soil and atmospheric ammonia.—M. P. Schutzenberger, in researches on some phenomena produced during the condensation of gases containing carbon under the influence of the silent discharge, has investigated the composition of the brown solid formed together with carbonic acid from the condensation of carbonic oxide. The experimental results give a formula intermediate between $C_{12}H_2O_{10}$ and $C_{12}H_2O_{11}$.—Method of determining the pole of an ellipsoid of three unequal axes by the observation of its catoptric images, by M. D. E. Sulzer.—On a new system of electrical accumulators and some accessory apparatus, note by M. Charles Pollak.—On the double thiosulphates of lead and sodium, by M. J. Fogh.—The action of sulphuric acid on aluminium, by M. A. Ditte. The author finds aluminium to behave much like amalgamated zinc. With a smooth plate of this metal immersed in dilute cold sulphuric acid for some time but little hydrogen is liberated owing to the formation of a protecting film of the free gas, and that any circumstances tending to facilitate the removal of this film increase the rapidity of action of the acid; for instance, a trace of a chloride of any metal reduced by aluminium causes the plate to be comparatively rapidly attacked owing to the roughening of the surface due to the deposition of a metallic film; again a similar effect is obtained when the reaction is caused to occur in a vacuum, because of the freer disengagement of hydrogen. The product of the reaction is in the first place neutral sulphate of aluminium, but the reaction continues further, a basic sulphate being produced with further evolution of hydrogen. The conclusion is drawn that aluminium acts normally, in accordance with the heat of formation of its salts, when in contact with sulphuric acid or metallic sulphates, and that the slowness of the reaction is due to the mechanical interference of the liberated hydrogen.—On a new crystalline form of ammonium chloride, by MM. G. Geisenheimer and F. Leteur. M. Le Bel has shown the possibility of a second form of ammonium chloride (*Comptes rendus*, January 20, 1890); the authors give data leading them to conclude that they have probably obtained the second form, rendered stable by the presence of a slight impurity.—Note by M. J. Meunier, on the mono- and di-benz-acetals of sorbite.—On the α dextro- and lævo-rotatory borneol camphorates, by M. A. Haller. The author draws the conclusions—(1) that the total etherification of camphoric acid is only effected at a relatively high temperature and with the anhydride; (2) that isomeric bodies are certainly produced under these conditions; (3) that camphoric acid, in the acid ethers studied in this note, is analogous to phenol in its reactions.—On oxytetric acid, by M. Ch. Cloez.—On the value of the heat of hydration of malic acid, by M. Iw. Ossipoff.—Note by M. J. A. Muller, on the dissociation of the hydrochlorides of amines and dissolved salts of fatty acids. Using phenolphthalein as indicator, the author has been enabled to trace the dissociation of

these bodies on diluting or heating their solutions.—A botanical note, by M. Léon Guignard, on the formation and differentiation of the sexual elements which take part in fertilization.—Another botanical paper, by M. A. Prunet, on the comparative structure of the nodes and internodes in the trunk of the Dicotyledones.—Under geology, M. de Folin has a paper on the formation of nummulitic rocks. He concludes that these rocks are formed by the work of an organism of the same order as the Rhisopodes.—Also under geology, M. Stanislas Meunier contributes some chemical researches on the fossil shells of Foraminifera, Mollusks, and Crustacea. He has investigated the composition of the flocculent organic residue formed when these fossil shells are dissolved in acid.—On Pyrenean kersanton, its age and affinities with ophite, by M. J. Caralp.

Berlin.

Physiological Society, February 28.—Dr. Rosenstein exhibited a patient with distension of the lymphatics in the leg, and fistulous openings which discharged an albuminous fluid sometimes amounting to 1100 c.c. in a day. Dr. J. Munk has made observations on this fluid. It is sometimes transparent, but is always milky after a meal containing fat. It thus resembles chyle rather than lymph, and probably really is chyle. At least two-thirds of the fat given at any one meal reappeared in the fluid from the fistula. On giving olive oil, fat appeared in the fluid in two hours, increased steadily till its maximum after five hours, then diminished, and in ten or twelve hours disappeared. With a harder fat, *e.g.* mutton fat, the phenomena were the same, but were longer in appearing. Erucic acid given to the patient appeared as a neutral fat, and not as free acid, synthesis having been effected in the body. No appreciable absorption of fat occurs from the rectum. Large doses of starch or sugar scarcely increased the percentage of sugar, nor did large meals of albumen increase that of proteids in the fluid. Thus the only food-stuff which leaves the intestine by the lacteals is fat.

Meteorological Society, March 4.—Dr. Vettin, President, in the chair.—Dr. Wagner spoke on fire-damp explosions in mines in their relationship to cosmic and meteorological conditions. He discussed the collection of the gas, the conditions necessary for its explosion, the part played by coal-dust, and the several chance circumstances which may lead to the non-discovery of the gas in the workings. He next discussed the various means available for avoiding and removing accumulations of fire-damp, and gave an account of researches on the relationship of its explosion to varying barometric pressures. His own work had consisted in working up the statistics of the Dortmund mining district in which explosions are more frequent than in any other state of Prussia. The reports cover a period of 21 years and give a record of 7000 explosions. He first compared the numerical relationship of the explosions with the phases of the moon, and concluded that there is no connection between the two. He then made a similar comparison of their frequency with the rotational period of the sun, taking the latter as 25·5 days: the result was again negative. He finally compared their frequency with periods of 27·9 days, this being, according to Buys-Ballot, the cycle of temperature variations resulting from the sun's rotation. In this case the curves he obtained were quite uniform and regular, showing a maximum on the third day and a second maximum on the twentieth. He refrained from drawing any definite conclusions from this last observation in view of the numberless chance circumstances which may lead to explosions.

Physical Society, March 7.—Prof. Kundt, President, in the chair.—Dr. Rubens spoke on the employment of the bolometer for observing the electrical radiations of Hertz as carried out by himself and Dr. Ritter. Up to the present it had not been found possible to measure the intensity of the radiation owing to the extraordinarily minute amplitude of the oscillations; but the speaker had been able to carry out the determination by means of a bolometer whose construction and working he fully described. It consists essentially of an accurately balanced primary Wheatstone bridge, two of whose arms are again converted into secondary Wheatstone bridges. If a current passes through one of them its resistance is altered by the rise of temperature, and the galvanometer gives a proportionate throw. A similar effect is produced by a wave of electrical radiation, and hence its amplitude can be measured by this bolometer when once it has been calibrated. When experimenting with the polarizing wire-grating it was found that there is a constant relationship between the intensity of the rays which pass the grating and the angle of inclination of the wires to the plane of oscillation of the rays. It was further observed that the energy which does not pass the grating is reflected, and to the extent of 98 per cent., when the wires are at right-angles to the plane of oscillation. Experiments in illustration of the above were shown at the end of the communication.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Report of the Meteorological Service of Canada, 1886: C. Carpmael (Ottawa).—The Mammalia of the Uinta Formation: W. B. Scott and H. F. Osborn (Philadelphia).—A Monograph of Oriental Cicadidæ, Part 2: W. L. Distant (West, Newman).—Il Monismo: E. dal Pozzo di Mombello (Castello, Lapi).—British Fossils and where to seek them; J. W. Williams (Sonnenschein).—Poems, complete edition: W. Leighton (Stock).—Classification of Birds: H. Seebohm (Porter).—Personal and Social Evolution (Unwin).—Proceedings of the Physical Society of London, Vol. x. Part 3 (Taylor and Francis).—The Asclepiad, vol. vii. No. 25 (Longmans).—Travaux de la Société des Naturalistes de St. Pétersbourg, Section de Zoologie et de Physiologie, Tome xx. Livr. 2.—Supplément aux Travaux de la Société des Naturalistes de St. Pétersbourg.—An International Idiom; A Manual of the Oregon Trade Language: H. Hale (Whittaker).—Second Melbourne General Catalogue of 1211 Stars for the Epoch 1880 (Melbourne, Brain).—Essays of an Americanist: Dr. D. G. Brinton (Philadelphia. Porter and Coates).—Days and Hours in a Garden, 7th edition: E. and B. (Stock).—Weather and Tidal Forecasts, 1890: D. Dewar (Glasgow, Brown).—Royal University of Ireland Calendar for 1890 (Dublin, Thom).—Report of the Rugby School Natural History Society, 1889 (Rugby, Lawrence).—The Signing of the Treaty of Waitangi: W. Colenso (Wellington, Didsbury).—Mekrolog auf Theodor Kirsch (Berlin, Friedländer).—Journal of the Chemical Society, March (Gurney and Jackson).—Journal of Physiology, vol. xi., No. 3 (Cambridge).

CONTENTS.

THURSDAY, APRIL 3, 1890.

TECHNICAL EDUCATION IN THE CODE.

MR. KEKEWICH is to be congratulated on the reception which his Code has hitherto met with. From all sides it has been received with a unanimous chorus of congratulation, tempered only by the difficulty which has been experienced in distinguishing clearly what is new from what is old. Many parts of the Code have in fact been entirely re-cast and re-arranged, and in the absence of the schedule of alterations which it is customary to issue as an appendage to the Code, the compilers of abstracts for the daily papers have this year had a terrible time of it. They have been unable to criticise the alterations without reading the document through, and even this unwonted exercise has not prevented them in more than one case from reproducing as new, old and familiar articles, the order of which has been changed.

But these trials, and the further difficulty of picturing at once the effect on various classes of schools of the action and reaction of numberless modifications, additions, and omissions both small and great, fortunately affect us but little. A great part—some would say the most important part—of the alterations, deal with matters of finance, management, and control, rather than directly with the education given in the schools. And it is with this that we are chiefly concerned in the present article.

So far as regards the changes in curriculum there is no ambiguity. We may fairly congratulate the Government on a solid and unequivocal advance in the right direction. In fact, the framers of the Code have gone a very long way (without the aid of Sir Henry Roscoe's new Bill) to enable elementary school managers to provide technical education, or more strictly to provide the general educational basis on which all specialised technical instruction must be founded.

A few weeks ago, when dealing with the changes in the new Scotch Code, we ventured on two forecasts regarding the coming changes in English elementary schools. The first was that the English Education Office would be unable to maintain its previous *non possumus* attitude on the subject of manual instruction after the Scotch Department had virtually assented to Sir Horace Davey's now famous opinion by including manual training among the grant-earning subjects of the Code. The second was that the policy of the Department would be found to lean (as in Scotland) towards the encouragement and extension of "class subjects," taught throughout the whole school, even at the expense of "specific subjects" which only affect a small minority of picked scholars.

Both these forecasts, as we shall see, have been verified, but this does not by any means exhaust the new provisions by which the range of study, especially of technical and scientific instruction, is extended. We will consider some of the changes in order.

To take first the most striking change, the clause by which manual instruction for the first time is recognized as a part of elementary education will come to many as a surprise. It indicates a change of front on the part of the Department on a matter of interpretation of the Education Acts. Hitherto the authorities at Whitehall have declared that the recognition of manual training without a new Act of Parliament was impossible. They asserted that their hands were tied by statute. That was the position a few months ago. And now no statute has been altered, and manual instruction is in the Code. It may be taught either in or off the school premises, and either by the ordinary teachers of the school or by special instructors, provided "special and appropriate provision approved by the inspector is made for such instruction and the times for giving it are entered on the approved time-table." In a later clause manual instruction is specially recognised as an object to which part of the school funds may be devoted.

Thus the aim of the Bill just drafted by the Technical Association is virtually attained without it. One omission, however, may attract notice. No special grants are provided in aid of manual training. In Scotland, it becomes a "class subject," and is paid for accordingly, but no grant is attached to it in the English Code. We presume, however, that there is nothing to prevent it being paid for as a specific subject under the clauses which provide for grants in aid of any subject "if sanctioned by the Department," provided that "a graduated scheme for teaching it be submitted to, and approved by the inspector."[1]

There is, however, yet another way in which grants for manual instruction may be made, and, reading between the lines of the Code, it looks not unlikely that the Government mean to adopt it. Drawing is already paid for by the Science and Art Department, and in Art. 85 (*b*) of the new Code we find drawing and manual training coupled together. Boys in a school for older scholars must be taught drawing "with or without other manual training." Unless, then, the present confusion of overlapping authorities is to be made worse confounded, it is reasonable to expect that both these subjects will be under the same Department, and we shall look with interest for the inclusion of manual instruction in the next Science and Art Directory. There is this further inducement to the Government to take this course, that payments made by the Science and Art Department fall outside the 17*s*. 6*d*. limit. In any case, two main conditions should be fulfilled in making grants for manual instruction: first, that they should not be given on results of examination; secondly, that they should be dependent on a really effective inspection. The first condition is necessary because no satisfactory scheme of individual examination in such a subject can be devised so as to be a real test of efficiency; the second is necessary to guard the public purse from being depleted to enable small children to construct bad soap-boxes when they ought to be in school.

But if the official recognition of manual instruction (which we assume includes, as in the Technical Instruction Act, "modelling in wood, clay, and other material"), is the most striking victory of the advocates of technical instruction, there are other changes of greater importance from an educational point of view.

The Department has at last screwed itself up to the

[1] Arts. 16 and 101 (*f*).

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point of refusing to acknowledge any boys' school as efficient which does not include drawing in its curriculum. This is an enormous advance—how great will be seen if we remember that less than a million out of the five million scholars of our elementary schools are receiving instruction in drawing at the present time. It is a great advance, also, on the halting proposal of last year, when the requirement was restricted to large schools which aimed at the maximum grant. When a radical change, such as the present one, is proposed, it is only reasonable that the transition stage should be made easy for schools which have to adapt themselves to the new requirements. We make no complaint, therefore, of the year of grace granted before the regulation comes into force, nor even of the power given to the inspector to dispense with it altogether in cases where the "means of teaching drawing cannot be procured." This provision would, indeed, seriously cripple the usefulness of the change if it were intended to be permanent. But clearly it is only meant to obviate temporary hardships in small schools; and we may congratulate ourselves that within a short space of time, every boy (or at least every boy among the working classes) will be receiving instruction in what is stated by all authorities to be the indispensable basis of almost all technical instruction. As a corollary to the change, there is another of less importance, but of value in its way, which makes drawing an alternative to needlework for boys in infant schools.

While thus the manual instruction of boys is provided for, a useful extension is given to the curriculum for girls, by the provision of a grant for laundry work calculated on much the same principle as that for cookery.

Passing to science teaching, the reforms introduced are no less satisfactory. In the first place, science instruction (as well as manual training) is placed on the same footing as cookery as regards facilities for the grouping of schools for central instruction, and attendance at such centres will count as attendance at school.

A still more important change is the extension of the range of class subjects. Under former Codes a single course of elementary science was sketched out meagrely enough in Schedule II., while managers were invited if they pleased to submit alternative courses to the inspector. The result might have been expected. Science teaching gives in any case more trouble than geography, and the additional necessity of framing their own courses of instruction was quite enough to deter managers from taking up the subject. Now, however, while still giving permission to managers to draw up other courses of instruction, the Department gives a lead by suggesting as examples no fewer than eight different courses in various branches of science, which are embodied in a supplement to Schedule II. The subjects thus treated are mechanics, physiology, botany, agriculture, chemistry, sound, light, and heat, electricity and magnetism, and domestic economy; while the model course still retained in the main schedule embodies a scheme of elementary instruction in "nature knowledge" of a more mixed and varied character.

In each of the first two standards the instruction is to consist of thirty object-lessons in common things, designed to lead on to the more specialised instruction in the third and higher standards, the courses for which follow (perhaps somewhat too closely) the syllabus laid down for the corresponding subjects in the schedule of specific subjects. It has, of course, been necessary somewhat to simplify and curtail the schemes of instruction in adapting courses framed for picked pupils to suit the capacity of the whole school. It seems to us that the process of simplification might in some cases be carried still further with advantage. Elementary physics for children should consist of a general view of the properties of matter and the forces which act upon it, rather than a more detailed study of one out of many branches of the subject. This was the line taken up by Michael Faraday in his inimitable lectures to children on the "Physical Forces." This too is the view of the Scotch Department, which has laid down a course of class instruction in "Matter," designed to give general preliminary notions of the whole range of physics. And, we may add, this also is the view taken by the Science and Art Department in framing the alternative course in physics for those who (like the vast majority of elementary school children) are not likely to carry their study of physics to a higher stage.

This, however, is a matter of detail, while the suggestion of alternative courses in science, linked to the instruction of the Kindergarten by graduated object-lessons in the first two standards, is a reform which we cannot praise too highly.

Other changes to be noticed are the inclusion among class subjects of history, and the disappearance of the requirement that English grammar should be compulsory as a class subject.

Turning to the schedule of specific subjects, we find less alteration. Mensuration is separated from Euclid and the alternative course of mechanics disappears. There are a few slight changes in the syllabus of the various subjects. Thus the law of conservation of energy drops out of the course on mechanics, presumably because the idea is thought too hard for young children to grasp. But if it be too difficult for *picked* scholars in the fifth and higher standards, how comes it that in the new Scotch Code this very law appears in the syllabus for the "*class*" subject of "matter" (which we have alluded to above), as part of the course suitable for the whole of Standard IV.? Are Scotch children so very far in advance of English as this difference would seem to imply?

If, however, the fourth schedule presents few changes worthy of note, considerable additions are made to the list of specific subjects for which no special syllabus is suggested, such as book-keeping, shorthand, German, and (in Wales) Welsh. In this way the demand for commercial instruction is met, though how far advantage will be taken of the permission to present scholars in these new subjects remains to be seen. And lastly, payments will be made on account of any other specific subject which the Department may sanction, provided a graduated scheme of instruction be submitted to the inspector.

We have now completed the survey of the purely educational changes of the Code. Henceforth (assuming, as we do, that the provisions of the Code will come into force much in their present form) there can be little complaint on the part of advocates of scientific or technical instruction that its introduction into elementary schools is hindered by the action of the Department. There need be no longer any talk of an educational ladder

with its lower rungs wanting. How far managers will take advantage of their powers remain to be seen. The changes which are compulsory, such as that which makes drawing universal for boys' schools, will, of course, take effect widely at once. Those which are merely permissive may be slow in their operation. Meanwhile, those who are in earnest about the introduction of such subjects as manual training into elementary schools could not better occupy the time which intervenes before the new Code comes into force, at the end of August next, than in perfecting a graduated scheme of instruction such as may be confidently recommended to school managers to submit to the Education Department.

We have laid stress in this article on the proposed changes in the elementary school curriculum, because, important as these are, they are likely to be overshadowed in the coming discussions on the Code by other questions which appeal more directly to party politicians. We have thus left ourselves no room to do more than allude to other reforms which will affect as powerfully the educational character of our schools as the widening of the course of study. After all, the main guarantee of efficiency is the quality of the teaching staff. The new Code raises the requirements of the Department as to minimum staff, improves the regulations regarding the examination and training of pupil teachers, and provides for the creation (on a very limited scale it is true) of day Training Colleges attached to the Universities or Higher Local Colleges, as well as for the attendance of day students at the existing Training Colleges. The Code further revises the system under which the Parliamentary grant is paid, and almost entirely abolishes payment on results of individual examination. It gives freedom to teachers to classify their scholars as they please, so that a child may be in three different standards in the three R's, and in two different standards again in the two class subjects. All these and other changes, which demand much more notice than we can give them, make the Minute of the Department which has just seen the light emphatically a "Teachers' Code."

THE CAVE FAUNA OF NORTH AMERICA.

The Cave Fauna of North America, with Remarks on the Anatomy of the Brain and Origin of the Blind Species. By A. S. Packard. Pp. 1–156, with 27 Plates.

THIS important memoir is the first of vol. iv. of the "Memoirs of the National Academy of Sciences," and contains the results of an examination of the Mammoth Caves in Kentucky made during the months of April and May 1874, and of some other caves in Indiana and Virginia which were visited by the author at a later date.

A description of eighteen caves, with notes on their hydrography and geological age, and an account of the fauna of those which are better known, form the first section of the memoir. The caves form the natural drains of the country, all the surface drainage being at once carried down into them through the innumerable "sink-holes" which pierce the thin stratum overlying the Carboniferous Limestone, in which the caves are excavated. The Mammoth Cave is the largest and best known, with its 150 miles of passages and avenues, frequently crossing one another at different levels.

Their geological age is uncertain, but there is very little doubt but that they assumed their present proportions long after the melting of the glacial ice and are coæval with the Niagara river-gorge. And as the caves must have been incapable of supporting life while flooded, their preglacial fauna, if they had one, must have been killed off, and they could not have become ready for their present fauna until comparatively recent times; therefore, they must have been colonized by members of the existing fauna. The mode of colonization is very simple. Tracks of bears, wolves, and smaller animals occur in nearly all those caves which are easily accessible from without, and clinging to the skins of these animals various small Arthropods may have been carried in; other species of insects and Myriopods which naturally lead a subterranean life may voluntarily enter the fissures and sink-holes which abound in this region; others, again, get carried in by the agency of torrents which flow in during certain seasons of the year, as, for instance, the eyed fishes and species of Crustacea which abound in the surface waters.

That cave animals have entered the caves from without is further corroborated by the fact that in the case of very many cave species closely allied outdoor species are found in great numbers in the immediate vicinity of the caves. Also caves situated near one another are populated by a similar fauna, which allows us to classify them in groups closely corresponding to the various zoo-geographical regions of the country.

The author then proceeds to the systematic detailed description of the fauna, a section which constitutes more than one-third of the memoir. As in the case of the fauna of the outside world, the species of Arthropoda form a very large percentage of the total number of cave species; but, however different the groups to which the various species belong may be, they possess the common characteristics of slenderness of body and appendages and of the absence of functional eyes. The systematic description is followed by lists of all the North American and European cave species known at present, showing that the European species are by far the most numerous. It is therefore argued that the European caves have been inhabited for a longer period than the American.

Although the animal kingdom, at any rate as far as certain groups are concerned, is comparatively well represented, vegetable life is almost absent, evidently owing to the dryness and the absence of light; in fact, so far as is known at present, it is only represented by a few Fungi and two or three Moulds. The air must also be comparatively free from the germs of bacteria of putrefaction, as the decay of organic refuse is very slow, and meat hung up in the cave will keep a long time. But though bacteria are absent, their office is performed by larvæ of the blind beetle (*Adelops hirtus*) and of flies.

Cave animals are mostly carnivorous. The blind fish (*Amblyopsis*) lives on Crustacea, and especially on the blind crayfish, which in its turn preys upon living *Caudotea*, but how they and other small aquatic Crustaceans maintain an existence is unknown. The Myriopods, which are very common, feed on decayed wood and fungous growths.

However, in all cases, as a rule, food must be very

scanty, and "lack of food as well as the absence of light was one of the factors concerned in the diminution of size and in the slenderness of blind cave animals as compared with their lucicolous allies."

The effect of total darkness upon animals is twofold. Firstly, colour is either entirely or partially bleached, and, secondly, the sense of sight is lost. Eyesight may be lost in various ways. Either the optic lobes and nerves may atrophy, while the retina, pigment, and lens remain more or less persistent; or the optic lobes and nerves may persist, while the retina and eye-facets atrophy; or, again, the whole of the optic apparatus may atrophy. Examples of all these cases are given in the important chapter which is devoted to a description of the anatomy of the brain and eyes of certain blind Arthropods, and illustrated by numerous drawings of sections through various regions of the head.

It is argued that this atrophy must be comparatively sudden and wholesale, because no series of individuals has been found with the optic lobes or nerves in different stages of disappearance. Transitional forms have been observed with eyes with a varying number of crystalline lenses, as in the case of *Chthonius*; those individuals which live near the mouth of the cave have better developed eyes than those which live far in. And surely, on further examination, more transitional forms will be discovered, as animals must be continually getting into the caves from the outside; their descendants becoming gradually adapted for cave life, until they finally reach the degree of modification of the present older occupants.

As the sense of sight diminishes, it is compensated by an increase of the delicacy of other senses. The tactile and olfactory senses are rendered more sensitive, the appendages become much more slender, and the blind form is altogether more timid and cautious than its eyed allies, as has been particularly noticed in the blind cray-fish.

The last part of this memoir deals with what is of most general interest to the biologist, viz. the bearing of these facts upon the theories of evolution. The author states that here the term "natural selection" expresses the result of a series of causes rather than any one cause in itself. The most important of these causes are: the *change of environment*, from light to partial or total darkness, involving diminution of food, the disuse and loss of certain organs, with compensation as has been mentioned above; *adaptation*, enabling the more plastic forms to survive and perpetuate the stock; *heredity*, which operates to secure the future permanence of the newly originated forms—the longer it acts, the earlier will the inherited characters appear in the development of the animal; and, lastly, *isolation*, which, after adaptation and heredity have established the typical characters, prevents inter-crossing with out-door forms, and thus insures the permanence of these characters.

The author adduces facts which seem to prove that the organic adaptations to a life in darkness may have been induced after but a few generations, perhaps one or two only, resulting in the comparatively rapid evolution of cave species. If that be the case, then, there is no reason why they should not be produced artificially, but at present no experiments have been made to prove the mutual convertibility of cave species and their lucicolous allies. If a cave species could be made to revert to an epigean form by keeping it for a number of generations in a gradually increasing amount of light; and if, on the other hand, a lucicolous species could be changed into a cave form by a converse process, the theory of occasional rapid evolution due to sudden changes in the environment would receive its final proof.

Mr. Packard draws attention to the interesting parallel between the life of the abysses of oceans and lakes and that of caves. In both cases vegetable life is almost absent, and a large proportion of the animal forms have become similarly modified with regard to the degeneration of the optic organs and corresponding development of other organs as compensation. But while caves have only been populated comparatively recently, the ocean abysses have had inhabitants for a very much longer time, and consequently these have had time to become much more highly specialized than the inhabitants of caves.

This most valuable contribution terminates with a bibliography containing the titles of previous publications on the subject, and we must not omit to mention that in a separate chapter a list is given of the known non-cavernicolous blind animals. As far as the higher classes are concerned, this list contains about the same number of species as the one of the blind cave-dwelling forms.

R. T. G.

LINEAR DIFFERENTIAL EQUATIONS.

A Treatise on Linear Differential Equations. By Thomas Craig, Ph.D. Vol. I. Equations with Uniform Coefficients. (New York: John Wiley and Sons, 1889.)

TREATISES on this subject have been somewhat numerous of late. We recently noticed in these columns an excellent, but fairly elementary work, "On Ordinary and Partial Differential Equations," by Prof. Woolsey Johnson. The student who wishes to enter on the profitable perusal of the book before us must be well versed in all the ordinary modes of procedure,[1] and then he will find that Dr. Craig is well qualified to lead him through the intricate windings of this difficult branch of mathematics. The advanced student will find the author's analyses of use to him whilst reading the various original memoirs here introduced to him, for the first time, in English. Some may remember that Mr. Forsyth, in his classical treatise, omitted the investigations of Fuchs, the recent researches of Hermite and Halphen, contented himself with a slight sketch of Jacobi's method for partial differential equations, and did not at all touch upon the methods of Cauchy, Lie, and Mayer. The consideration of these matters he reserved for a future volume.

The theory of the subject before us, *i.e.* of linear differential equations, almost owes its origin, in Dr. Craig's opinion, to two memoirs by Fuchs, published in vols. lxvi. and lxviii. of *Crelle's Journal* (1866, 1868):—

"Previous to this the only class of linear differential equations for which a general method of integration was known, was the class of equations with constant coefficients, including, of course, Legendre's well-known equation, which is immediately transformable into one with

[1] "The reader is of course supposed to be familiar with the ordinary elementary theory of differential equations" (p. 32).

constant coefficients. After the appearance of Fuchs's second memoir, many mathematicians, particularly in France and Germany, including Fuchs himself, took up the subject, which, though still in its infancy, now possesses a very large literature."

As happens in such cases, these memoirs have to be dug out of journals and publications of learned Societies before the student can be put in possession of results obtained. It is for this labour of research, and then for the arrangement in due sequence of theorems, that the reader has to thank Dr. Craig.[1] Even in the first two chapters, where most of the results are old, the treatment is comparatively new, being founded upon papers by Laguerre (*Comptes rendus*, 1879), and upon memoirs, or works, by Briot and Bouquet and Jordan; reference is also made, in connection with a proof by Jordan, to a paper by Picard (*Bulletin des Sciences Math.*, 1888). Here we may note that the author reserves an account of the investigations of Laguerre, Halphen, and others, from a still higher point of view, to a subsequent volume.

This first instalment discusses principally Fuchs's type of equations, but accounts are given of the researches of Frobenius (chapters iv., viii.), Markoff, Heun, Riemann, and Humbert (chapter vi.), Thomé (chapter ix.), Halphen (chapter xii.), Forsyth's canonical form and associate equations, Brioschi, Lagrange's adjoint equation, Halphen's adjoint quantics and Appell's theorem (chapter xiii.), and Picard (chapter xiv.). An account, due to Jordan, is given of the application of the theory of substitutions to linear differential equations (chapter iii.). Many points are touched lightly here, a fuller development being held in reserve. A prominent feature is the reproduction (chapter vii) of a thesis by M. E. Goursat on equations of the second order satisfied by the hypergeometric series. This consists of two parts. The first part gives an application of Cauchy's theorem, and relations between Kummer's (24) integrals, an application to the complete elliptic integral of the first kind, and Schwarz's results. The second part discusses the transformations of the hypergeometric series, Tannery's theorem, and some other points, the article closing with a collection of 137 transformations due (apparently) to Kummer.

The pages bristle with references to original sources, so that, as we have already indicated, this treatise is an invaluable handy-book to what has been done in this field.

One more word: there is no collection of examples for solution on the Cambridge model, but the work is strictly on the lines of a French or German treatise.

The book itself is very elegantly turned out.

[1] For instance, he obtains certain forms in the same way that Fuchs obtained them, "if for no other reason than that of the desirability of developing the subject in historical order" (p. 64).

THE BACTERIA OF ASIATIC CHOLERA.

The Bacteria of Asiatic Cholera. By E. Klein, M.D (London: Macmillan and Co., 1889.)

SO masterly and complete was the account which Koch gave in 1884 of the comma-bacillus, which he held to be the virus of cholera, that but little, if anything, has been added to our knowledge of its mode of growth, of its reaction to dyes, or of its life-history. As might be expected, the assiduity of many observers, now it has been directed to the subject, has led to the discovery of many other bacilli, which may be described as comma-shaped. But, so far, no bacteriologist, who has had his observations corroborated by other observers, has proved that any of them are indistinguishable in all their physical characters, whether in appearance, in reaction to dyes, or in their mode of growth, &c., from the choleraic bacillus. So far as is known, animals are not susceptible to cholera. If Asiatic cholera could be induced by inoculating with pure cultivations of choleraic comma-bacilli, then beyond a doubt they would be the *vera causa*, or, in other words, the contagium of cholera; but this step in Koch's argument was wanting, probably for the above-named reason, and is likely to remain so; the experimental inoculations of guinea-pigs which have taken place being by no means conclusive.

The present volume is a valuable and most trenchant criticism of every step of Koch's argument, and may be said to contain everything that can at present be said against Koch's theory, of which the author is the most active opponent.

The author commences with an account of the various comma-shaped bacilli which are at present known, and there are well-recognized characteristics which distinguish them from the first form, in all of them, except in those which depend upon solitary observations.

The following is the list of comma-shaped bacilli with the names of their discoverers:—

(1) Koch, in Asiatic cholera; $\frac{1}{2}$ to $\frac{2}{3}$ the length of tubercle bacilli, but thicker and curved. (2) Finkler and Prior, in cholera nostras; but Koch and Frank failed to demonstrate these in typical cases. They are thicker and longer than (1). In 10 per cent. gelatine, the growth is broad and conical, liquefying the gelatine more rapidly. (3) Lewis, in the fluid of the mouth, thicker than (1) Klein only twice has succeeded in growing them; every one else has failed. (4) Miller, in some cases of caries of the teeth, similar to (2). (5) Kuisl, in human fæces similar to (2). (6) Deneke, in stale cheeses. The growth on gelatine is similar, but they will not grow on potatoes. (7) Klein, in some cases of diarrhœa, especially in monkeys. They grow differetly in gelatine, and cause it to smell offensively. (8) Ermengen and others, in the intestines of guinea-pigs, pigs, rabbits, horses, &c., but they will not grow in 10 per cent. gelatine. (9) Lingard, two kinds in a case of noma, the smaller of which is said to have been very similar to the choleraic one. (10) Weibel, various forms in mucus, but their mode of growth is distinct. (11) Gamaleïa, in a fatal fowl disease, which was prevalent at Odessa. He did not distinguish them from (1). (12) Klein, in the intestines of a monkey with diarrhœa. The organisms were smaller, but the growth was similar to (1).

Klein lays great stress upon the difficulty there is in demonstrating the presence of the bacilli in the walls of the intestine in cases of cholera, and thinks that they are not present in the parts which are still alive, but only where the tissue has died; moreover they are absent from the blood.

The bacilli are most readily found in the mucous flakes; and in the presence of fæcal matter they are

readily destroyed, which may explain why they are sometimes not easily detected.

The author has done good service in threshing out all the evidence afresh, but the matter remains very much where Koch left it. The detection of the bacilli may enable us more readily to diagnose the earliest cases in an epidemic of cholera; and, as one result of his experiments, we may expect soiled linen to be most efficiently sterilized by drying it; at the same time, until the disease has been reproduced by inoculation with the organism, it cannot be said to be conclusively proved that this is the true virus.

OUR BOOK SHELF.

Manuel de l'Analyse des Vins. Par Ernest Barillot. Pp. xii.–131. (Paris: Gauthier-Villars et Fils, 1889.)

THE student of practical chemistry will find in this book a handy guide to the examination of wines. Works on the same subject are frequently rendered both unwieldy and tiresome by a multiplicity of analytical methods and the introduction of a bulky collection of tables embodying the composition of various classes of wine, a knowledge of which is deemed necessary in forming an opinion of the quality or purity of a particular sample. Here, however, details of this kind are reduced to a minimum. One or two methods, only, of carrying out any estimation are given, and free use is made of such empirical relations between the proportions of the constituents of a wine as seem warranted by the results of previous analyses.

The book consists of two parts and an appendix. Part I. is concerned with the determination of the normal constituents of wines, alcohol, total solids, ash, grape sugar, &c. Part II. deals with adulterations. In its opening sections are placed the indications traceable to the presence of added water, added alcohol, cane sugar dextrine, &c., but the greater bulk of the part is devoted to the detection of foreign colouring matters. The subject of colour reactions is very fully treated, and by the arrangement of the experiments in tabular form their nature and interpretation can be readily appreciated. It seems a pity that in connection with these tests no notice is taken in the text of the absorption spectrum of the colouring agents, as a clue to their identification; in a footnote the author contents himself by merely referring the reader to the works of Vogel and Wurtz for information on this subject. In the appendix is a statement of the chemical constitution of the colouring matters mentioned, followed by an account of some recent work of the author on the detection of added alcohol. His method is based on the effect of the alcohol introduced on the proportion of volatile acid which distils from the wine, and the result is shown to be consistent with the theory of the rate of etherification of organic acids.

The book is intended to be useful for commercial purposes, and for such the analytical processes described are sufficiently accurate. The apparatus employed, as is stated in a footnote, has been constructed by the Société Centrale de Produits Chimiques, and judging from the illustrations, is in some cases, to English eyes at least, a trifle antiquated. The occasional reference to vessels provided with marks, and to which no numerical values are attached, detracts somewhat from the general usefulness of the book, and is unintelligible to a reader who has failed to notice the explanatory footnote.

The graduation of alcoholometers, the maximum amount of alcohol permissible in wines, &c., are of course in accordance with the regulations of the French Excise.

Synoptical Tables of Organic and Inorganic Chemistry. Compiled by Clement J. Leaper, F.C.S. (London: George Gill and Sons, 1890.)

THE compiler says in his preface that "the mass of facts presented to the mind of the beginner in chemistry is so large that he often experiences a difficulty in distinguishing the useful from the ornamental, and is apt, consequently, to neglect fundamental principles and reactions for comparatively useless minutiæ. These tables are intended to prevent this error. . . . The experience of many years has convinced the author that the student who honestly commits these tables to memory will lay for himself a solid groundwork for future reading and research." Whatever may be meant therefore by the expression "future reading and research," it appears that the compiler aims no higher than to give a series of unconnected statements which if learned will enable the would-be student to begin his study of chemistry. We do not think this committing to memory will make the study more easy, and should fear that the learner might imagine after his memory exercise that he thereby knew something of chemistry. The separation of "the useful from the ornamental" is always difficult, and it is rare to find two authorities at one in such a matter. It is doubtful, for example, whether any chemist will agree with the compiler when he states as Charles's law that "All gases expand or contract $\frac{1}{273}$ of their volume for each rise or fall of 1° C.," and omits, presumably as ornamental, the limitation of this proportion to the volume of the gas at 0° C.

The British Journal Photographic Almanac, 1890. Edited by J. Traill Taylor. (London: Henry Greenwood and Co., 1890.)

IN this year's volume we find a most interesting collection of notes and articles relating to almost every branch of the subject. Captain Abney contributes an article in which he warns photographers to beware of their principal enemy—dust—and concludes with the best method of exclusion. The Rev. S. J. Perry gives a short summary of the instruments used in celestial photography during the past year, and of the work accomplished, including the wonderful photographs taken by Isaac Roberts of the nebula of Andromeda, nebulæ in the Pleiades, &c. Mention is also made of the success of Mr. Common in rendering still more perfect the reflecting surface of his magnificent five-foot glass mirror. Amongst the other articles we may refer to that on halation by Chapman Jones, hydroquinone by W. B. Bolton, and celluloid films by Colonel J. Waterhouse. An epitome of the year's progress, with notes on passing events, original and selected, is given by the editor, who marks the great advance made in film photography, and also the tendency to diminish the bulk of cameras, as shown by the innumerable hand or detective cameras that have appeared during the last twelve months. Allusion also is made to the new developer, eikonogen, which can, it is believed, develop into full printing density a plate that has been impressed by feeble radiations.

No alteration has been made as regards the general order of the work; there are only slight additions to the tables, formulæ, &c. The specimens of processes which illustrate the volume, especially that of Mrs. Sterling from a negative by Vander Weyde, are very fine.

Four-Figure Mathematical Tables. By J. T. Bottomley, M.A., F.R.S., &c. Second Edition. (London: Macmillan and Co., 1890.)

THIS useful collection of tables has been considerably enlarged and revised since its first appearance. It comprises logarithmic and trigonometrical tables, tables of squares, square roots, and reciprocals, and a collection of useful formulæ and constants. The introduction is still

ciently detailed to make the construction of the table readily understood, assuming a knowledge of the use of logarithms. The book will prove a handy substitute for more bulky volumes in cases where extreme accuracy is not required, such as computations in chemistry and physics.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Panmixia.

MY letter of March 6 commenced with the remark that, without entering into controversy, I proposed to draw attention to the opinions expressed concerning the inheritance of acquired characters by Mr. Darwin. The reasons for my own beliefs on the questions at issue I have given in "The Principles of Biology," § 166, and, with other illustrations, in "The Factors of Organic Evolution." Here it must suffice to say that I have seen no reason to abandon the conclusions there set forth.

Respecting the doctrine of "panmixia," either as enunciated by Prof. Weismann, or as recently presented in modified forms, I will say no more than that I should like to see its adequacy discussed in connection with a specific instance—say the drooping ears of many domesticated animals. "Cats in China, horses in parts of Russia, sheep in Italy and elsewhere, the guinea-pig in Germany, goats and cattle in India, rabbits, pigs, and dogs in all long-civilized countries, have dependent ears."

Here the influence of natural selection is almost wholly excluded; nor can artificial selection be supposed to have operated in most of the cases: save, perhaps, in some pet animals, selection has been carried on to develop other traits. In the cases of most of these creatures, too, artificially fed and often over-fed, it does not seem that individual fates can have been affected by economy of nutrition, either general or special; since there has been no struggle for existence to cause the survival of those in which nutriment was most advantageously distributed. Further, the parts in question are not of such sizes that economy in nutrition of them could sensibly affect the fates of individuals, even had the struggle for existence been going on. Again, it seems that in respect of the ears themselves (though not in respect of their motor muscles) there has been extravagance of nutrition rather than economy of nutrition; since even where selection has been carried on for increasing other traits, the ears have not dwindled but rather increased. Lastly, at the same time that there has been this superfluity of nutrition in the ears themselves, their motor muscles appear to have dwindled either relatively or absolutely—at least relatively, we must suppose, where the weight of the ears has increased, and absolutely where the weight of the ears has not increased.

The question presented by these facts is one in the solution of which the theory of "panmixia" may, I think, be satisfactorily tested; and without expressing any opinion upon the matter myself, I should be glad to see it discussed.

HERBERT SPENCER.

I AM not aware how far Prof. Ray Lankester is disposed to acknowledge his obligations to Prof. Weismann for what I am glad to see he now calls his "anti-Lamarckian" (as distinguished from "pure Darwinian") proclivities. Therefore I do not know how far he professes to be one of "the followers of Prof. Weismann," to whom my previous letter on this subject was addressed. But it seems desirable that I should take some notice of the altogether distinct question which he has now raised—viz. whether, or how far, Prof. Weismann's anti-Lamarckian views were anticipated by Mr. Darwin.

His argument is that Darwin must have been a Lankesterian anti-Lamarckian in disguise; and, more particularly, that "the doctrine of panmixia is recognized and formulated in the last (sixth) edition of the 'Origin of Species' published in 1872."

Taking the most general statement first, Prof. Lankester represents it as not improbable that "when Darwin refers, here and there throughout his works, to a reduced or rudimentary condition of an organ as 'due to disuse,' or 'explained by the effects of disuse,' he does not *necessarily* mean such effects as the Lamarckian second law asserted and assumed (though often he does appear to mean such); but he may mean, and probably had in his mind, the effects of disuse as worked out through panmixia and economy of growth."

Now, here we have a specimen of Prof. Lankester's dialectic at its worst. Truly, with such an interpreter, Darwin "*may*" be made to "mean" anything. First it is represented as seeming "not at all improbable that when Darwin refers" to one principle, "he does not *necessarily* mean" what he says; and then it is concluded that "he may mean, and *probably* had in his mind a totally different principle." Moreover, what is represented as mere references, "here and there throughout his works," are, as all the world knows, one whole and "highly important" (though still subordinate) side of Darwin's system. Yet again, in all passages where the meaning assigned to his term "disuse" is explained, there can be no shadow of ambiguity attaching to it, and everywhere it is alluded to as a principle wholly distinct from the "economy of growth"; while panmixia, as I shall presently prove, is nowhere mentioned at all. This, indeed, is clearly shown even in the passages quoted by Prof. Lankester, and now re-quoted below. For it is there said that, could a certain explanation be found, "then we should be able to understand how an organ which has *become useless* would be rendered, *independently of the effects of disuse*, rudimentary." Obviously, in this context, "the effects of disuse" cannot possibly mean "the effects of disuse as worked out through panmixia and economy of growth": they *can* only mean the direct effects of disuse itself in causing inherited atrophy. And now, lastly, "the effects of disuse" are habitually pointed to by Mr. Darwin in association with the "effects of *increased use*"; and how he can "seem" to have "explained" *these* either by the economy of growth (which he fully recognized), or by panmixia (which he never recognized), I must leave Prof. Lankester to indicate.

It will be observed, from the point last mentioned, that this attempt to read the doctrines of Weismann into the writings of Darwin must equally collapse, whether or not any other human being can be found to follow Prof. Lankester in his commentary on Darwin's "here and there" references to "the effects of disuse": the equally constant and as frequently detailed references to "the effects of the increased use of parts, which I have always maintained to be highly important," are of themselves sufficient to dispose of the Lankesterian gloss. Nevertheless, it remains worth while to see whether there is any shred of evidence in support of the narrower or more particular statement, that the principle of panmixia is to be found "already indicated" in the "Origin of Species." The following are the passages upon which this statement is founded—passages, I may remark, which have certainly neither been "missed" nor "neglected" by me.

(1) "If under changed conditions of life a structure before useful, becomes less useful, its diminution will be favoured, *for it will profit the individual not to have its nutriment wasted in building up a useless structure*. . . . Thus, as I believe, *natural selection* will tend in the long run to reduce any part of the organization as soon as it becomes, through changed habits, superfluous, without by any means causing some other part to be largely developed in a corresponding degree" ("Origin of Species," sixth edition, p. 118).

(2) "Organs, originally formed by the aid of natural selection, when rendered useless, may well be *variable*, for their variations can no longer be checked by natural selection. . . . It is scarcely possible that disuse can go on producing any further effect after the organ has once been rendered functionless. Some additional explanation is here requisite, which I cannot give. If, for instance, it could be proved that every part of the organization tends to vary in a greater degree towards diminution than towards augmentation of size, then we should be able to understand how an organ which has become useless would be rendered, independently of the effects of disuse, rudimentary, and would at last be wholly suppressed; for the variations towards diminished size would no longer be checked by natural selection. *The principle of the economy of growth* explained in a former chapter [cited in quotation No. 1], by which the materials forming any part, if not useful to the possessor, are saved as far as possible, will perhaps come into play in rendering a useless part rudimentary" ("Origin of Species," sixth edition, pp. 401–402).

Can it be that Prof. Lankester has not even yet perceived the significance of "the idea" of panmixia? Such certainly seems to be the case from his use of the above quotations. For the words which I have italicized render it most obvious that the only principle under consideration is the economy of growth or nutrition, *i.e.* the *reversal* of selection: there is no allusion to panmixia, or the *cessation* of selection. In the second passage it is shown that, because "no longer checked by natural selection," useless organs will become *variable;* and hence that *if there were any other cause tending to degeneration* (such as the "impoverished conditions" subsequently suggested), natural selection would *not interfere* with—*i.e.* prevent or "check"—the degenerating process thus induced. But there is no hint that the mere cessation of natural selection must be *itself*, and in *all cases*, a cause of degeneration.

Similarly, at the end of his letter, Prof. Lankester again fails to distinguish between the cessation and the reversal of selection. For, after endeavouring to represent that Mr. Darwin did not understand my "view,"[1] he says, "it is not at all surprising that Mr. Darwin did not recognize any resemblance between it and his own statement, viz. that 'the materials forming any part, if not useful to the possessor, are *saved* as far as possible,' thus 'rendering a useless part rudimentary.'" Not surprising, indeed. But it is surprising that Prof. Lankester, even at this time of day, should thus appear incapable of clearly distinguishing between natural selection as *withdrawn* and as *reversed.* For this is the whole point, and the only point so far as "the doctrine of panmixia" is concerned. It is a matter of familiar knowledge that Mr. Darwin at all times and through all his works laid considerable stress upon the "economy of growth," (or, more generally, reversed selection); but, most emphatically, this is *not*, as Prof. Lankester now says it is, "the essence of the anti-Lamarckian view of the effects of disuse." The essence of this view is, and can only be, the *cessation* of selection, as Prof. Weismann has clearly perceived.[2]

In order that there shall be no doubt upon this point, I must here explain the importance of the *cessation* of selection, as distinguished from the *reversal* of selection, in regard to "the essence of the anti-Lamarckian view"—even though in so doing "I feel it rather a severe burden when I am called upon to expound the merest commonplaces of the subject under discussion."

As stated in my previous letter, "the principal evidence on which Mr. Darwin relied to prove the inheritance of acquired characters was that which he derived from the apparently inherited effects of use and disuse—especially as regards the bones *of our domesticated animals.*" Now, the reason why our domesticated animals appeared to furnish the most unequivocal proof of the inherited effects of disuse (and so, likewise, of the inherited effects of use, as explained in my last letter) was this. In the case of all species in a state of nature, it is, as Darwin observed, impossible to eliminate the effects of natural selection (acting through the economy of growth, or otherwise) from those of disuse, supposing disuse to be a cause of degeneration in species as it is in individuals. Therefore, in order to estimate what, if any, is the proportional part that is played in degeneration by the inherited effects of disuse, it is necessary to find cases where disuse, if it ever acts at all, must be acting *alone.* Such cases Mr. Darwin took to be furnished by our domesticated animals, seeing that they are so largely protected from the struggle for existence on the one hand, while, "on the other hand, with highly-fed domesticated animals, there seems to be no economy of growth, nor any tendency to the elimination of superfluous details." Having found in such cases material for ascertaining the effects apparently caused by disuse *alone*, Darwin concluded that he was able to estimate the degree in which these effects occurred elsewhere, or generally; even though in all wild species they must usually be more or less associated with the effects of reversed selection. Therefore it was that he chose domesticated animals for all his weighings and measurings of comparatively disused parts—with the result of appearing to obtain good evidence of a high degree of reduction as due to the inherited effects of disuse alone. But it did not occur to him that the amount of reduction thus proved might be equally well explained, not indeed by the *reversal* of selection (as in wild species), but by the *cessation* of selection, or panmixia. And it is just because the cessation of selection thus applies with even more certainty to the case of domesticated animals, than does the reversal of selection to the case of wild animals, that the former principle is of such unique importance to "the essence of the anti-Lamarckian view": by its means, *and by its means alone*, can the apparent evidence of the inherited effects of disuse be overthrown.

Therefore, by seeking to assimilate the distinct principles of selection as withdrawn and selection as reversed, Prof. Lankester is performing but a sorry service to his anti-Lamarckian cause. Weismann may well cry, "Save me from my friends," when he finds them thus playing into the hands of his opponents. For on all the logical bearings of his principle of panmixia, Weismann has perfectly clear and accurate views; and although he was not accurate in representing the relations which obtain between this principle and that of reversed selection, such is but a small error compared with Lankester's identification of the two principles—with the necessary result of again bringing into court the whole body of direct evidence on which Darwin relied in his apparent proof of Lamarck's "second law."

We shall now, perhaps, be able to understand what Prof. Lankester means when he says: "The idea [of panmixia] occurred to me also shortly after the passages above quoted from Mr. Darwin were published." If this is the case, "the idea" in question must have "occurred" to Prof. Lankester before he had reached his teens, seeing that one of "the passages" in question is not confined to "the last edition of the 'Origin of Species,'" but runs through them all. Allowing this to pass, however, what I have now to remark is, that if the idea which occurred to Prof. Lankester "shortly after the publication of that work" (1872) was, as he alleges, the idea of panmixia, it becomes a most unaccountable fact that in his laborious essay on "Degeneration" (1880) there is no hint of, or even the most distant allusion to, this idea. Yet, in the presence of this idea, "Hamlet" without the Prince of Denmark would be a highly finished work compared with an essay on "Degeneration" without any mention of panmixia. Therefore, here again, I can only understand that Prof. Lankester has not even yet assimilated "the idea in question." He confounds this idea with that of the economy of growth: he fails to perceive the very "essence" of the idea, in the all-important distinction between selection as withdrawn and selection as reversed. Without question, his essay on "Degeneration" proves a familiar acquaintance with the doctrine that "the materials forming any part, if not useful to the possessor, are saved as far as possible"; but, most emphatically, this is *not* "the idea of panmixia," while it *is* the idea that is definitely "formulated" scores and scores of times through all the editions of Mr. Darwin's works—an "idea," therefore, which must necessarily have "occurred" to every reader of those works since the time when Prof. Lankester was at school.

As this letter has already run to an inordinate length, I will relegate to a footnote my discussion of the merely personal criticisms which Prof. Lankester has passed upon my former communication.[1] GEORGE J. ROMANES.

London, March 28.

[1] There is something comical to me in this endeavour, in view of all the conversations and correspondence which I had with Mr. Darwin upon the cessation of selection. Moreover, I do not in the least agree with Prof. Lankester where he says that my "view, as it appears in Mr. Darwin's words ('Variation,' &c., vol. ii. p. 309), is certainly *not* the same as that which Mr. Romanes has expounded in NATURE of March 13, 1890." That my "view" is not *fully* given, Mr. Darwin himself affirms; but, "as far as it can be given in a few words," it is given as correctly as I could wish.

[2] It appears to me that Prof. Lankester cannot have read Prof. Weismann's exposition of "the doctrine of panmixia." For, not only does he make this otherwise unaccountable (and, in relation to his "anti-Lamarckian view," suicidal) blunder of seeking to unite, if not virtually to identify, the principles of panmixia and economy of growth; but he alludes to Weismann as having "stated briefly" the former principle. "Stated briefly" it certainly is in "the translated essays"; but this is only because it is set out at length in one of the untranslated essays, which is entirely devoted to expounding the matter ("Ueber den Rückschritt in der Natur"). And this reminds me that in his review of Mr. Wallace's "Darwinism" there is a passage which similarly indicates that Prof. Lankester has either not read, or has strangely forgotten, another of Weismann's unpublished essays. Therefore, seeing how ready he is, on account of a precisely similar omission, to jump upon Mr. Herbert Spencer—whose recent and protracted illness is notorious—one can scarcely refrain from asking in his own words, "Will not Mr. Spencer and *others* who are interested in these matters read Weismann's essays?"

[1] Prof. Lankester says:—"As soon as the matter had taken root in his mind, Mr. Romanes published in NATURE, March 12, April 7, and July 2, 1874, an exposition of the importance of the principle of cessation of selection as a commentary upon a letter by Mr. Darwin himself (NATURE, vol. viii. pp. 432, 505), in which Mr. Darwin had suggested that, with organisms subjected to unfavourable conditions, all the parts would tend towards reduction. Mr. Darwin, with his usual kindly manner towards the suggestions of a young writer, gives, at p. 309 of vol. ii. of 'Animals and Plants under Domestication,' Mr. Romanes's view, 'as far as it can be given in a few words.'" Now, as it is only a few days ago that I myself directed Prof.

The Spectrum of Subchloride of Copper.

IT is noticed in NATURE (vol. xli. p. 383), as the substance of a paper read to the Academy of Sciences in Paris, on the 10th ult., by M. G. Salet, on the blue flame of common salt, and on the spectroscopic reaction of copper-chloride, that the strongest lines of the former flame, in the indigo and blue, are due to copper-chloride, and coincide with bands given in M. Lecoq de Boisbaudran's "Spectres Lumineux."

Copper and chlorine appear, from the easy formation of copper-subchloride, to have a very unstable affinity for each other; and the readiness with which copper itself seems to volatilize, as shown by Mr. John Parry, in his spectroscopic experiments for the Ebbw Vale Steel-making Company in Wales, on the detection of impurities in iron and steel, by the free and wide diffusion of its vapours compared with those of other metals to a distance from a blowpipe flame, would perhaps tend to promote dissociation and to the production of subchloride from chloride of copper, at least in the presence of reducing-gases, in a flame.

There is a considerable general resemblance in respect of place and brightness between the groups of lines belonging to chlorine, and those belonging to copper-chloride, as those two spectra are represented in M. Lecoq de Boisbaudran's work. But the two spectra are of course very far from showing any precise coincidences with each other. My attention was drawn some time ago (in July 1878, NATURE, vol. xviii. p. 300) to a set of line bands of this same description, in very near correspondence, apparently with the chief lines of the copper-chloride spectrum, which presented itself in a violet-blue flame seen very frequently in ordinary fires when they have been fed with almost any kind of household dust and rubbish. But the remarkably neat triplet of line-pairs—green, blue, and indigo—in this blue fire-flame's spectrum could only be recognized as very indistinctly matched by those chief lines of the spectrum of copper-chloride, as those are produced, for instance, by introducing into a Bunsen-flame a piece of copper-foil well wetted with hydrochloric acid; and no counterpart at all to them, any more than to the ordinary chloride of copper spectrum, could be traced in the well known blue fire-flame of common salt, in whose spectrum, when pure, as well as in that of the equally familiar blue fire-flame (when pure also) of carbonic oxide, I do not remember to have ever detected any lines or bands of greatest brightness so obviously discernible and distinct as to admit of measurements.

In the case of a copper-melting furnace, round the loose junction of whose lid small escaping bodies of blue flame, on one of the days on which I analyzed them, showed the well-defined triplet spectrum very neatly, it was afterwards mentioned to me (when that observation had been noted at the above place in NATURE), that pieces of ships' old copper-sheathings were sometimes put into the copper-melting pot; and just as the use of logs of broken-up ship-timber (as was also stated at that place in NATURE) explained a gorgeous blaze of this flame's fine blue colour in a London house-fire very satisfactorily, so foreign importations by salt into waste-materials from seaworn ships, might by such a practice's occurrence as this in the melting furnace, account very well for the presence of chlorine along with copper in the furnace efflagrations which showed the neat and easily recognized line-spectrum on one of the days of my spectroscopic examinations of them, very plainly. Neglected scraps of brass and copper become, however, so soon contaminated with chlorine in nearly all situations, that it suffices, in general, to throw any rusty piece of them, such as an old, dirty piece of thin brass or copper wire, among the glowing coals of a bright fire, to produce this peculiar-spectrumed blue flame in the hottest crevices of the fuel.

The nature of this flame, since it differs very materially, by the simplicity of its spectrum, from the ordinary one of chloride of copper, although in the strong point of line-positions there is a partial feature of similitude in the spectra of the two flames by which they agree very nearly with each other, remained a mystery to me for several years; but about four years ago I chanced by good fortune to hit upon a compound, in some experiments on subchloride of copper, which yielded in a flame, at least a successful imitation, if not, as seems most probable, the really natural and perfectly exact reproduction of it. Copper subchloride is easily obtained by evaporating hydrochloric acid to dryness in an open dish on an excess of wire clippings or other small fragments of metallic copper. It is a dark greenish-brown powder, which easily deliquesces, and by absorbing oxygen, if exposed to the air, is soon converted into the green chloride of copper. For the spectroscopic purpose it should be dissolved when first formed, and dry, in about its own weight of hot glycerine, and the solution be allowed to cool in a well-corked bottle. This pasty solution inflames, when heated on a wire, and burns with the peculiar-spectrumed violet-blue flame which is observable in common fires when contaminations of copper by chlorine are introduced among the fuel, in its hottest parts. Although these contaminations in the state of exposure to common air probably all consist of ordinary chloride of copper, yet among the interstices of the fire, by the presence of hot fuel and great abundance of carbonic oxide, they doubtless undergo reduction to subchloride, and, in place of the many-lined and banded green-flaming spectrum of ordinary copper-chloride, the far simpler and symmetrically grouped one of three line-pairs—green, blue, and indigo—belonging to subchloride of copper vapour presents itself in the fine blue tint which the fire's flames assume, one may suppose, by chloride's reduction to subchloride, and by the infinitesimal admixture in them of this latter foreign substance. The varieties of tint, from blue below to green above, which a Bunsen-flame exhibits when chloride of copper is introduced into it, are probably due to the same chemical conversion, in dependence on the reducing or oxidizing constitution of the flame in its inner and outer layers, which most purely exhibit the two different colorations.

To produce the subchloride of copper spectrum very purely, the thinnest possible smear of its pasty solution in glycerine, on one side of a narrow strip of paper, suffices very amply, since its colouring effect upon the flame, when the strip is rolled up into a spill and lighted, is very powerful. Chlorate of potash powder, kneaded up with the glycerine solution, burns also self-supportingly with the characteristic rich blue colour, but the spectrum in this case, and also when the paper stain of the glycerine solution is left long exposed to air upon the strip

Lankester's attention to this passage, and as it appears evident that he has not referred to my original letters in NATURE, I conclude that he does not know how completely I there recorded my obligation to the article by Darwin which really first did engender the doctrine of panmixia. But, be this as it may, the following is what I wrote:—

"In a former communication I promised to advance what seemed to me a probable cause—additional to those already known—of the reduction of useless structures. As before stated, it was suggested to me by the penetrating theory proposed by Mr. Darwin, to which, indeed, it is but a supplement" (1874).

Again, in 1887, while anticipating and greatly extending Prof. Lankester's present criticism touching Mr. Spencer's attitude with respect to panmixia, I said:—

"The leading idea in Mr. Darwin's suggestion was that impoverished conditions of life would accentuate the principle of economy of nutrition, and so assist in the reduction of useless structures by free intercrossing. Now, in this idea, that of the cessation of selection was really implied; but neither in his own article, nor in a subsequent letter by Mr. George Darwin on the same subject (NATURE, October 16, 1873), was it exhibited as an independent principle. It was inarticulately wrapped up with the much less significant principle of impoverished nutrition."

The simple history of the matter, therefore, is as follows. Even up to the time of publishing his article in NATURE, Mr. Darwin had not perceived the principle of panmixia as an "independent principle"—any more than Dr. Dohrn perceived it in 1875, or Prof. Lankester perceived it in 1880,—which must act in all cases of degeneration, whether with or without the co-operation of reversed selection in the economy of growth, "impoverished conditions," &c. Therefore, in the sixth edition of the "Origin of Species," after having explained the phenomena of degeneration by the inherited effects of disuse, combined with the economy of growth, he proceeds to give very good reasons for concluding that "some additional explanation is here requisite which I cannot give"; and he suggests that, "*if it could be proved* that every part of the organization tends to vary in a greater degree towards diminution than towards augmentation of size, *then we should be able to understand* how an organ which has become useless would be rendered, *independently of the effects of disuse*, rudimentary," &c. But although he thus saw the "explanation" that was "requisite," he said he was unable to give it; therefore at that time he could not have seen that the cessation of selection was exactly the explanation of which he was in search—to wit, a principle which must always make every unused part of the organization tend to degenerate. Later on, however, it occurred to him that "impoverished conditions," *combined with intercrossing*, might lead to this result. But, although he thus came to such close quarters with the idea of panmixia that he immediately suggested it to me on reading his exposition, the idea was still entangled with that of "impoverished conditions" being required in order to *starve* the degenerating parts. Therefore, the only hand that I had in the matter was to liberate the all-important principle of panmixia from the toils of this entanglement, and thus to show that it must necessarily act in the case of *all* unused structures, with the result of destroying the evidence of "the effects of disuse."

Such is a simple history of the facts; and my only object in previously alluding to the part which I had played in the matter was not that of claiming priority touching so very obvious an "idea," but in order to show how it was that Mr. Darwin, through all the editions of the "Origin of Species," continued to attribute important weight to a line of evidence in favour of the inherited effects of disuse, which the doctrine of panmixia, *and the doctrine of panmixia alone*, has entirely destroyed.

of paper, is apt to lose its purity and acquire confusing lines and bands of ordinary copper-chloride, by oxidation, which the preparation then undergoes spontaneously, before igniting it. For pyrotechnists, therefore, it seems scarcely probable that the subchloride of copper, with its pure cerulean flame, will ever be of any very useful value. But as a parallel example of a coloured-fire composition, it may be mentioned here, that powdered Val-Traversite (a bituminous limestone found near Neuchatel, in Switzerland), on account of its prodigious natural richness in bitumen, when mixed with sufficient chlorate of potash, also burns self-supportingly, with a fine orange-red flame in which the familiar spectrum of calcic oxide is, of course, most vivid. Were hot asphalt, pitch, or bitumen, instead of hot glycerine, used to dissolve or to "masticate" the dry subchloride of copper when it is freshly made, a copper-chlorinated mass would be produced which would probably be capable of resisting atmospheric action, and whose mixture with chlorate of potash would, like the similar Val-Traversite mixture, probably also not suffer by keeping and exposure, and would furnish a source of blue flame and of the significantly simple spectrum of subchloride of copper, not less vividly true and fixed in their distinctness, than the orange-red light and strongly pronounced calcic-oxide spectrum of the other combination of chlorate of potash with a bitumen-containing substance.

As regards the blue salt-flame, whose spectrum in its purity shows no conspicuous lines, or bands of greatest brightness, it can hardly be doubted that the element chlorine, from the positions of its own principal line groups, contributes mainly to produce the blue coloration, at a temperature, in the fire, which is not high enough to dissociate the sodic chloride and liberate sodium vapour, with its tell-tale yellow line, from its chemical union. In the green flame of chloride of copper the colouring groups of lines show a more detailed resemblance than this to the chief colorific lines in the elementary chlorine spectrum,[1] while in copper subchloride's "bluest of blue" flames, the wide green light-bands of copper chloride fade out, leaving the colorific light concentrated almost entirely in three close pairs, or in six bright lines, which, if they do not coincide in place with, are at least not far distant in position from, three chief line-pairs in the ordinary spectrum of chloride of copper. There is much in these resemblances which betokens some kind of continuity of connection with the primary features of the chlorine spectrum itself; the evidences of which, although thus displayed by copper and chlorine in the spectroscope, may perhaps be sensibly regarded as having some near relation of analogy to the appearance of variable chemical combining power under the influence of light, between silver and chlorine, presented in photography. But there is also, undoubtedly, a very marked distinction between the "spectroscopic reactions" of these two different copper chlorides; and, similarly, there are in the apparently mutable photochemical affinity between silver and chlorine in photography, two fairly stable delimitations of its range, in the "subchloride" (or as it has been termed by Mr. Clement Lea, the "photochloride") of silver, and in ordinary silver-chloride. Further discriminations of the copper-chloride spectra in intermediate forms which they seem to comprise transitionally between the two definite ones of the chloride and subchloride, would perhaps extend and strengthen this analogy, and may not impossibly help, at some future time, to explain and illustrate it, if there is any real soundness in it, more fully and completely.

The example of fluoride of calcium is a curious one in spectrum analysis, where sprinkling fluor-spar dust in a Bunsen-flame produces, in addition to the normal calcic oxide spectrum of one orange-red and one green band, a second bright and narrow green one at a distance from the first about equal to that of the red band from it. There are no other distinguishable bands. But if the pair of normal ones is really due to calcium-oxide vapour produced by decomposition in the flame, it is not very easy to conjecture to what other product of decomposition the additional, sharply defined and brilliant, solitary green band can be ascribed. The spectrum of hydrofluosilicic acid gas presents a very gorgeous band-array of violet-blue lines, whose lustrous group is probably indicative of near neighbourhood in place to some bright line concentration in the spectrum of fluorine itself; but if so, the collection of its colorific strength in the single additional green line of the fluor-spar spectrum, seems to imply a freedom from uniformity in fluorine's power of imparting spectral coloration to its compounds, just opposite to the sensible continuity and kinship of spectral clusterings, above described, which the presence of chlorine appears to impose upon its compounds by common resemblances discernible in the blue light-ascendencies of the fire-flames of common salt, chloride and subchloride of copper, when they are spectroscopically analyzed.

A. S. HERSCHEL.

Observatory House, Slough, March 3.

[1] A very suggestive example of a substance's detection by recognition of its spectrum was described, with a drawing of the recorded spectra, by Mr. A. Percy Smith, in a short notice of a series of observations on the spectra of chlorides, and on the blue flame of common salt, in the *Chemical News*, vol. 39, p. 141 (1879). An examination of the flame-spectra of several different chlorides, enabled the author of that notice to recognize a common similarity among them all to the spark- or flame-spectrum of hydrochloric acid gas. This gas showed a belt of green line-bands which agreed in their main positions with the green portion of a long array of band-pairs shown with much constancy by several different alkaline and earthy chlorides, and especially by ammonium chloride, and by mercurous chloride (or calomel, where the agreement was also verified by a direct comparison), in a Bunsen flame; but no line-counterparts to the equally bright, blue-lined portion of the same constant spectral striation were observable in the hydrochloric acid spectrum.

From the easy conversion of chlorides into the corresponding oxides in an air-gas flame, when the flame is not kept artificially saturated with hydrochloric acid gas, we might pretty certainly assume that in the flame's ordinary condition, the heated chlorides would always disengage sufficient chlorine to produce by combination with hydrogen in the coal-gas of the flame, traces of the stable product, hydrochloric acid gas, among the gases of the flame's combustion; and the different chlorides would thus, by suppositions which may not perhaps be unlikely and inadmissible, all supply the flame alike with the substantial factor needed, for the appearance of the green line portion of the constant spectrum.

At the same time new carbon-compounds would be formed by dehydration of the flame's gaseous hydrocarbons, to furnish hydrogen to the liberated chlorine, and some constant carbon-gases then, of not yet known descriptions, might be conjectured just as comprehensibly and fitly, to be concurrently productive in the constant chloride-rank's illumination, of the blue-line portion of its bands, of which no spectral counterparts could be detected in the hydrochloric acid spectrum.

But whether the interesting figure and description given by Mr. A. Percy Smith in the above paper, of his long series of experiments, may or may not admit of such a simple spectro-chemical interpretation, the conflicts of contending chemical affinities of which the spectroscopic recognition of hydrochloric acid in flames fed with different chlorides furnishes such a wonderful example, give weight and value to the notes of the discovery recorded by Mr. A. Percy Smith, in a new wide field of the spectroscope's utility, which are of much deeper interest than any single theory to account only for this particular recognition and discovery itself.

Mr. A. Percy Smith's own capitally based, and clearly proved deductions from his numerous experiments, were accordingly, in prospect of their further prosecution, expressed thus, quite generally:—that the blue flame of common salt in a hot fire owes its coloration to reactions either exactly or very nearly similar to those which produce resemblance of a nearly constant spectral type in different chloride flames, to that of hydrochloric acid; and that, again, among the partly undetermined, and perhaps to some extent variable reactions which produce the similarity, there appear to be some which disturb and modify the ordinary appearance of the hydrochloric acid spectrum, and which would appear to superadd to it a series of blue line-bands which, as it is presented in a flame, or electrically in vacuum tubes, the spectrum of pure hydrochloric acid gas alone does not usually exhibit.

Brush-Turkeys on the Smaller Islands North of Celebes.

THE reviewer of Dr. Hickson's book, "A Naturalist in North Celebes" (March 20, p. 458), believes that the brush-turkey or moleo, *Megacephalon maleo*, has never been recorded as occurring in the smaller islands north of Celebes. I beg to remark that in the year 1879 I recorded this species from Siao, and in the year 1884 from Great Sangi, on both of which islands, besides, occurs a *Megapodius* peculiar to them, viz. *M. sanghirensis*, Schlegel, representing there *M. gilberti*, Gray, from Celebes (see the *Ibis*, 1879, p. 139; *Isis*, 1884, pp. 6 and 53, &c.). Perhaps Mr. Guillemard did not comprise Siao and Great Sangi under the head of "smaller islands," but Dr. Hickson himself (p. 95) records *two* brush-turkeys from the smaller island of Tagulandang, a larger and a smaller one, and these must be *Megacephalon maleo* and a *Megapodius*. Tagulandang is situated between Celebes and Siao, and much nearer to the latter island. From the volcano islet of Ruang, opposite and within about a mile from Tagulandang, he only records (p. 41) one brush-turkey, and this, of course, may be either the *Megacephalon* or a *Megapodius*, if both do not occur, as appears rather probable. When I visited Ruang in 1871 after the heavy eruption in March of that year (see NATURE, vol. iv. p. 286), nearly the whole of its forest was destroyed and burnt down, and I do not believe that a living brush-turkey then remained on the islet; but it has since been repeopled from its near neighbour, Tagulandang, where both species occur, and therefore, if the one could reach Ruang, the other may have reached it too. This is of no consequence at all. Dr. Hickson's following remark as to brush-turkeys on Tagulandang (p. 95), "The larger bird is perhaps the *Megapodius sanghirensis* of Schlegel, a brush turkey, which is bigger than the *Megacephalon*, and extends over the Sangir Islands," contains a mistake, as *M. sanghirensis* is much smaller than

Megacephalon maleo. The reviewer corrects, by the way, my calling the Celebean whimbrel *Numenius phæopus*, saying that it is probably *N. uropygialis*, but these two names are synonymical, cf. for instance, Salvadori, *Orn. Pap.*, iii., 332, 1882, *sub N. variegatus*. As to its nesting on small trees "small brushes" were intended to be implied (see Legge, "Birds of Ceylon," 1880, p. 913). A. B. MEYER.

Royal Zoological Museum, Dresden, March 22.

Crystals of Lime.

IT was pointed out to me by Mr. W. J. Pope, of the City and Guilds of London Institute, that a lime cylinder which had been used in the lantern during a lecture had become distinctly crystalline where affected by the oxyhydrogen flame.

Examined under the microscope by polarized light, the crystals are seen to be well-defined cubes with striated faces. When immersed in water they break up and give rise to minute doubly refracting plates of rhombic outline, behaving in this respect like ordinary lime; the cubic crystals, however, are less rapidly affected by exposure either to air or water than is amorphous lime.

Lime is commonly stated to be infusible at the temperature of the oxyhydrogen blow-pipe; and the only crystals previously recorded, so far as I know, are those obtained by Brügelmann, by fusing calcium nitrate (*Annalen der Physik und Chemie*, ii. p. 466, iv. p. 277, 1877–78). It seems, therefore, worthy of notice that they are possibly always formed upon the surface of the lime cylinders by the action of the oxyhydrogen flame.

The crystals resemble in all respects those described by Brügelmann. The jet used on the present occasion was an ordinary blow-through jet. H. A. MIERS.

Foreign Substances attached to Crabs.

I AM glad to see that Mr. Garstang agrees with me in regarding the presence of the Ascidians on *Hyas* as accidental.

I had no intention of decrying the value of Mr. Garstang's experiments with Ascidians, but his rule might, perhaps, be limited to those members of the group to which it can be proved to apply. Under natural conditions it apparently fails to apply to *P. corrugata* and *M. arenosa*. As to the latter, Prof. McIntosh assures me that he has frequently found it in the stomach of the cod and haddock.

The appreciation of the cod for *A. mesembryanthemum* is, I think, sufficiently proved by the fact that the latter is one of the most successful cod-baits used here.

ERNEST W. L. HOLT.

St. Andrews Marine Laboratory, March 29.

Wimshurst Machine and Hertz's Vibrator.

IT may interest those who wish to repeat Hertz's experiments on electro-magnetic radiation to know that many of these can be done very well by using a small Wimshurst machine in place of the usual induction coil and battery. The vibrator and resonator which we used were like those described in NATURE (vol. xxxix. p. 548), and the Wimshurst machine had two 12-inch plates (giving at most with the jars on a 4-inch spark). The wires from the vibrator, instead of being connected with an induction coil, were connected with the two outer coatings of the jars of the machine. The machine spark-gap and the vibrator spark-gap should be so adjusted that when a spark occurs at the former one also occurs at the latter. With the apparatus described we got good results when the spark-gaps were 38 mm. and 3 mm. respectively. The outer coatings of the jars are only connected together by the wood of the machine, but it is sometimes an advantage to put a few inches of damp string between the balls of the vibrator.

This combination is obviously a modification, adapted to work a Hertz vibrator, of one of Dr. Lodge's well-known Leyden jar arrangements.

No doubt many persons have connected the vibrator directly with the terminals of the machine, but this arrangement does not work nearly so well. T. A. GARRETT.
W. LUCAS.

THE INSTITUTION OF NAVAL ARCHITECTS.

THE annual meeting of the Institution of Naval Architects was held under the presidency of Lord Ravensworth, on Wednesday, Thursday, and Friday of last week. There was a fair list of papers on the programme, although at one time, shortly before the meeting, it was feared that there would be a sad lack of contributions from members. At the last minute, however, one or two papers came in, and the list, although perhaps below the average in the importance of the memoirs, was of passable interest.

The following is a consecutive enumeration of the business that was transacted at the meeting:—

Wednesday, March 26th: morning sitting—Annual Report of the Council, and other routine business; Address by the President. Paper read and discussed—Notes on the recent naval manœuvres, by Mr. W. H. White, F.R.S., Director of Naval Construction.

Thursday, March 27th: morning sitting—The Maritime Conference, by Rear-Admiral P. H. Colomb: strength of ships, with special reference to distribution of shearing stress over transverse section, by Prof. P. Jenkins; steatite as a pigment for anti-corrosive paints, by Mr. F. C. Goodall. Evening sitting—On the evaporative efficiency of boilers, by Mr. C. E. Stromeyer; on the application of a system of combined steam and hydraulic machinery to the loading, discharging, and steering of steam-ships, by Mr. A. B. Brown; the revolving engine applied on ship-board, by Mr. Arthur Rigg.

Friday, March 28th: morning sitting—On leak stopping in steel ships, by Captain C. C. Penrose Fitzgerald, R.N.; on the variation of stresses on vessels at sea due to wave motion, by Mr. T. C. Read; spontaneous combustion in coal ships, by Prof. Vivian Lewes. Evening sitting—Experiments with life-boat models, by Mr. J. Corbett; on the screw propeller, by Mr. James Howden.

The annual dinner was held on the evening of Wednesday.

Out of the above list of a dozen papers there were fewer than usual of scientific interest, and, indeed, in one or two instances they were not either distinguished by practical interest. Mr. White's paper, which formed the *pièce de resistance* of the meeting, was of military rather than scientific importance, and was chiefly notable from the number of admirals that took part in the discussion; indeed, the whole naval contingent of the Board of Admiralty was present to hear the paper read. Admiral Colomb's paper on the recent Washington Maritime Conference was practically reduced to a consideration of the rule of the road at sea. The general opinion of the authorities assembled appeared to be that the present rule of the road is very well as it stands, with the exception that the "holding-on ship" should not be required, or even allowed, to slacken her speed. This seems in conformity with common-sense. If two ships are converging towards a point, say at right angles to each other, and one shifts her helm to go under the other's stern, if the second, or holding-on ship, slacken speed, the probability will be that the giving-way ship will crash into the other's broadside or cross her bows; in the latter case, there is probability that the holding-on ship will give the other her stem. What is most wanted when danger of collision arises, is certainty on each vessel as to what the other may be going to do. If the holding-on ship never slacken speed—is not allowed to slacken speed—then the other vessel knows exactly what course to take; as the law stands, the quartermaster, or officer in charge, is never quite sure until the last minute, especially at night, whether the other ship considers there is danger of collision or not, and, therefore, whether she will slacken or keep to full speed. We anticipate the proposed alteration, if put in force, will greatly lessen the list of collisions.

The memoir contributed by Prof. Jenkins on the strength of ships was decidedly the most important contribution to naval science of this year's meeting. The paper will open up to the majority of those practi-

cally engaged in the design of ships a new field of research, the investigation of which will enable them to solve some problems which have hitherto been without explanation. That is, speaking generally—for the influence of longitudinal bending moment on shearing stress has before been investigated by naval architects; notably by Mr. W. H. White, the Director of Naval Construction, and Mr. W. John. This, however, was many years ago, and in connection with wooden ships with no longitudinal connection between the planking except that supplied by dowells, the friction of the edges, and the "anchor-stock" shape of the pieces. It will be evident, therefore, that previous investigations must have been of a qualitative, rather than of a quantitative, form; and the world of naval architecture is much indebted to the occupant of the John Elder Chair at Glasgow for putting the problem on a practical quantitative basis.

The paper contributed by Mr. C. E. Stromeyer had a most attractive title, "The Evaporative Efficiency of Boilers"; and a good many of the working marine engineer members of the Institution, who were acquainted with the thorough manner in which the author follows up all his work, had assembled to hear the paper read, and take part in the discussion. We are afraid it must have been somewhat of a disappointment to several of these gentlemen when they turned over the leaves of the paper as it was placed in their hands, and found that the matter was rather of a suggestive than of a conclusive character. There is so much business to be crowded into the three days' annual meeting of this Institution that it is necessary the papers should be read with despatch; and we quite sympathize with the engineer whose daily task is of an administrative rather than a contemplative nature, when he is asked to assimilate at a galloping pace two or three pages of mathematical formulæ of by no means an every-day character.

Mr. Stromeyer confined himself chiefly to a consideration of the relative distribution of efficiency in the tubes. He points out that the distribution is governed partly by the temperatures in the combustion-chamber and smoke box, and partly by the resistance of gas in the tubes, and this again depends upon the velocity and temperature of the gas, and on the loss of heat experienced by it. Mr. Longridge has found that the coefficient of transmission of heat through boiler-tubes or combustion-chamber plates is eleven calories of heat per square foot per hour for every degree F. of difference between the gas and the water: 0·091 is the reciprocal value, and is the resistance offered to the flow of heat under the above condition. This resistance is offered when heat passes from one medium to another, as, for instance, from the gas to the metal, from the metal to the boiler scale, or to the water, and it also includes the resistance offered by the metal to the scale. For iron and boiler scale the resistances are 0·00202 and 0·207 per inch thickness; so that a clean ½-inch plate would offer 0·001 resistance; or, if covered with scale one-tenth inch thick, the resistance would be 0·001 + 0·021 = 0·022.

Arguing from these facts the author concludes that the chief resistance, about 80 per cent., is encountered at the surfaces; and he doubts whether the change of medium from iron to scale, and to water, influences the values very much. The chief difficulty in transmitting heat from the gas to the tubes is want of circulation, or admixture of gas in the tubes. He speaks favourably of draught retarders, corrugated tubes, and ribbed-tubes for the purpose.

Mr. Stromeyer next refers to the experiments of Haverez (see *Ann. du Génie Civil*, 1874), by whom it was shown that more heat is absorbed in the fire-box with flaming material than with flameless coke. It is well known that a luminous flame radiates more heat than one which is non-luminous; and it is for this reason that the latter may not be used in the Siemens-Martin furnace. For reasons given, Mr. Stromeyer would prefer that, in the formulæ used by Mr. Longridge for heating boiler tubes, the coefficient of resistance $\frac{1}{m}$ should be somewhat increased; say from 0·091 to 0·1. This the author works out in detail. We have stripped Mr. Stromeyer's arguments of their mathematical aspect, as, however interesting the matter may be, we have not space to do it justice. We must refer those of our readers who are sufficiently interested in the subject to the Transactions of the Institution.

Mr. Macfarlane Gray, of the Board of Trade, was the chief speaker in the discussion which followed. He said he could not pretend at one reading to follow the author in all his reasoning. Mr. Fothergill, who is the superintending engineer to a north country line of steamers, gave the meeting the benefit of his practical knowledge upon the subject. Mr. Fothergill is well qualified to speak on the question of the evaporative efficiency of marine boilers, as he has made an especial study of the matter in the actual working of vessels in connection with his well-known researches on the subject of forced draught on ship-board.

Mr. Brown's paper was one of unusual interest to the members of the Institution. In it he described the most recent development of that beautiful system by which he has so vastly improved the loading and discharging of cargo on steam-ships, and the steering of vessels. The paper was illustrated by several diagrams without the aid of which it would be impossible to make clear the details of the very ingenious methods by which the author has applied his combined steam and hydraulic practice to the purposes named. Briefly stated, it may be said that, in place of the usual deck winches, there is placed at every hatch a derrick, having mounted upon it the hydraulic cylinder which supplies the motive power to lift the goods. The steering motor is placed directly on the quadrant of the tiller, and is actuated from the bridge by means of what the author describes as a telemotor. The transmission of the controlling force which governs the steering motor is through hydraulic pipes; a vast improvement on the rattling chains and rods now in common use. In fact the great virtue of Mr. Brown's system is its quiet working.

Mr. A. Rigg's revolving engine is an ingenious device, perhaps better suited to water than steam. It was fully described in Section G at the last Birmingham meeting of the British Association.

"Leak Stopping in Steel Ships" was the somewhat misleading title of a rather weak paper by Captain Fitzgerald. The only point the author suggested was that war-ships should be outside sheathed with wood in order that there might be some attachment to which leak stoppers could be affixed. The contention that the swelling of wood by moisture that takes place, or used to take place, when a shot cut through the side of an old man-of-war is quite beside the mark, as we suppose no one proposes to make the wood sheathing of a modern steel steamer as thick as the sides of our old wooden walls. Three or four inches of elm would do very little swelling when pierced by a modern projectile of any considerable size.

Mr. T. C. Read's paper on the variation of stresses at sea is another of those contributions which are the despair of the practical naval architect, not over-given to abstruse science, who attends the meetings of his Institution, hoping to take part in the discussions. We are quite at one with the speaker, Mr. Alexander Taylor, who proposed that a rule should be passed compelling contributors to send in their papers sufficiently early for them to be printed and distributed to members before the meetings. The executive say it cannot be done, but it would be worth trying for a time.

Prof. Lewes's paper on the ignition of coal cargoes was quite a new departure in the practice of the Institution. When the members assembled they found an array of bottles, flasks, and chemical apparatus, that was not a little puzzling to those not in the secret, and must have reminded many of the dear old Polytechnic days and Prof. Pepper. However, the lecture, and the experiments by which it was illustrated, were of a thoroughly sound and practical nature. The question of spontaneous ignition of coal cargoes is one for the ship-owner rather than the shipbuilder; excepting that ship-builders have to replace the vessels which are destroyed by reason of such spontaneous ignition. The lecturer illustrated the influence of carbon in producing heating by the power it possesses of attracting and condensing gases upon its surface. The action of the bituminous constituents of the coal in spontaneous ignition was next dealt with, and the author then proceeded to point out the important part the action of iron disulphide, pyrites, or coal-brasses played in promoting spontaneous ignition. The remedy Prof. Lewes advises for the evils of spontaneous ignition are: firstly, non-ventilation of holds, so that oxygen may not be admitted to carry on the chemical processes by which heat is generated; secondly, by placing thermometers, suitably protected, in the mass of coal, so that, by electrical communication, warning may be given when the temperature rises to a dangerous point; and, thirdly, by placing flasks of liquid carbonic anhydride in the coals, the flasks to be sealed by an alloy with a low melting-point. This would be fused when the dangerous temperature was reached, and the carbonic acid, in expanding to its gaseous state, would cool the mass of coal to a safe temperature.

At the last sitting of the meeting, Mr. Corbett's paper on lifeboat models raised a lively controversy. The Royal National Lifeboat Institution had brought Mr. G. L. Watson all the way from Glasgow to meet the bold innovator who proposed to abolish their cherished self-righting boats. Of course, who is right remained an open question, as it always does when the properties of lifeboats are concerned.

Mr. Howden's paper on the screw propeller was of great length, containing no less than twenty-four pages without the appendix. Mr. Howden, like many other people, has a theory of his own on the screw propeller, which is opposed to that of all other authorities on the subject; for he believes that Rankine, Froude, Cotterill, and others, have based their conclusions on erroneous premises. It will be evident that we cannot enter into this vast subject at the end of a notice such as this, but we may briefly record our opinion that the older authorities were right.

On the whole, the meeting passed off very well. The attendance was good, and Mr. Holmes, the secretary, had made his arrangements so that the business proceeded without a hitch, as, indeed, is invariably the case at this well-managed institution.

BOURDON'S PRESSURE GAUGE.

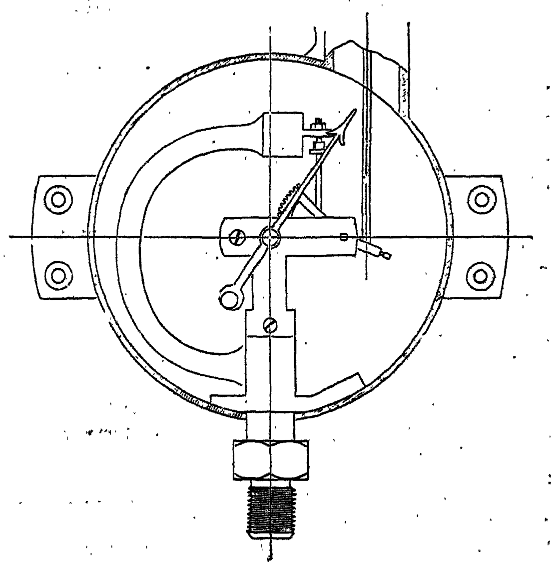
MR. WORTHINGTON'S letter to NATURE, January 30 (p. 296), on the theory of this instrument, has excited some criticism and disagreement of opinion; so it is proposed to examine here how far it is possible to construct a theory which shall be quantitative, in addition to giving a general explanation of the action.

The instrument is in very extensive use, hardly a steam-boiler being in existence which is not provided with one; and the simplicity and strength of the construction are such that it does not easily get out of repair, while it can be made to register either the highest pressure of the hydraulic press, or to record in the form of a barometer the minute fluctuations of atmospheric pressure.

The principle of the instrument was discovered by accident, and the account of this had best be given in the inventor's own words, taken from the paper read by him before the Institution of Civil Engineers, printed in the Proceedings I.C.E., vol. xi., p. 14, 1851:—

"The author had occasion to construct a worm-pipe for a still, by bending a cylindrical tube into a spiral or helical form. The workman performed the operation awkwardly, and partially flattened a considerable portion of the tube. In order to restore its form, one end was closed and the other was connected with a force-pump, by which water was forced into the tube; as the flattened portion of the tube resumed its cylindrical form, it was observed that the spiral uncoiled itself to a certain extent, and it was immediately perceived that this action might be applied to the construction of a pressure gauge."

To construct, then, a Bourdon gauge to register high pressures (*vide* figure, representing a gauge fitted to an indicator, not shown) a steel tube bored out of the solid bar to the requisite thickness for strength is taken, and purposely flattened, and then bent round into the arc of a circle so that the longer axis of a cross-section stands at right angles to the plane of the circle: one end of the



tube is screwed to a pipe which communicates with the liquid whose pressure is to be measured, while the other end is closed and joined by levers and racks to a shaft and a pointer, which traverses a dial on a box in which the curved tube is enclosed.

As the pressure in the tube is increased, the circular axis uncoils into a larger circle of smaller curvature, and the corresponding indications of the pointer on the dial are marked; and thus the instrument is graduated empirically by reference to some standard pressure gauge. As the pressure is again diminished, the elasticity of the tube brings it back to its original form, and the pointer retraverses the dial.

Lord Rayleigh gives an elementary explanation of the action of Bourdon's gauge in the Proc. Royal Society, No. 274, December 13, 1888; treating the movement of the walls of the tube as one of pure bending, he says:—

"In this instrument there is a tube whose axis lies along an arc of a circle and whose section is elliptical, the longer axis of the ellipse being perpendicular to the general plane of the tube. If we now consider the curvature at points which lie upon the axial section, we learn from Gauss's theorem (that in the bending without stretching of an inextensible surface, the

product of the principal radii of curvature of the surface at any point remains constant) that a diminished curvature along the axis will be accompanied by a nearer approach to a circular section, and reciprocally. Since a circular form has the largest area for a given perimeter, internal pressure tends to diminish the eccentricity of the elliptic section, and with it the general curvature of the tube. Thus, if one end be fixed, a pointer connected with the free end may be made to indicate the internal pressure." Lord Rayleigh adds, "It appears, however, that the bending of a curved tube of elliptical action cannot be pure (*i.e.* unaccompanied by stretching), since the parts of the walls which lie furthest from the circular axis are necessarily stretched. The difficulty thus arising may be obviated by replacing the two halves of the ellipse, which lie on either side of the major axis, by two symmetrical curves which meet on the major axis at a *finite angle*."

In fact some Bourdon gauges, notably those required for low pressures only, and requiring great sensibility but not much strength, are constructed in this manner, and the difficulty of manufacture is thereby considerably reduced. Barometers are constructed in this way, and give good results; the tube is partially exhausted of air, and closed at both ends; and now an increase of external atmospheric pressure tends to flatten, and thus curl up the tube.

In constructing any theory, we are then immediately brought up by the great difficulty at present engaging the attention of our mathematical elasticians, such as Rayleigh, Basset, Pearson, and Love; who are not agreed as to how far it is legitimate to theorize on the equilibrium of elastic shells, by treating separately the bending and the stretching as independent of each other, and considering the first—the bending—of the most importance.

If we take a piece of thin sheet metal in our hands, we find we can bend it with comparative ease, but any stretching we can produce is quite insensible; and it is thence argued that bending only is likely to take place, as so easily produced; and apparently reversing the ordinary mathematical procedure, the large stresses due to any stretching are neglected, as not likely to be in existence. These difficulties confront us in any attempt at a rigorous theory of the instrument, which would give quantitative results, enabling us to graduate the instrument from a formula.

The Rev. E. Hill has given in the *Messenger of Mathematics*, vol. i., 1872, an explanation of the Bourdon metallic barometer, treating the question as one of pure bending, and giving a quantitative formula for the change of curvature a of the total curvature θ in terms of the change x in the semi-minor axis b, viz. $a/\theta = x/b$. But the determination of x/b for a given change of pressure is as yet an intractable mathematical problem, even for the simplification of supposing the tube a straight elliptic cylinder.

When we attempt to determine mathematically the pure bending produced in an elliptic cylinder by an increase of internal pressure and consequent tendency of the cross-section to the circular form, we are baffled by the analytical difficulties of determining the change in the length of the axes of the section, subject to the condition of keeping the perimeter unchanged in length, this length being expressed by a complete elliptic integral of the second kind, of which the modulus is the eccentricity of the ellipse. This problem was mentioned by Sir W. Thomson at the British Association in 1888; but we have not yet seen any development of it published by him.

Mr. Worthington, on the other hand, treats the question from the point of view of pure stretching; and now, with rectangular cross-section of the tube, as he supposes, a thrust in the inner wall due to the internal pressure will cause this wall to contract, while the pull in the outer wall will cause this wall to elongate; and thus an increase of internal pressure would cause the tube to curl up, the opposite effect to what happens when the bending effect due to the outward bulging of the flat walls is considered the leading phenomenon.

Even with a circular cross-section the stretching hypothesis would prove that the tube curls up under internal pressure; but this effect would be so small as to be imperceptible, because of the enormously greater stresses required for stretching than for bending in a thin tube; and this is found to be practically the case, inasmuch as the circular cross-section of the tube destroys all indications; and further, that the indications of the tube are reversed in direction when the axes of the elliptical cross-section are interchanged so that the minor axis is perpendicular to the plane of the circular axis of the tube.

The action of Bourdon's gauge is a differential effect; the bending of the surface changes the curvature one way, and the stretching produced by the same pressure the other way; but the bending effect is so much greater than that of stretching, that the latter may be left out of account.

In Gunnery we have, in a similar manner, two antagonistic causes producing a tendency for an elongated rifled projectile to deviate from a vertical plane of motion. If fired from a gun rifled with a right-handed screw, the vortex set up in the air by the spinning of the projectile causes differences of pressure, tending to deviate the projectile to the left, and this effect is sometimes very noticeable with golf or tennis balls; but, in addition, the forces set up by the tendency of the projectile to fly with its axis in the tangent of the trajectory urge the projectile to the right, and these latter forces are found to preponderate in practice.

A mathematician might be tempted to apply to the problem of Bourdon's gauge the formulas on the equilibrium of elastic plates and their change of curvature, anticlastic and synclastic, which are given in Thomson and Tait's "Natural Philosophy" (§§ 711–720), but these formulas apply only to a plate originally plane; and, besides, the applied pressures of the liquid complicate the analysis of the question to an extent which has not yet been overcome by elasticians.

The final conclusion would thus appear to be, that any quantitative formula cannot be hoped for yet, for a long time; but that Lord Rayleigh's reasoning, quoted above, gives a clear and concise descriptive explanation of the action.

The analogous practical problem of the resistance of flues to collapse still stands in need of a rational theory, when the supporting influence of the ends or of collapse rings is taken into account. When this question has received satisfactory treatment at the hands of theorists, we may hope to pass on to the far more difficult quantitative theory of Bourdon's gauge.

A. G. GREENHILL.

NOTES.

THE half-yearly general meeting of the Scottish Meteorological Society was held in the hall of the Royal Scottish Society of Arts, Edinburgh, on Monday afternoon. The following papers were read:—Influenza and weather, with special reference to the recent epidemic, by Sir Arthur Mitchell and Dr. Buchan; the temperature of the high and low-level Observatories of Ben Nevis, by T. Omond, Superintendent; thunderstorms at the Ben Nevis Observatory, by R. C. Mossmann. In the last Report presented by the Council, reference was made to a proposed systematic observation of the numbers of dust-particles in the atmosphere with the instrument recently invented by Mr. John Aitken, and an opinion was

expressed that, for many reasons, Ben Nevis Observatory was the place where such observations could be most satisfactorily conducted. From the Report presented on Monday, we learn that a grant of £50 has been obtained from the Government Research Fund for commencing this novel and important investigation. Two instruments, constructed by Mr. White, of Glasgow, under the direction of Mr. Aitken, have been obtained—one to be placed permanently within the Observatory itself, and the other, a portable instrument, for outdoor observation. Both instruments are now at the Observatory, and the regular work of observation has begun. The Report also states that the delay in completing the buildings of the low-level Observatory at Fort William turned out to be more serious than was contemplated. This has arisen from various causes, chiefly from the great drought in the West Highlands last summer rendering it necessary that the ships conveying the stones for the building from Elgin be sent round the north and west coast instead of through the Caledonian Canal, which for the time was closed for through traffic; and also from the wet, broken weather of the past winter. In about three weeks the Observatory will be completed, and immediately thereafter the Meteorological Council will erect the self-registering instruments which were originally at Armagh, and otherwise supply a complete outfit of instruments for a first-class Meteorological Observatory. An additional observer has been engaged, and the staff of the two Observatories now consists of Mr. Omond, superintendent, and three assistants. By arrangement with the Post Office, direct communication will be opened between the two Observatories. The regular work of recording the continuous observations will be begun in May. The Directors of the Ben Nevis Observatory will thus soon be in a position to put scientific men in possession of two sets of hourly observations of the completest description, one at the top and the other at the foot of the mountain. With these observations, the changes of the conditions of the weather may be followed hour by hour; particularly those great changes, so vital and essential to the advancement of our knowledge of storms, which take place in the lowermost stratum of the atmosphere between the two Observatories. It is within this aërial stratum, of a vertical height of 4406 feet, that the gradual development of many weather changes from hour to hour may be satisfactorily investigated.

THE Chemical Society held its first anniversary dinner at the Hôtel Métropole on Thursday evening last. Among those present were the Presidents of the Royal Society, the Institute of Civil Engineers, the Society of Chemical Industry, the Institute of Chemistry, the Pharmaceutical and the Physical Societies, Sir F. Abel, Sir Henry Roscoe, Sir F. Bramwell, Mr. Thiselton-Dyer, Prof. J. Dewar, Dr. J. H. Gladstone, and Mr. W. Crookes. Dr. W. J. Russell, the President, in proposing posperity to the Chemical Society, sketched briefly the history of its rise and development. Sir Frederick Abel gave the toast of "Kindred Societies and Institutions," referring to the far-reaching character of the science of chemistry. There was not, he said, a single society or institution which was not dependent upon chemists for, at any rate, some amount of the usefulness which it exercised. The Royal Society was the great parent of them all; and the Royal Institution demanded special homage on account of the splendid discoveries made under its auspices, so many of which were specially interesting to chemists. Sir G. Stokes, in response, said that though specialism had been gaining ground very widely of late years, and though each branch of science had its own particular exponents enrolled in their own association, yet the old society, with which he had the honour to be closely connected, was not altogether effete. He thought that chemistry had as much need of cognate societies as any other branch of scientific research. Sir Lowthian Bell also replied. Prof. M. Foster, secretary to the Royal Society, proposed "The Visitors," and the toast was responded to by Sir F. Bramwell and by Mr. Thiselton-Dyer. The health of the chairman was proposed by Sir H. Roscoe.

ON Friday evening last the learned societies of Newcastle held their second annual gathering at the Durham College of Science. Among the societies represented were the following: the Durham College of Science, Engineering Students' Club, Foremen Engineers and Draughtsmen, Geographical Society, Institute of Mining and Mechanical Engineers, Literary and Philosophical Society, Medical Society, Microscopical Society, Natural History Society, N.E.C. Institution of Engineers and Shipbuilders, Pharmaceutical Association, Photographic Association, Society of Antiquaries, and Society of Chemical Industry. The *Newcastle Daily Journal* says that the professors of the Durham College of Science "worked hard for the success of the gathering," and that "the exhibits which they explained in the chemical, physical, geographical, botanical, and other departments in the building, afforded a vast amount of pleasure."

BY permission of the trustees of the British Museum, the *conversazione* of the Society of Arts will be held this year at the Natural History Museum, South Kensington.

MR. WRAGGE, Government Meteorologist, Queensland, has been dangerously ill with fever caught some time since in his tours of inspection. He has now gone to the Darling Downs to recruit his heath, which has been seriously undermined.

THE following lectures on scientific subjects will probably be delivered at the Friday evening meetings at the Royal Institution after Easter:—Friday, April 18, Sir Frederick Bramwell, F.R.S., welding by electricity; Friday, April 25, Sir John Lubbock, Bart., M.P., F.R.S., the shapes of leaves and cotyledons; Friday, May 9, Mr. R. Brudenell Carter, colour-vision and colour-blindness; Friday, May 16, Prof. Raphael Meldola, F.R.S., the photographic image; Friday, May 23, Prof. A. C. Haddon, manners and customs of the Torres Straits islanders; Friday, May 30, A. A. Common, F.R.S., astronomical telescopes; Friday, June 6, Prof. W. Boyd Dawkins, F.R.S., the search for coal in the South of England.

AT the twenty-first annual meeting of the Norfolk and Norwich Naturalists' Society, held at the Norwich Museum on March 25, Mr. Henry Seebohm was elected president for the ensuing year. The treasurer's report showed that the financial condition of the Society was very satisfactory, and that during the past year there had been an increase of several members. The retiring president, Dr. Taylor, after briefly reviewing the work of the Society during the past year, delivered an address on "Microbes."

THE London Geological Field Class, under the direction of Prof. H. G. Seeley, F.R.S., has made arrangements for a number of excursions, in which many students might find it pleasant and profitable to take part. One set of excursions is specially arranged for the practical study of geography. Others are planned for the illustration of the geological structure of the London district.

A VIOLENT earthquake shock was felt at Trieste on March 26 at 20 minutes past 9 p.m.

AT the last meeting of the Scientific Committee of the Royal Horticultural Society, Mr. Morris alluded to the peculiar vegetation of St. Helena, now confined, for the most part, to a small area in the central and higher part of the island. Many of the trees formerly native to the island are now all but, or quite, extinct. Among them is a species of Trochetia, or Melhania. The trunks of this tree are embedded in the cliffs of the island, and are dug out by the inhabitants for the sake of manufacturing ornaments. The following quotation from Melliss's

exhaustive work on St. Helena refers to this plant:—"The Native Ebony of St. Helena.—This plant I believe to be now extinct. It formerly grew on the outer portions of the island, near the coast, at altitudes of 2 to 4, where the weather-beaten stems are still found deeply embedded in the surface-soil. The last plant I saw was a small one growing in the garden at Oakbank, about twenty-five years ago, but it is not there now, and I have searched the whole island over for another, but in vain. The leaves were dark green, and the flowers white; the wood is very hard, heavy, black in colour, and extremely brittle. It is still collected and turned into ornaments, which are much prized on account of its rarity. That this tree once formed a considerable portion of the vegetation clothing the island on those parts that are now quite barren, is strongly evidenced by the many references to it in the local records. Pl. 29. It is the *Dombeya erythroxylon* of Andr., *Bot. Repos.*, vi., t. 389, not of Willdenow." It is interesting to know that the plant is still in existence under cultivation at Kew (and perhaps elsewhere), under the name of *Dombeya erythroxylon*. At the present time the plant, which was obtained from the gardens at Herrhausen, is in flower at Kew. Mr. McLachlan called attention to the interesting remark on the rare plants of St. Helena, contained in Mr. Wollaston's book on the Coleoptera of the Atlantic islands.

CAPT. DELPORTE, Professor of Topography, Astronomy, and Geodesy, at the Military School of Brussels, has just started for the River Congo, for the purpose of making geodetic researches.

THE Geographical Society of Berlin has presented the sum of 1000 marks (£50), to Dr. Hettner for a journey of research in the southern provinces of Brazil.

SOME prehistoric German tombs were recently excavated on the road leading from Apolda to Jena. About 20 skeletons were found (two being without skulls), and a number of ornaments and weapons.

IN the course of some excavations lately made at Ludwigshafen, on the Rhine, the tibia and two teeth of a mammoth, and the jaw of a stag, were found. The skeleton of another "antediluvian" animal was discovered in the limestone near Oberhildesheim. The researches are being continued.

THE *Zoologist* for 1884 announced a proposed supplement to Thompson's "Natural History of Ireland," and contributions of information were invited from persons interested in the subject. A considerable amount of fresh material has been accumulated, but as it relates chiefly to birds, it is now intended that the supplement shall deal only with ornithology. The new work will be published by Messrs. Gurney and Jackson, and an appeal for additional facts has been issued to students who may be able and willing to supply notes. Anyone who is in a position to respond to this appeal is requested to communicate with Mr. R. J. Usher, Cappah, Lismore, Ireland.

MR. ELLIOT STOCK has issued the seventh edition of "Days and Hours in a Garden," by E. V. B. The volume is prettily printed and bound, and lovers of the country will find much to interest them in the writer's bright and pleasant descriptions.

THE Royal University of Ireland has issued its Calendar for the year 1890, and a supplement consisting of the examination papers of 1889.

THE first edition of the life of the Rev. J. G. Wood, by his son, the Rev. Theodore Wood, has been already exhausted; and a second edition is about to be issued.

A FACT noted by Mr. T. H. Hall in the new number of the *Entomologist's Monthly Magazine* indicates the extraordinary variety of conditions in which beetles may thrive. The men employed in breaking up an old disused gasometer at Home Park Mills, King's Langley, spoke to him of some "very curious beetles," which were living in the rusty water at the bottom of the hole left when the iron casing had been removed. Both the water and mud were strongly impregnated with gas. The beetles proved to be of the *D. marginalis* species, and were there in some numbers. Many were carried away when the water was pumped off, but Mr. Hall secured specimens from the mud and shallow water left. He says:—"They carry with them a strong odour of gas, even after two or three fresh-water baths, and the grooves in the elytra of the females are filled with a ferruginous mud which is difficult to remove. In other respects they appear to be quite normal in form and colour. I think this old gas-holder must have been their home for a long period of beetle life, judging from the time of year when they were found, a fortnight ago, and from the number of *both* sexes seen. The water was partly enclosed and quite stagnant, being unconnected with any other water. Were they there by choice? If not, why did they not emigrate? Most likely they came there by chance, as they are plentiful in the canal not far away, and lacking the inclination to depart, 'made themselves at home.' Had the water been disagreeable to them, we may presume they would not have done so; they were quite active when disturbed."

ACCORDING to a French journal, the number of foreign students now studying in Paris is about 1000, of whom 729 (107 of them women) are studying medicine, and 182 law. Literature has 66 (including 9 women), science 60, and pharmacy 23. It is remarkable that Russia furnishes the largest contingent of the foreign medical students, viz. 150, America coming next with 139. We find no mention of England. The foreign element is, on the above estimate, about one-tenth of the whole.

THE Punjab Forest Administration Report for 1888–89 was recently published. During the year, nine thousand acres were added to the area of gazetted forests in the Multan district. This area was taken up in pursuance of the policy of establishing irrigated plantations in connection with several new canals constructed in what are known as the "Bar" tracts—that is, the dry upland deserts of the Punjab. The number of forest fires increased during the year, and 17,617 acres were burnt as against 10,324 during 1888. The financial results are satisfactory. The net revenue amounted to Rs. 4,52,846, or nearly half a lakh in excess of the net revenue of the preceding year. The Conservator complains that the Working Plans Branch cannot get on with their work on account of the undermanning of the Department. As a consequence, working plans are only in force over 364 square miles, out of a total of two thousand square miles gazetted and six thousand controlled by the Forest Department. Experiments with exotics were made, but the result was not encouraging. European fruit-trees have been introduced in many places with great success.

THE first Report published by the Marine Fisheries Society of Great Grimsby is a modest record of work done and investigations decided on by an institution which, by employing scientific methods, will probably amass information of great value to the biologist, and improve our fisheries in their commercial aspects. The Society was incorporated in June 1888. It has already established an aquarium and hatchery which is 37 feet by 21 feet, and a small museum and library. The building has a frontage of 50 feet, and is situated at Cleethorpes, facing the Promenade, two miles distant from Grimsby. The tanks are set on concrete walls; they were purchased from the National Fish-Culture Association, and originally formed the aquarium at the Fisheries Exhibition at South Kensington. They form a reservoir storing 4000 gallons of sea-water, from which the water is

pumped into a wooden tank 10 feet above the hatchery, holding 1200 gallons. Thus a constant circulation of the water in the tanks is maintained. The water is pumped from the sea at high water, and left to settle some days in a storage reservoir before use; each hatching tank has room for twelve wooden trays, measuring 16 inches by 10 inches, by 9 inches in depth, with a canvas strainer at the bottom to prevent the eggs escaping. The Society aims at recording observations respecting marine life, and the improvement of the fisheries of the United Kingdom, by the artificial propagation of marine fishes and Crustacea, by the pursuit of scientific observations and investigations respecting the natural history, habitat, migration, spawning food, and the effect of weather, temperature, and conditions of the water, currents, tides, light, and darkness upon the fauna of the sea; by the protection of young fish, and the introduction of practical appliances for the capture of mature fish; by endeavouring to ascertain the best methods of transporting fish in a fresh condition, and economically preserving them. By admitting fishermen into the Society, at a nominal subscription, they hope to get numerous observers and collectors from amongst those who spend their life reaping the harvest of the sea.

AT the last meeting of the Société Chimique de Paris a paper by M. Meslans was presented by M. Moissan, announcing the isolation of fluoroform, CHF_3, the fluorine analogue of chloroform, $CHCl_3$. A brief abstract of this preliminary communication will be found in the *Chemiker Zeitung* for March 26. During the course of the work recently published concerning propyl and isopropyl fluorides, M. Meslans had occasion to study the action of silver fluoride upon iodoform. The result of this action was found to vary according to the conditions of experiment, liquid products being obtained under certain conditions, and gaseous products under others. The end result, however, was always the production of a gas, which turns out to be fluoroform. Chloroform, as is well known, is readily attacked by a warm alcoholic solution of potash, potassium chloride and potassium formate being produced: $CHCl_3 + 4KOH = H.COOK + 3KCl + 2H_2O$. It is interesting to learn that fluoroform behaves in precisely the same manner, for the gas is decomposed by either aqueous or alcoholic potash with formation of fluoride and formate of potassium. On being heated to redness in a glass tube fluoroform is also decomposed, with production of gaseous silicon tetrafluoride and a deposit of carbon. The gas is only very slightly absorbed by water, but it dissolves readily in chloroform or alcohol. Fluoroform has also been prepared by substituting chloroform or bromoform for the iodoform used in the first experiments.

AT the same meeting M. Chabrié reported that he also had obtained a gas by heating silver fluoride with chloroform in a sealed tube, which yielded potassium formate with potash, and was evidently identical with the fluoroform described by M. Meslans. The density of the gas was determined, and found to be 2·414. Fluoroform possesses the density 2·43, so there can be no doubt as to the identity of the gas. Although so readily attacked by warm potash, it was found that a cold alcoholic solution of potash was almost incapable of acting upon it.

M. MOISSAN also presented another interesting paper in the names of MM. Guenez and Meslans, describing the isolation of fluoral, $CF_3.CHO$, the analogue of chloral, $CCl_3.CHO$, the tri-chlor derivative of common aldehyde, $CH_3.CHO$, and the hydrate of which has recently become so famous as a drug. Fluoral, like fluoroform, is a gas, and has been obtained by heating silver fluoride with anhydrous chloral. The gas dissolves to only a very slight extent in water, but is absorbed by aqueous or alcoholic potash with formation of formate and fluoride of potassium, thus again resembling its chlorine analogue. To complete the proof of its identity, the density of the gas was determined and found to agree very closely with the calculated density of anhydrous fluoral.

THE additions to the Zoological Society's Gardens during the past week include two Ring-necked Pheasants (*Phasianus torquatus* ♂ ♀), British, presented by H.R.H. the Prince of Wales, K.G.; a Chacma Baboon (*Cynocephalus porcarius* ♀) from South Africa, two Indian Pythons (*Python molurus*) from India, five Common Boas (*Boa constrictor*) from South America deposited; three Red-footed Ground Squirrels (*Xerus erythropus*) from West Africa, two Himalayan Monauls (*Lophophorus impeyanus* ♀ ♀) from the Himalayas, two Diuca Finches (*Diuca grisea*), a Black-chinned Siskin (*Chrysomitris barbata*) two Field Saffron Finches (*Sycalis arvensis*), an Alaudine Finch (*Phrygilus alaudinus*) from Chili, purchased; a Hog Deer (*Cervus porcinus* ♂), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at Greenwich at 10 p.m. on April 3 = 10h. 48m. 43s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G.C. 2343	—	Greenish.	11 8 10	+55 36
(2) 44 Leonis	6	Yellowish-red	10 19 27	+ 9 20
(3) 58 Leonis	4·5	Whitish-yellow.	10 54 54	+ 4 13
(4) θ Leonis...	3	White.	11 8 30	+16 2
(5) 145 Schj.	8	Red.	12 19 36	+ 1 23
(6) S Coronæ	Var.	Reddish-yellow.	15 16 55	+31 46

Remarks.

(1) This is the well-known nebula 97 M, near β Ursæ Majoris. In the General Catalogue it is described as "a planetary nebula, very bright, very large, round; at first very gradually, then very suddenly brighter in the middle to a planetary disk; 19·os. in diameter." Lord Rosse's drawing of the nebula indicates a very complex structure. I examined the nebula recently with Prof. Lockyer's 30-inch reflector at Westgate-on-Sea, but was unable to see all the details shown in Lord Rosse's drawing. The nebula appeared to be a large disk, ill-defined at the edges, and equally illuminated, with the exception of two darker disks situated diametrically opposite to each other, each being about half a radius in diameter. Dr. Huggins observed the spectrum in 1866, and found it to consist of bright lines. The two lines near λλ 500 and 495, and possibly a little continuous spectrum were recorded. On the occasion above referred to I saw the three usual nebula lines and the hydrogen line at G, but was unable to continue the observations on account of clouds. In further observations, additional lines ought to be looked for, and the character of the chief line near λ 500 particularly noted, as in the case of the nebula G.C. 2102, given last week.

(2) A star of Group II. Dunér states that the bands 2–8 are well seen, but that they are not strongly marked. It is important to secure further observations of stars like this, as there may very well be other differences besides the weakening of the bands as compared with those in which the banded spectrum is more fully developed.

(3) This has a fine spectrum of the solar type (Vogel). The usual differential observations are required.

(4) The spectrum of this star is a typical one of Group IV. (Vogel). The hydrogen lines are probably therefore very thick, and the metallic lines very thin, if visible at all. The thicker the hydrogen line the hotter the star, and the higher therefore its place on the "temperature curve."

(5) Vogel and Dunér agree in describing the spectrum of this star as a very fine one of Group VI. The three carbon bands are stated to be visible, but the intensity of the band near λ 564 relatively to the others is not given. This point should therefore receive attention. The secondary bands 4 and 5, and possibly 2 and 3 are visible. It is interesting to note that this star shows considerably more detail than several brighter ones of the same group.

(6) This variable will reach a maximum about April 9. Its period is about 360 days, and the magnitudes at maximum and minimum are 6.1–7.8 and 11.9–12.5 respectively (Gore). The spectrum is a very fine one of Group II., and the great range of variation makes it extremely probably that bright lines will appear at maximum or soon after, as already observed by Mr. Espin in variables with similar spectra. Variations in the intensities of the bright carbon flutings should also be noted.

A. FOWLER.

THE GREAT COMET OF 1882.—The *Bulletin Astronomique* for February 1890 reproduces with some additions a paper presented by M. F. Tisserand to the Academy of Sciences on February 3. It will be remembered that the segmentation of the nucleus of this comet was observed on September 30, 1882—that is, thirteen days after perihelion passage, and that Mr. Common in January 1883 saw five nuclei in a line. From an elaborate investigation into the conditions necessary for the development of these secondary nuclei, M. Tisserand concludes that the cause existed in the comet itself, and was not the result of external influence. The minimum relative variation required for the disaggregation of the nucleus is $\frac{1}{100000}$ of the perihelion velocity. And it is suggested that this variation may be produced by interior actions, collisions, mutual attractions, or explosions, because of an excessive increase of temperature or the rotation of the head.

MELBOURNE STAR CATALOGUE.—In 1874 the First Melbourne General Catalogue of 1227 stars for the epoch 1870 was issued. The Second General Catalogue has just been received, and contains 1211 stars for the epoch 1880, deduced from observations made at the Melbourne Observatory under the direction of Mr. Ellery from 1871.0 to 1884.7. The separate results and the details of the observations from which this Catalogue has been compiled are contained in vols. v., vi., and vii., of the *Melbourne Observations*, and in the present Catalogue explanations are given of the processes used in forming the stars' places and the corrections applied. The whole of the observations were reduced and prepared for publication by Mr. E. J. White, the First Assistant Astronomer.

COMET *a* 1890.—The first comet of this year was discovered just before sunrise on March 19 by Mr. Brooks, of Geneva, U.S. Its exact place was found to be—

Cambridge Mean Time.	R.A.	Decl.
	h. m. s.	° ′ ″
21 March ... 16h 57m.5	21 9 34.07	6 25 30 N.

The daily movement in right ascension is + 16s., and in declination + 25′.

DISCOVERY OF ASTEROIDS.—On March 20, Dr. Palisa, at Vienna, discovered another minor planet, and the telegram announcing his discovery was received at the *Astronomische Nachrichten* office at midday on March 21. This comet is of interest, for, from its rapid movement, viz. − 25′ in R.A. and + 10′ in N.P.D., it appears to be near to the earth.

M. Charlois, of Nice Observatory, discovered a minor planet on March 10, and re-observed it on March 20. This brings the number of asteroids up to 290.

The asteroid (288) discovered by Prof. Luther on February 24 has received the name of Glauke.

SOLAR ACTIVITY IN 1889.—The record of the past year as to solar phenomena presents several noteworthy features. (1) The number of days on which the sun appeared to be free from either spots or faculæ; the days without spots being 211 as compared with 158 in 1888; and the days when neither spots nor faculæ were seen being more than twice as numerous last year as in the year previous. (2) The distinct but temporary revival of spot activity during the months of June, July, August, and September. (3) The appearance of spots in high latitudes; and lastly, the remarkable falling off in chromospheric phenomena, particularly during the last months of the year. It is, therefore, still difficult to be certain whether we have yet reached the actual minimum or no; the revival of the spots during last summer, connected as it was with so remarkable an increase in their mean distance from the equator, seemed to point to the minimum having been passed; but the season of almost perfect quiet which followed it, together with the decrease in the number and size of the prominences, favour the opposite conclusion. The mean daily spotted area for 1889 was less than that for 1888, but only by about one-seventh.

The three most remarkable groups of 1889 were those first seen on June 16, June 29, and August 2 respectively. The first-named was the largest group of the year; it formed and disappeared on the further side of the sun, and was seen during three rotations. The third was also seen during three rotations, but formed and died out in the visible hemisphere. It was the second group as to dimensions, and lay in S. lat. 20°, whilst the spot of June 16 was in S. lat. 6°. The spot of June 29 was only a very small one, and lasted but a couple of days, but was noticeable from its high latitude, 40° S. A fourth group, that first seen on August 9, though not attaining so large a mean area as the spot of June 16, exceeded it on one particular day, August 15.

The following table gives the monthly numbers for spots and faculæ as supplied by Prof. Tacchini in the *Comptes rendus*, vol. cviii. No. 21, vol. cix. No. 4, and vol. cx. No. 5, and may be compared with those given in NATURE for 1889 March 7, and in previous volumes:—

1889.	Proportion of days without spots.	Sun-spots. Relative frequence.	Sun-spots. Relative size.	Sun-spots. Mean daily number of groups.	Faculæ. Relative size.
January	1.00	0.00	0.00	0.00	6.00
February	0.50	3.26	8.12	0.56	1.56
March	0.62	1.69	3.64	0.50	6.81
April	0.60	0.65	4.35	0.40	7.25
May	0.96	0.04	0.65	0.04	5.30
June	0.56	1.97	25.22	0.45	9.63
July	0.39	2.75	16.97	0.87	14.35
August	0.19	6.97	20.03	1.26	17.77
September	0.48	1.18	8.22	0.61	28.48
October	0.73	0.64	1.55	0.27	18.18
November	1.00	0.00	0.00	0.00	0.62
December	0.61	1.68	4.09	0.65	29.55

The table shows that as in 1888 the faculæ did not vary quite in accordance with the spots, September and December being heavy months for the former, their relative area then exceeding that for any month since July 1886. The prominences on the other hand showed a very marked falling off towards the end of the year; February and March, light months for spots and faculæ, being much the most prolific as to the flames. The following are the mean numbers for the prominences resulting from Prof. Tacchini's monthly reports. It must be borne in mind that the difference in the atmospheric conditions of England and Italy renders it impossible to compare Prof. Tacchini's results with those formerly given by the late Rev. S. J. Perry, and which have been incorporated in former annual summaries in NATURE.

	Prominences. Days of observation.	Prominences. Mean daily number.	Prominences. Mean height.	Prominences. Mean extent.
			″	°
1887	214	8.26	45.2	1.7
1888	227	7.94	45.9	1.5
1889	247	3.20	34.7	1.1

The variations in the magnetic elements accorded in their more general features, though not in details, with those of the sunspots, as the following table given by Dr. R. Wolf in the *Comptes rendus*, vol. cx. No. 3, sufficiently shows:—

1889.	Wolf's relative numbers (Zurich). r	Wolf's relative numbers (Zurich). Δr	Variation in magnetic declination (Milan). r	Variation in magnetic declination (Milan). Δr
January	1.0	− 12.0	1.75	− 1.28
February	7.9	+ 0.9	3.99	+ 0.97
March	6.3	+ 0.0	6.17	− 0.94
April	4.9	+ 1.0	8.85	+ 0.58
May	2.4	− 8.4	8.19	− 0.29
June	7.0	+ 0.5	8.86	− 0.41
July	8.0	+ 6.1	8.25	− 0.32
August	20.6	+ 18.7	8.99	− 0.18
September	6.3	− 1.5	6.84	− 0.47
October	0.0	− 2.0	6.10	− 0.22
November	0.0	− 12.9	2.55	+ 0.37
December	5.7	− 4.2	1.96	+ 0.20
Mean	5.8	− 0.9	6.04	− 0.17

Dr. Wolf's formula for Milan, $v = 5'.62 + 0'.045\, r$, wh[illegible] $r = 5.8$, would give $v = 5'.88$, a much closer accord than f[illegible] the two preceding years.

THE GLOW OF PHOSPHORUS.[1]

THE word *phosphorus*, originally applied to any substance, solid or liquid, which had the property of shining in the dark, has gradually lost its generic sense, and is nowadays practically restricted, as a designation, to the wax-like inflammable substance which plays such an important part in the composition of an ordinary lucifer match. Phosphorus, indeed, is one of the most remarkable of the many remarkable substances known to the chemist. The curious method of its discovery, the universality of its distribution, its intimate connection with the phenomena of animal and vegetable life, its extraordinary physical properties and chemical activity, its abnormal molecular constitution, the Protean ease of its allotropic transformations—all combine to make up a history which abundantly justifies its old appellation of *phosphorus mirabilis*. Godfrey Hankewitz more than 150 years ago wrote: "This phosphorus is a subject that occupies much the thoughts and fancies of some alchymists who work on microcosmical substances, and out of it they promise themselves golden mountains." Certainly no man of his time made more in the way of gold out of phosphorus than Mr. Hankewitz, for at his little shop in the Strand he enjoyed for many years the monopoly of its sale, guarding his *Arcana* with all the jealousy of a modern manufacturer of the element.

Phosphorus, or, as it was then called, the *noctiluca*, was first seen in this country in 1677. It was shown to Robert Boyle, who had already worked on phosphorescence in general, and who seems to have been specially struck with the remarkable peculiarity of a factitious body which could be made "to shine in the dark without having been befo e illumined by any lucid substance and without being hot as to sense." In these respects the substance differed from all the *phosphori* hitherto known. The conditions which determine its glow were the subject of the earliest observations on phosphorus, and Boyle has left us a minute account of his work on the point. In the first place, he noticed that the substance was only luminous in presence of air. He accurately describes the nature of the light, and noticed that the water in which the phosphorus was partially immersed acquired "a strong and penetrant taste, . . . and relished a little like vitriol." On evaporation it would not "shoot into crystals, . . . but coagulated into a substance like a Gelly, or the Whites of Eggs which would be easily melted by heat." On heating this "Gelly" it gave off "flashes of fire and light," and had a "garlick smell." He also found that the *noctiluca* was soluble in certain oils, and he particularly mentions oil of cloves as a convenient means of showing the luminosity, as it is "rendered more acceptable to the standers-by by its grateful smell." "In Oyl of Mace it did not appear luminous nor in Oyl of Aniseeds." Boyle describes a number of experiments showing how small a quantity of the phosphorus is required to produce a luminous effect. "A grain of the *noctiluca* dissolved in Alkohol of Wine and shaken in Water; it render'd 400,000 times its weight luminous throughout. And at another Tryal I found that it impregnated 500,000 times its weight; which was more than one part of Cochineel could communicate its colour to." "And one thing further observable was that when it had been a long time exposed to the air it emitted strong and odorous Exhalations distinct from the visible Fumes." The strong and odorous exhalations we now know to be ozone.

The earlier volumes of the Philosophical Transactions contain several papers on the luminosity of phosphorus, and one by Dr. Frederic Slare is noteworthy as giving one of the earliest, if not actually the earliest account of what is one of the most paradoxical phenomena connected with the luminosity of phosphorus, namely its increase on rarefying the air. "It being now generally agreed that the fire and flame [of phosphorus] have their pabulum out of the air, I was willing to try this matter *in vacuo*. To effect this, I placed a considerable lump of this matter (phosphorus) under a glass which I fixed to an engine for exhausting the air; then presently working the engine, I found it grow lighter [*i.e.* more luminous] though a charcoal that was well kindled would be quite extinguished at the first exhaustion; and upon the third or fourth draught which very well exhausted the glass, it much increased its light, and continued so to shine with its increased light for a long time; on re-admitting the air, it returns again to its former dulness." This observation was repeated and its result confirmed by Hawksbee in this country and by Homberg in France, and seems subsequently to have led Berzelius, and after him Marchand, to the conclusion that the luminosity of phosphorus was altogether independent of the air (*i.e.* the oxygen) but was solely due to the volatility of the body. Many facts, however, combine to show that the air (oxygen) is necessary to the phenomenon. Lampadius found that phosphorus would not glow in the Torricellian vacuum; and Lavoisier, in 1777, showed that it would not inflame under the same conditions; and the subsequent experiments of Schrötter, Meissner, and Müller are decisive on the point that the glow is the concomitant of a chemical process dependent upon the presence of oxygen. It is, however, remarkable that phosphorus will not glow in oxygen at the ordinary atmospheric pressure and temperature, but that if the oxygen be rarefied the glow at once begins, but ceases again immediately the oxygen is compressed. Indeed, phosphorus will not glow in compressed air, and the flame of feebly burning phosphorus may be extinguished by suddenly increasing the pressure of the gas. Phosphorus, however, can be made to glow in oxygen at the ordinary pressure or in compressed air if the gases be gently warmed. In the case of oxygen the glow begins at 25° and becomes very bright at 36°. In compressed air the temperature at which the glow is initiated depends upon the tension. If the oxygen be absolutely deprived of moisture the phosphorus refuses to glow under any conditions. This fact, strange as it may seem, is not without analogy; the presence of traces of moisture appears to be necessary for the initiation or continuance of chemical combination in a number of instances.

It was observed by Boyle that a minute quantity of the vapour of a number of essential oils extingui hed the glow of phosphorus. The late Prof. Graham confirmed and extended these observations; he showed that relatively small quantities of olefiant gas and of the vapours of ether, naphtha, and oil of turpentine entirely prevented the glow; and subsequent observers have found that many essential oils, such as those of peppermint and lemon and the vapours of camphor and asafœtida, even when present in very small quantity, stop the absorption of oxygen and the slow combustion of phosphorus in air.

It has been established that whenever phosphorus glows in air or in rarefied oxygen, ozone and hydrogen peroxide are formed, but it is not definitely known whether the formation of these substances is the cause or the effect of the chemical process of which the glow is the visible sign. That there is some intimate connection between the luminosity of the phosphorus and the production of these bodies is highly probable. Schönbein, as far back as 1848, sought to demonstrate that the glow depends on the presence of ozone. It is certainly true that many of the substances, such as the essential oils, which prevent the glow of phosphorus, also destroy ozone. At a low temperature, phosphorus produces no ozone in contact with air, neither does it glow. It has been found, in fact, that, with air, ozone is produced in largest quantity at 25°, at which temperature phosphorus glows brightly. On the assumption that the oxidation of the phosphorus consists in the immediate formation of the highest oxide, the production of the ozone and the hydrogen peroxide has been represented by the following equations:—

$$P_2 + 3O_2 = P_2O_5 + O.$$
$$O + O_2 = O_3.$$
$$O + H_2O = H_2O_2.$$

Both these reactions may, of course, go on simultaneously; ozone and hydrogen peroxide are not mutually incompatible; the synthesis of hydrogen peroxide by the direct oxidation of water seems to occur in a number of processes. But such symbolic expressions can at most be only very partial representations of what actually occurs. It is highly probable that the combination which gives rise to the glow only occurs between the *vapour* of phosphorus and the oxygen. Phosphorus is sensibly volatile at ordinary temperatures, and by rarefying the atmosphere in which it is placed its volatilization is increased, which serves to account for the increased glow when the pressure of the gas is diminished. When phosphorus is placed in an atmosphere of hydrogen, nitrogen, or carbonic acid, these gases, when brought into contact with oxygen, become luminous from the oxidation of the vapour of phosphorus diffused through them. The rapidity of volatilization varies with the particular gas; it is greatest in the case of hydrogen, and least in that of carbonic acid. Indeed, a stream of hydrogen gas at ordinary temperatures carries away comparatively large quantities of phosphorus, which may be collected by appropriate solvents. No ozone and no glow is

[1] Lecture delivered on Friday evening, March 14, at the Royal Institution, by Prof. Thorpe, F.R.S.

produced in oxygen gas at ordinary temperatures and pressures, but on warming the oxygen, both the ozone and the glow are formed. On passing ozone into oxygen at temperatures at which phosphorus refuses to glow, the phosphorus at once becomes luminous, oxygen is absorbed, and the characteristic cloud of oxide is produced, and the effect continues so long as the supply of ozone is maintained. A drop of ether at once extinguishes the glow. The ether is in all probability converted into vinyl alcohol with simultaneous formation of hydrogen peroxide by the reaction indicated by Poleck and Thümmel:—

$$\begin{matrix} CH_3CH_2 \\ CH_3CH_2 \end{matrix}\Big>O + O_3 = \begin{matrix} CH_2CHOH \\ CH_2CHOH \end{matrix} + \begin{matrix} HO \\ HO \end{matrix}\Big\}.$$

A. W. Wright has shown that formic, acetic, and oxalic acids are also formed by the action of ozonized oxygen on ether.

Phosphorus combines with oxygen in several proportions, and the study of the mode of formation and properties of these oxides is calculated to throw light upon the nature of the chemical process which attends the glow of phosphorus. Certain of these oxides have recently been the subject of a considerable amount of study in the chemical laboratories of the Normal School of Science. When phosphorus is slowly burned in air, there is produced a considerable quantity of a volatile substance, having a characteristic garlic-like smell, which solidifies, when cooled, in beautiful arborescent masses of white crystals. It melts at about 23°, and boils at 173°. In a sealed tube kept in the dark, it may be preserved unchanged, but on exposure to light, and especially to bright sunshine, it rapidly becomes deep red. It slowly absorbs oxygen at the ordinary temperature and pressure, but from the mode in which the solid product of the reaction (P_2O_5) is deposited, it is evident that the union only takes place between the *vapour* of the oxide and the oxygen gas. Under diminished pressure the act of combination is attended with a glow which increases in brilliancy if ozone be present. On compressing the oxygen, the glow ceases. No ozone is formed during the act of oxidation. The degree of rarefaction needed to initiate the glow depends upon the temperature of the oxide—the warmer the oxide the less is the diminution of pressure required. By gradually warming the oxide, the luminosity steadily increases both in area and intensity, until at a certain temperature the mass ignites. The change from glow to actual flame is perfectly regular and gradual, and is unattended with any sudden increase in brilliancy. In this respect the process of oxidation is analogous to the slow and barely visible burning of fire-damp which is sometimes seen to occur in the Davy lamp, or to the slow combustion of ether and other vapours, which has been specially studied by Dr. Perkin. Other instances of what may be called *degraded combustion* are known to chemists. Thrown into warm oxygen, the substance bursts into flame at once and burns brilliantly, and it also takes fire in contact with chlorine. Alcohol also ignites it, and when it is warmed with a solution of potash or with water it evolves spontaneously inflammable phosphoretted hydrogen. In contact with cold water it suffers only a very gradual change, and many days may elapse before even a comparatively small quantity is dissolved. This substance has long been known; it was discovered, in fact, by the French chemist Sage, but its true nature has only now been determined. Its chemical formula is found to be P_4O_6; hence its composition is similar to that of its chemical analogue, arsenious oxide.

The study of the properties of this remarkable substance enables us to gain a clearer insight into the nature of the chemical process attending the glow of phosphorus. When phosphorus is placed in oxygen, or in an atmosphere containing oxygen, under such conditions that it volatilizes, the phosphorus oxidizes, partly into phosphoric oxide and partly into phosphorous oxide. Ozone is formed, possibly by the reaction already indicated, and this reacts upon the residual phosphorus vapour and the phosphorous oxide with the production of the luminous effect to which the element owes its name. The glow itself is nothing but a slowly-burning flame having an extremely low temperature, caused by the chemical union of oxygen with the vapours of phosphorus and phosphorous oxide. By suitable means this glow can be gradually augmented, until it passes by regular gradation into the active vigorous combustion which we ordinarily associate with flame. Many substances, in fact, may be caused to phosphoresce in a similar way. Arsenic, when gently heated, glows in oxygen, and sulphur may also be observed to become luminous in that gas at a temperature of about 200°.

"BEFORE AND AFTER DARWIN."

On Tuesday, March 25, Prof. G. J. Romanes, F.R.S., concluded his course of between thirty and forty lectures, which, under the above title, he has been delivering at the Royal Institution during the last three years. At the close of the lecture he announced his intention of publishing the whole course in November next, and distributed among the audience printed slips, conveying in the form of twelve propositions the "general conclusions" to which his lectures for the present year have led. The following is a copy of this printed slip:—

(1) "Natural selection has been the main, but not the exclusive means of modification," both as regards species and all the higher taxonomic divisions.

(2) Of the other factors of organic evolution it is not improbable that we are still to a large extent ignorant. Whether, or to what extent, sexual selection and the Lamarckian principles have co-operated, is a matter with which I am not specially concerned; but I think there is abundant evidence to establish the high importance in this connection of amixia, or independent variability,—at all events as regards the evolution of species.

(3) Natural selection is primarily a theory of the cumulative development of adaptations wherever these occur, and therefore is only incidentally, or likewise, a theory of the origin of species in cases where allied species differ from one another in respect of peculiar characters, which are also adaptive characters.

(4) Hence it does not follow from the theory of natural selection that all species—much less all specific characters—must *necessarily* have owed their origin to natural selection, since it cannot be proved deductively from the theory that no "means of modification" other than natural selection is competent to produce such slight degrees of modification as go to constitute diagnostic distinctions between closely-allied species; while, on the other hand, there is an overwhelming mass of evidence to prove the origin of "a large proportional number of specific characters" in causes of modification other than natural selection.

(5) Even if it were true that all species and all specific characters must necessarily owe their origin to natural selection, it would still remain illogical to define the theory of natural selection as indifferently a theory of species or a theory of adaptations; for, even upon this erroneous supposition, specific characters and adaptive characters would remain very far indeed from being conterminous—by far the larger number of adaptations which occur in organic nature being the common property of many species.

(6) In no case can natural selection have been the cause of mutual infertility between allied or any other species.

(7) Without isolation, in the sense of either separate or segregate breeding, organic evolution is in no case possible; and hence, heredity and variability being given, the whole theory of organic evolution may be regarded as a theory of the causes and conditions which have led to isolation, or the mating of similar variations to the exclusion of dissimilar.

(8) Natural selection is one among sundry distinct kinds of isolation, and presents in this relation the following peculiarities: (*a*) the isolation is with reference to superiority of fitness; (*b*) is effected by destruction of the excluded individuals; and (*c*) unless assisted by some other kind of isolation, can only effect monotypic as distinguished from polytypic evolution.

(9) It is a general law of organic evolution that the number of possible directions in which divergence may occur can never be more than equal to the number of cases of efficient isolation; but, excepting natural selection, any one kind of isolation need not necessarily require the co-operation of another kind in order to create an additional case of isolation, or to cause polytypic as distinguished from monotypic evolution.

(10) Where common areas are concerned, the most general and most efficient kind of isolation has been the physiological—and this whether the mutual infertility has been the antecedent or the consequent of morphological changes on the part of the types concerned, and whether or not these changes are of an adaptive character.

(11) This form of isolation—which in regard to *incipient species* I have called physiological selection—may act either alone, or in conjunction with other kinds of isolation on common areas: in the former case its agency is of most importance among plants and the lower classes of animals; in the latter case its importance consists in its greatly intensifying the segregating power of whatever other kind of isolation it may be with which it is associated.

(12) Although physiological selection must in all cases refer primarily to first crosses, its activity as a cause of segregation is intensified in cases where it extends also to second crosses.

SCIENTIFIC SERIALS.

American Journal of Mathematics, vol. xii., No. 3 (Baltimore, March 1890.)—A memoir "Sur les équations aux dérivées partielles de la physique mathématique," by that brilliant mathematician, M. Poincaré, occupies pp. 211–294. Some idea of the writer's aim will be gained from the following passages:—"Quand on envisage les divers problèmes de calcul intégral qui se posent naturellement lorsqu'on veut approfondir les parties les plus différentes de la physique, il est impossible de n'être pas frappé des analogies que tous ces problèmes présentent entre eux." "Cette revue rapide des diverses parties de la physique mathématique nous a convaincus que tous ces problèmes, malgré l'extrême variété des conditions aux limites, et même des équations différentielles, ont, pour ainsi dire, un certain air de famille qu'il est impossible de méconnaître. On doit donc s'attendre à leur trouver un très grand nombre de propriétés communes." The concluding sentence is: "Je pourrai dire alors que les conclusions sont démontrées d'une façon rigoureuse au point de vue physique. Peut être même est-il permis d'espérer que, par une sorte de passage à la limite, on pourra fonder sur ces principes une démonstration rigoureuse même au point de vue analytique."—The remaining article of the number is one on singular solutions of ordinary differential equations, by H. B. Fine (pp. 295–322). Following the lead of Briot and Bouquet, this memoir bases the theory of singular solutions on the differential equation, and avoids all use, direct or indirect, of the notion of the complete primitive.

In *Bulletin* No. 2 of the Brussels Academy of Science, M. E. Ronkar criticizes a paper by M. J. Liagre, on the mutual impulse of the earth's surface and centre because of interior friction. The paper in question dealt with the interior structure of the earth, and the conclusions drawn have some bearing on diurnal nutation.—In a paper on the venous pulse, M. Léon Fredericq gives his investigations into the form of various pulses—jugular, venous, and carotid; traces the identity of the pulse of the jugular vein and that of the right auricle; and discusses generally the phenomena of circulation and respiration. The same author adds a note on the preservation of oxyhæmoglobin.—M. A. F. Renard has examined phillipsite crystals from the deposits obtained from the centre of the Pacific Ocean. These microscopical crystals were discovered by Mr. Murray, and a brief description of them published by him in conjunction with the author in 1884 (Royal Society of Edinburgh). A more particular description and determination of the character of these zeolites, and the deposits in which they occur, is now given. A plate containing four drawings of the crystals accompanies the paper.—M. G. van der Mensbrugghe, in a paper on the condensation of water-vapour in capillary spaces, reviews the principal facts owing their origin to such condensation, and shows that they are in confirmation of the theory propounded by Sir William Thomson in 1874, in a paper on the equilibrium of vapour at a curved surface of liquid. The experimental verification of the formula there given will form the subject of a second communication.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, February 20.—"Some Stages in the Development of the Brain of *Clupea harengus*." By Ernest W. L. Holt, Marine Laboratory, St. Andrews. Communicated by Prof. McIntosh, F.R.S.

The stages described are (i) newly-hatched or early larval; (ii) early post-larval; (iii) ½ inch long; (iv) ¾ inch long.

The development of the pineal region is treated separately, and in this a fifth stage—1 1/12 inch long—is introduced.

In the early larval stage the downward flexure of the fore part of the brain is very noticeable. It appears due to the general conformation of the head at this stage. A diverticulum of the 3rd ventricle extends downwards and backwards, its distal extremity underlying the optic commissure. The broad ventral commissure of the infundibulum, noticed by McIntosh and Prince in *Anarrhicas*, is well marked. A commissure shuts off the lumen of the infundibulum from the hind part of the 3rd ventricle immediately in front of the splitting off of the infundibulum. The valvula appears in transverse section as a pair of ridges externally to the tori, before it shuts off the aqueduct of Sylvius. The cerebellar fold is very short.

In the early post-larval stage "an apparent rectification of the cranial axis" has taken place, by the upward rotation of the cerebrum on its posterior end, doubtless owing to the rapid development of the oral and trabecular cartilages, and consequent forward rotation of the mouth. The same causes have also operated so as to withdraw the diverticulum of the 3rd ventricle from its position below the optic commissure. The infundibulum has undergone vertical flattening. The future lobi inferiores are indicated as lateral expansions, behind which the 3rd oculomotor nerves pass outwards from the centre of the ventral surface of the cerebral mass. The infundibulum extends some way back above the notochord as a thin-walled sac. Its walls are little plicated compared with those in some other forms, *e.g.*, *Rhombus*, *Anarrhicas*.

In the ½-inch stage the olfactory lobes appear as bulbous masses projecting from the front end of the cerebrum. A pale median septum appears between the anterior extremities of the lateral optic ventricles, its base resting on the fibrous tract over the hind part of the 3rd ventricle. The tip of the valvula now appears in transverse section before its connection with the cerebral mass can be made out, having thus grown forward. The cerebellum has greatly increased in size; instead of terminating as before on the surface of the brain, it is now continued into a thick fold bent sharply down on the anterior portion; its posterior end passes at once into the thin roof of the 4th ventricle. Two fibrous bands cross over the aqueduct of Sylvius in the substance of the cerebellum; their lateral extremities are fused. The lobi inferiores are better marked than in earlier stages. Longitudinal bands of fibres pass back from the roots of the oculomotor nerves through the medulla oblongata. Groups of large ganglionic cells appear on either side of these bands, and are connected by a fine commissure passing through both bands. At the origin of the 8th auditory nerves, this commissure is replaced by a St. Andrew's cross of fibres, the dorsal limbs of the cross passing to the nerve roots, and the ventral to the ganglionic areas.

In the ¾-inch stage the olfactory lobes are more elongated. The olfactory nerves pass outwards from their anterior extremities. The septum behind the pineal body, after losing its ventral connection with the fibrous tract over the 3rd ventricle, persists for some way back as a cellular leaf-like appendage of the thin median roof of the optic ventricle; a few fibres pass back into this appendage.

Large ganglionic cells appear in the tori semicirculares about the region of the splitting off of the infundibulum.

From behind the region of the auditory nerves a ganglionic area on either side persists backwards through the medulla oblongata.

Pineal Region.

The roof of the thalamencephalon in the early stages is a single layer of large columnar cells passing forward from the front wall of the pineal stalk. It passes into the roof of the cerebrum, the cells diminishing greatly in size. The superior commissure of Osborn is present from the early post-larval stage; it it also present in the larval and post-larval *Zoarces viviparus*, where it is distinctly double. The first signs of the infrapineal recess of Hoffman are seen in the ½-inch stage. It is thus much later in developing than in *Salmo*, and the fold forming its front wall never extends backwards to the same degree as in that form and in *Anarrhicas*. This fold, in the post-larval *Zoarces*, is thickened in its apex, and lodges a fine commissure. As pointed out by Balfour in Elasmobranchs the fold is due to the upward rotation of the cerebrum.

The fibrous tract over the 3rd ventricle in the herring is well marked in the ¾-inch stage. It is seen to consist of fibres passing upwards and inwards from the optic thalami to the middle line above the 3rd ventricle, and then running forward to the stalk of the pineal body. The tract has a double nature, as is readily seen in vertical longitudinal sections of a herring 1 1/12 inch long. It is seen here to be a backwardly directed fold of the brain roof, continuous ventrally with the back wall of the pineal stalk, and dorsally with the roof of the optic ventricle, the apex of the fold being the posterior commissure. Its length in this form is due to the flattening of the brain, the tract being very short in

Zoarces, where the brain is not flattened. In *Zoarces*, also, from the same cause, the limbs of the fold are less closely applied to each other and much thicker.

The pineal body is roundish and solid in the early larval stage in the herring. It is vertically flattened in the early post-larval stage. In the $\frac{1}{2}$-inch stage it is much larger and contains a lumen; it shows signs of constriction into proximal and distal elements, and the lumen contains a coagulable albuminous fluid, as in *Petromyzon*. In the $1\frac{1}{16}$-inch stage the constriction is still visible, and the walls are generally crenated. The tissues of the pineal wall are now divided into three layers, and are of varying thickness. The cartilage of the tegumen cranii overlies the body at this stage. The constriction of the body appears to be an exaggeration of the crenation of the pineal wall met with in *Salmo*; it has not, probably, the morphological value of the constriction of the body in *Petromyzon*.

March 27.—"On the Stability of a Rotating Spheroid of Perfect Liquid." By G. H. Bryan. Communicated by Prof. G. H. Darwin.

The investigations of Riemann, Basset, and others have proved that Maclaurin's spheroid, when composed of frictionless liquid, ceases to be stable for an "ellipsoidal" type of disturbance when its eccentricity attains the value 0·9528867. The object of the present paper is to discuss the conditions of stability with reference to disturbances of a general type expressible in terms of spheroidal harmonics, with the view of examining whether Riemann's condition is sufficient to ensure stability for displacements other than ellipsoidal.

Taking the criteria of stability determined in a previous communication (Phil. Trans., A., 1889), the author shows by numerical calculation that the form which is critical for an ellipsoidal disturbance is stable for disturbances determined by several of the lower harmonics. These results are then extended by a perfectly general investigation to all other types of displacement.

The conclusion is that Riemann's and Basset's condition of stability is sufficient to ensure the absolute stability of Maclaurin's rotating spheroid for every possible displacement. Also that, unless the liquid is subject to hypothetical constraints, we cannot initially obtain any form other than ellipsoidal from the instability of the spheroidal form. In the case considered of perfect liquid this ellipsoid does not rotate as if rigid, but its principal axes rotate with half the angular velocity of the liquid.

Physical Society, March 7.—Prof. W. E. Ayrton, F.R.S., President, in the chair.—Dr. S. P. Thompson described Bertrand's refractometer, and exhibited the capabilities of the instrument before the Society. Its action depends on total reflection. The refractometer consists of a hemisphere of glass, about 8 mm. diameter, set at the end of a tube, the plane face being outwards and inclined at about 30° with the axis. One side of the convex surface of the hemisphere is illuminated through a piece of ground glass set about perpendicular to the plane face. The hemisphere is viewed through an eye-piece focussed on a scale divided to tenths of millimetres placed within the tube. The instrument is particularly useful for mineralogical specimens and liquids. The procedure in the latter case is to smear a film of the liquid over the plane face of the hemisphere, and by looking through the eye-piece determine the scale reading of the line which separates the light and darker portions of the field. A reference to a calibration table gives the refractive index. In experimenting with solids a thin film of a very dense liquid (supplied with the instrument) is placed between the specimen and the glass, and the procedure is then as above. The refractive index of opaque solids can be determined in this way. In using the instrument for minerals great care must be taken not to scratch the glass. The handiness of the refractometer and its perfect portability (its dimensions being about 5 centimetres long by $2\frac{1}{2}$ cm. diameter) are great recommendations. Mr. Blakesley asked to what accuracy the scale could be read, and whether the sensitiveness of the instrument was at all comparable with that of other methods. Prof. Dunstan inquired if it could be used with volatile liquids. In reply Dr. Thompson said that with non-homogeneous light the scale could be read to 1 division, but with a sodium flame one-tenth of a division could be estimated. For volatile liquids, a drop may be used instead of a film, or the evaporation of a thick film may be retarded by a cover-glass.—Mr. H. Tomlinson's paper, on the Villari critical point in nickel, was postponed.—Prof. Dunstan described an apparatus for distilling mercury in a vacuum, devised by himself and W. Dymond, and showed the working of the arrangement. It consists of a 3 mm. soft glass tube rather more than a metre long, having an oblate spheroidal bulb blown at the upper end. The bulb is placed over a ring burner. At the top of the bulb, a tube of 1·5 mm. diameter is attached, and this passes outside the bulb, and descends close to the larger tube. The part of the smaller or fall tube just below the bulb is enlarged so as to form a condensation chamber, and the lower part serves as a Sprengel tube. A conical reservoir containing the mercury to be distilled is in flexible connection with the lower end of the large tube as in Clark's well-known apparatus. The advantages claimed for the new apparatus are, its relative shortness and portability, the small quantity remaining undistilled, and its non-liability to damage or derangement if left unsupplied with mercury. To ensure satisfactory working a constant pressure of gas is necessary, and this is obtained by inserting a Sugg's dry governor in the supply pipe. During distillation, peculiar green flashes are seen within the condensation chamber, and these are intensified by bringing it near an electric machine in action. The apparatus also serves well to show the character of an electric discharge through mercury vapour, for the mercury in the two tubes may be used as electrodes. Prof. Thompson said he devised a simple form of distilling apparatus some time ago which answered fairly well, and could be made by any amateur glass-worker. It consisted of a double barometer, one leg of which was of small bore, so as to act as a Sprengel tube. The rising part of the bend at the top of the larger tube was expanded and served as the evaporating chamber, below which a burner was placed. The President asked why Clark's apparatus is made so lengthy. In reply to this question Mr. Boys said that as the fall tube goes down within the rising one, the mercury near the top of the latter is heated by the condensing mercury (thus economising gas) and hence condensation does not take place until the vapour has passed a considerable distance down the fall tube.—Prof. S. U. Pickering read a paper on the theory of osmotic pressure and its bearing on the nature of solution. The author said that considerable doubt exists as to the accuracy of the premises on which the theory is based, and if the theory is to be regarded as true and not merely a rough working hypothesis, the following conditions must be fulfilled by weak solutions—(1) The molecular depression of the freezing-point must be independent of the nature of the dissolved substance. (2) Any deviations from (1) must be in the direction indicated by the theory. (3) the depression must be independent of the nature of solvent. (4) The depression must be independent of the amount of solvent (all solutions being weak). (5) The deviations with strong solutions should be in the theoretical direction. (6) They should be regular. Prof. Pickering proceeded to show that experiment, instead of confirming the above statements, disproves them all. As regards (1), without counting abnormally low (half) values, Raoult's results show variations of 60, 40, 30, &c., per cent. in different cases, and the author quoted other values where the variations were 500, 260, 230, &c., per cent. These variations, he considered, were too great to be explained by the fact of the solutions used being 3 or 4 times too strong. Referring to (2), he said that low values are reasonably explained by the polymerization of the dissolved molecules, high values by their dissociation into ions. He then argued that there are no abnormally high values, for the view that such exist, and that they are explainable by dissociation involves the following conclusions: (*a*) that the more stable a subtstance is, the more easily is it dissociated; (*b*) that solution dissociates molecules which we know can exist undissociated as gases; (*c*) that water must consist of $1\frac{1}{2}H_2O$, and the atomic theory is wrong; (*d*) that energy can be created, and therefore the theory of its conservation is untenable. With respect to (3), it was pointed out that in many instances the same dissolved substance gives the full depression with one solvent and half depression with another. Cases were quoted where the depression produced by the same dissolved body in different solvents showed variations of 36,000, 21,000, and 28,000 per cent. In discussing (4), the author said that even with solutions weaker than that corresponding to a gas, the law is not fulfilled. Taking the case of sulphuric acid (the only one at present fully investigated), the variations amount to 40 per cent., or about 28 times the experimental error. With reference to (5), it was stated that with strong solutions the molecular depression should become smaller, but in every known case (9 were quoted) it becomes larger, the increase in one instance being 3,200 per

cent. As regards (6), all experimental data available, especially those relating to sulphuric acid, show that the deviations are neither regular nor always in the same direction. Mr. T. H. Blakesley said he was greatly interested with Prof. Pickering's paper, for some time ago he was induced to make experiments on the volume of salts in solution by reading Joule's papers on that subject. Some of the results confirmed, but others did not agree with, Joule's theory that the molecular volume in solution was a whole number. If this theory was true, then (he said) it would be possible to predetermine the density of solutions, and from the measured density of any known solution we could determine the water of crystallization of the salt from the formula

$$n = \frac{1 - \frac{W}{w}(D - 1)}{D}\left(\frac{A}{H_2O} + x\right);$$

where W and w are the masses of the water and salt respectively, D the density of the solution relative to water at the same temperature, A the molecular weight of the dehydrated portion of the salt, x the number of molecules of water, and n the molecular volume of the salt in solution, the two latter being whole numbers.

Chemical Society, March 6.—Dr. W. J. Russell, President in the chair.—The President announced that the senior Secretary would attend the meeting to be held in Berlin on March 11 to celebrate the 25th anniversary of the promulgation of Prof. Kekulé's benzene theory, and would present a congratulatory address from the Society.—The following papers were read:—Some crystalline substances obtained from the fruits of various species of *Citrus*, by Prof. W. A. Tilden, F.R.S., and Mr. C. R. Beck. The authors have examined the solid matters which are deposited from freshly extracted oils of limes, lemons, and bergamot made by hand. The substance, limettin, obtained from oil of limes (*C. limetta*) has the composition $C_{16}H_{14}O_6$, and crystallizes in tufts of needles melting at 121°–132°. It is neither an acid nor a glucoside, is not acted upon by acetic chloride or phenylhydrazine, and yields phloroglucol, and acetic and formic acids on fusion with potash. Essence of lemons yields a substance, $C_{14}H_{14}O_6$, very similar to limettin in appearance, though the crystals are more lustrous and melt at 116°. Bergamot yields a compound which crystallizes in colourless prisms and melts at 270°–271°.—Reduction of α-diketones, by Prof. F. R. Japp, F.R.S., and Dr. F. Klingemann. Benzil, when reduced by boiling with fuming hydriodic acid for a few minutes, gives an excellent yield of deoxybenzoïn. Phenanthraquinone, under like conditions, gives so-called phenanthrone, which, contrary to Lachowicz's view, is not the deoxybenzoïn of phenanthraquinone, but a mono-hydroxyphenanthrene.—Studies on isomeric change, No. IV; halogen derivatives of quinone, by Mr. A. R. Ling. The experiments of Hantzsch and of Nietzki have proved, in opposition to those of Levy, that the "anilic" acids are paradihydroxy-derivatives of quinone, and Hantzsch and Schniter have shown that an isomeric change occurs when paradichloroquinone is brominated, the product being metadichlorometadibromoquinone. The author has investigated the action of bromine on paradichloroquinone and diacetylparadichloroquinol, and the action of chlorine on paradibromoquinone, and has obtained results which confirm Hantzsch and Schniter's conclusion, since all attempts to prepare paradichloroparadibromoquinone, $CO\left\langle\begin{matrix}CBr.CCl\\CCl.CBr\end{matrix}\right\rangle CO$, have been unsuccessful, the product in every case consisting of the isomeric metadichlorometadibromoquinone, $CO\left\langle\begin{matrix}CCl.CBr\\CCl.CBr\end{matrix}\right\rangle CO$.—Note on a phenylic salt of phenylthiocarbamic acid, by Prof. A. E. Dixon.—Contributions to the chemistry of thiocarbamides; interaction of benzyl chloride and of allyl bromide with thiocarbamide, phenyl- and diphenylthiocarbamides, by Mr. E. A. Werner.

Geological Society, March 12.—Mr. J. W. Hulke, F.R.S., Vice-President, in the chair.—The following communications were read:—On a deep channel of drift in the valley of the Cam, Essex, by W. Whitaker. In Scotland and in Northern England long and deep channels filled with drift have been noticed, but not in Southern England. For some years one deep well-section has been known which showed a most unexpected thickness of Glacial drift in the higher part of the valley of the Cam, where that drift occurs mostly on the higher grounds and is of no very great thickness. Lately, further evidence has come to hand, showing that the occurrence in question is not confined to one spot, but extends for some miles. The beds found are for the most part loamy or clayey. At the head of the valley various wells at Quendon and Rickling show irregularities in the thickness of the drift, the chalk coming to or near the surface in some places, whilst it is nearly 100 feet below it sometimes. Further north, at Newport, we have the greatest thickness of drift hitherto recorded in the South of England, and then without reaching the base. At one spot a well reached chalk at 75 feet; whilst about 150 feet off that rock crops out, showing a slope of the chalk-surface of 1 in 2. In the most interesting of all the wells, after boring to the depth of 340 feet, the work was abandoned without reaching the chalk, the drift in this case reaching to a depth of about 140 feet below the level of the sea, though the place is far inland. The chalk crops out about 1000 feet eastward, and at but little lower level, so that there is a fall of about 1 in 3 over a long distance. At and near Wenden the abrupt way in which drift comes on against chalk has been seen in open sections. Two wells have shown a thickness of 210 and 296 feet of drift respectively; and as the chalk comes to the surface, at a level certainly not lower, only 140 yards from the latter, the chalk surface must have a slope of 1 in less than 1½, and this surface must rise again on the other side, as the chalk again crops out. The drift here reaches to a depth of 60 or 70 feet below the sea-level. At Littlebury, in the centre of the village, a boring 218 feet deep has not pierced through the drift, which reaches to 60 feet below the sea-level. As in a well only 60 yards west and slightly higher, the chalk was touched at 6 feet, there must here be a fall of the chalk surface of about 1·2 in 1. Eastward, too, on the other side of the valley, the chalk rises to the surface. The places that have been mentioned range over a distance of 6 miles. How much further the drift-channel may go is not known, neither can we say to what steepness the slope of the underground chalk surface may reach; the slopes given in each case are the lowest possible. The author thinks that the channel has been formed by erosion rather than by disturbance or dissolution of the chalk. After the reading of the paper there was a discussion, in which Dr. Evans, Mr. Clement Reid, Mr. Topley, Mr. J. Allen Brown, Dr. G. J. Hinde, and the author took part.—On the Monian and basal Cambrian rocks of Shropshire, by Prof. J. F. Blake.—On a crocodilian jaw from the Oxford Clay of Peterborough, by R. Lydekker.—On two new species of Labyrinthodonts, by R. Lydekker.

Linnean Society, March 20.—Mr. W. Carruthers, F.R.S., President, in the chair.—After reading the minutes of the last meeting, the following resolution, moved from the chair, was unanimously adopted:—"On the occasion of a gift, from Mr. Crisp, of a handsome oaken table for the meeting-room, the Society desires to record its deep sense of the valuable services rendered by that gentleman, not only as Treasurer, but by numerous acts which are not generally appreciated because they are practically unknown to the Fellows."—Prof. P. Martin Duncan, F.R.S., exhibited several specimens of *Desmophyllum crista-galli* obtained from an electric cable at a depth of 550 fathoms. Though showing great variation in the shape and nature of the wall, the specific characters of the septa were maintained. The core, extending as a thin lamina far beyond the peduncle, had no connection with the septa. A section of *Caryophyllia clavus* showed theca between the septa, and a section of *Lophohelia prolifera* exhibited a true theca extending beyond the septa.—Mr. E. B. Poulton, F.R.S., exhibited some Lepidopterous larvæ showing the variation in colour induced by natural surroundings; and some lizards, in spirit, from the West Indies, showing the pineal eye very distinctly.—In continuation of a former paper on the external morphology of the Lepidopterous pupa, Mr. Poulton gave a detailed and interesting account of the sexual differences observed in the development of the antennæ and wings.—Prof. G. B. Howes read a paper on the intestinal canal of the Ichthyopsida, with especial reference to its arterial supply. He described certain arteries hitherto unrecorded, and some variations he had found in them in the Frog and Salamander. The artery known in the Elasmobranchii as the inferior mesenteric, was shown to belong to

the superior mesenteric series. Discussing the morphology of the intestine and its derivates, the author defined the large intestine of the Pisces more precisely than had hitherto been done, and showed that the appendix digitiformis of the Elasmobranchs must be regarded as homologous with the appendix vermiformis of mammals, and that a short cæcum coli is present at any rate in the Batoidei. The anatomical relationships of the appendix digitiformis were described in certain Elasmobranchs for the first time, and some notes were added upon the cæcum and large intestine among Teleosteans.—An interesting paper was then read by Mr. R. A. Grimshaw, on heredity and sex in the honey-bee.

PARIS.

Academy of Sciences, March 24.—M. Hermite in the chair.—M. Mascart presented a note on a direct-reading transmission dynamometer with a photographic registering arrangement, and also one on the Observatory at Tananarivo, setting forth some of the meteorological work to be undertaken in this new Observatory.—M. Berthelot, in a paper on the condensation of carbonic oxide, and on the penetrability of glass by water, says that he has been unable to obtain evidence of the transmission of water through glass under the influence of the silent discharge, and finds that the carbonic oxide is truly condensed into a body which rapidly takes up moisture from the air.—Under agricultural chemistry, M. Th. Schlœsing makes some remarks relative to the subject of M. Berthelot's observations on the reactions between soils and atmospheric ammonia, and discusses the differences of opinion existing between them.—M. L. Ranvier, in microscopical observations of the contraction of living muscular fibres striated and unstriated, has contrived a method by which muscles may be excited whilst being viewed under a microscope, and from comparative observations of muscular elements in repose and contracted, finds that the homogeneous period and the inversion imagined by Merckel does not exist.—On the regulation of the motion of governors by an auxiliary dynamo, by M. A. Ledieu.—On the Cretaceous Echinodermata of Mexico, by M. Cotteau. Descriptions are given of six specimens received from Mexico. The specimens are interesting both from a zoological and geological point of view, since they determine the age of the strata in which they were found.—In studies on the capture theory of periodic comets, M. O. Callandreau extends the elaborate work done by M. Tisserand on the same subject.—On the discovery of a remarkable transcendental function, by M. Fredholm.—On the invariants of a class of equations of the first order, by M. Z. Elliot.—Relation between the volume, the pressure, and the temperature of different vapours, by M. Ch. Antoine.—Comparative study of specific inductive power, and of the conductibility of spaces filled with rarefied air, by M. James Moser. From the study of these properties with spaces containing air in three states of rarefaction—namely, (1) at a pressure of 10 mm. of mercury, (2) at 1 mm. pressure, (3) with an extreme vacuum—the author deduces that while the conductibility varies the specific inductive power remains constant.—Electrolysis of a mixture of two salts in aqueous solution, note by M. L. Boullevigne. Using a mixture of Zn and Cu salts, it is found that the composition of the brass deposited varies rapidly with the intensity of the current employed, contrary to Buff's law. Considering the variation to be due to the chemical action of the sulphate of copper upon the zinc in the alloy deposited, and that the amount of this action is proportional to the time, an expression is found which allows the composition of the alloy obtained with any given intensity to be calculated with a fair degree of accuracy as tested by experimental results. — A new method of preparation of betaïnes, by M. E. Duvillier. The author uses a reaction similar to that by means of which M. Schützenberger obtained the leucines synthetically; an ethereal iodide is caused to act upon the zinc salt of an amide acid in the presence of zinc oxide.—Titration of acetone by the iodoform reaction, by M. G. Arachequesne.—On callose, a new fundamental substance existing in cell membranes, by M. Louis Mangin.—The estimation of fatty matter in milk, by M. Lezé. 100 parts of milk are heated in a flask with a graduated neck till the mixture becomes brown, ammonia is added till the whole becomes clear, the fatty matter rising to the top and its volume being read off on the graduated neck.—On new forms of crystallized silica, note by MM. Michel-Lévy and Munier-Chalmas.—The solubility of some substances in sea-water, by M. J. Thoulet.—On the development of siliceous sponges and the conformation of leaflets among the sponges, by M. Yves Delage.—On the physiological mechanism of hatching, sloughing, and metamorphosis among Orthopterous insects of the Acridean family, by M. J. Kunckel d'Herculais.—On the great sand dunes of the Sahara, note by M. G. Rolland.—On the gypseous formations of the Paris basin, and on the siliceous deposits which have replaced the gypsum, by M. Munier-Chalmas.—On the physiological action of arsenietted hydrogen, by MM. F. Joly and B. de Nabias.—On the diarrhœic action of cholera cultures, by M. N. Gamaleia.—On the vibration of the earth at Chung-Hai and the movements of the compass at Zi-Ka-Wei during this vibration, by M. Chevalier. It is remarked from observations that the vibrations of the earth are unaccompanied by magnetic disturbances.

BERLIN.

Physiological Society, March 14.—Prof. du Bois-Reymond, President, in the chair.—Dr. Heymans spoke on myelin, giving a concise account of the numerous chemical and scanty microscopical investigations of what Virchow had designated as myelin-formations in peripheral nerves. From a chemical point of view the controversy had turned chiefly upon the existence or non-existence of Liebreich's protagon. The speaker had made investigations on frogs' nerves, from which he concluded that both protagon and lecithin are present in them, and that myelin-formations are due to imbibition, with simultaneous production of an external membrane.—Dr. Goldscheider gave an account of his researches on the sensitiveness of the articular surfaces of joints, based upon experiments on the tibial and metatarsal joints in rabbits. It appeared that the sensitiveness was dependent not so much upon the irritability of the surfaces of the joints, as of that of the epiphyses. The greatest effect was produced by direct stimulation of the marrow of the respective bones, while stimulation of the compact bone-substance showed that this was quite insensitive.

CONTENTS.

THURSDAY, APRIL 10, 1890.

NEW LIGHT FROM SOLAR ECLIPSES.

New Light from Solar Eclipses; or Chronology corrected by the Rectification of Errors in the received Astronomical Tables. By William M. Page. With an Introduction by the Rev. J. Brookes, D.D. (St. Louis: Barns Publishing Co., 1890.)

THIS is a book with a considerable portion of which we can have no concern, for it treats largely of theological matters of a disputed kind. It is the production, no doubt, of a devout and pious mind, but of one not scientifically trained. Indeed, we are informed, in an introduction by a St. Louis divine, that it is "written by a brother actively engaged in the ordinary pursuits of life," and an attempt is made to enlist our sympathies with the author on that account. This appeal would have been more effectual if the scientific conclusions at which the author has arrived, and for which he hopes to gain attention, were put forward either with more modesty on his own part, or with greater respect for recognized authorities.

But the contrary is the case. Our prejudices are not respected, and while the crudest statements are made on the smallest possible evidence, the work so bristles with errors that it is difficult to present typical examples. We should have been tempted to leave this volume to the obscurity it merits from a scientific point of view, but for two circumstances. One is, that this book will probably circulate largely among readers not qualified to judge of the rashness of statement and inaccuracy of detail that characterize its astronomical portion, and that consequently a very erroneous and exaggerated opinion may be formed of the character and amount of the errors that still exist in one of the most exact of sciences. The second inducement to look a little closely into its pages is this: that another and more instructed class of readers may imagine that on matters of chronology astronomy speaks with an uncertain sound, and consequently be led to undervalue the very substantial advantages that history has derived from astronomical sources.

The main object of the book is the arrangement of a system that shall bring the narrative contained in the Gospels into the chronological order conceived by the author as correct, and to render consistent, the facts recorded in sacred and secular history, with this system. How far this method and system will satisfy competent theological critics it is, as we have said, not our duty to inquire; we can only hope that the service rendered to religion is greater than that to science, for from the latter point of view we have no hesitation in saying that his theory is erroneous in its conception and unwarranted in its application.

The means employed to produce this chronological harmony is based on the assumption that the places of the sun and moon cannot be correctly computed for distant dates from the existing tables, and that consequently additional terms, empirically determined, must be introduced. This new theory had best be described in the author's own words, for fear we should not do it justice:—

"Our present lunation is too long by a fraction of a second, amounting in the course of a century, to about six minutes of time. In the same length of time, the sun's anomaly is too long by about seven minutes ten seconds of space, the moon's anomaly too long by eight minutes twenty seconds of space, and the sun's mean distance from the node is too short by about eight minutes thirty-five seconds of space."

After an attentive perusal we have not been able to discover any additional explanation or reason for the introduction of these terms. Neither have we discovered to what assumed values of the mean longitude, the mean anomaly, and the argument of latitude these corrections are to be applied. The only references to authorities are apparently those of Baily's "Tables" and Fergusson's "Astronomy," and the author does not appear to have had access or thought it worth while to examine more modern and trustworthy sources. We cannot be quite sure that we have described correctly the elements of the lunar and solar orbits to which these corrections are to be made, but it is asserted that, when introduced into the tables, all the eclipses recorded by the ancients can be represented correctly within a few minutes of time. It is much to be regretted that no rigorous comparison between the observed and computed times of all the ancient eclipses has been attempted, in order that a correct judgment might be formed of the value of this assertion. This was the more necessary as the few cases selected are, we think, very infelicitous, and the incapacity of modern tables to represent these eclipses is unjustifiably, but no doubt unintentionally, exaggerated.

It is curious to notice that the author does not recognize any other criterion of accuracy than the possibility of satisfying these ancient eclipses, the records of which are so imperfect, and the interpretation so doubtful, that they are gradually being discarded in the discussion of the one question for which they at one time seemed peculiarly fitted—namely, the determination of the amount of the secular acceleration of the moon's mean motion. The whole mass of modern observation is ignored. The careful records of eclipses made at Bagdad and Cairo in the ninth and tenth centuries share the same fate. It would seem that any observation made after the first half of the first century does not appear to the author to possess any value.

It will scarcely be believed that this is a correct description of the author's method. No one will imagine that any sane man would attempt to construct a lunar theory from ancient eclipses alone, and expect that the results at which he has arrived will be generally admitted, because, forsooth, he is able to represent a few facts by the introduction of nearly as many variables. It is true that the tables founded on this vicious reasoning do not appear in their integrity, and probably do not exist; but there are given many pages of computation, which are well calculated to mislead the uninstructed, and to give an air of accuracy to the results, to which they are not entitled. We can imagine nothing better adapted to bring astronomy into disrepute with thoughtful, but not mathematically trained minds, than the unwarranted conclusions presented in the slovenly manner in which they appear here.

AA

Some grounds must be given for the severe stricture here passed, and the only difficulty is to select the most fitting examples from so much worthless matter. On p. 18 the author says: "It is considered sufficiently near to the truth, if our calculations came within *a few hours* of the time and near enough to the quantity of the eclipse to identify it as being in all probability the obscuration mentioned by the historian in connection with a certain event." The italics are our own, and the statement to which they call attention is absolutely a misrepresentation. It is scarcely necessary to say in these columns that no astronomer of repute would be satisfied with a discrepancy of anything like this amount between history and computation in any case in which the phenomenon is clearly indicated and accurately described. In the annexed table is given the comparison of the computations of various astronomers of the times of historic eclipses with the recorded times. To keep the table to a moderate length it is confined to those dates between which the examples have been worked out by the writer. In estimating the accuracy of representation, there are two circumstances to be taken into account. One is that an eclipse, being a phenomenon the exact time of whose occurrence could not be accurately predicted by the observer or recorder, must have been in progress some time before detection, or, all observations of the first geometrical contact, the phase computed from the tables, would be observed too late; and though the error from this cause would not be so large in the observation of the end of the total phase, it is probable that this phenomenon would be recorded too soon. The other circumstance is that we cannot regard Ptolemy, from whose work the times here given have been taken, as a totally unprejudiced witness. He was anxious to establish a theory, and it is probable that he selected those instances which most nearly fitted his preconceived system. In other words he may have—what is not unknown in these days—rejected a discordant observation.

Date.	Phase given in "Almagest."	Recorded Greenwich mean time.	Greenwich mean time computed by Newcomb.	Zech.	Oppolzer.	Ginzel.
		h. m	h. m	h. m.	h. m.	h. m.
− 490, April 25 ...	Middle	8 27	8 17	7 50	7 53	7 35
− 382, Dec. 22 ...	Beginning	15 35	15 52	16 19	16 15	16 7
− 381, June 18 ...	Beginning	5 8	4 25	4 54	5 9	4 40
− 381, Dec. 12 ...	Beginning	5 56	4 57	6 30	6 18	6 14
− 200, Sept. 22	Beginning	3 23	2 57	—	—	—
	End	6 25	5 55	6 29	6 24	6 14
− 199, Mar. 19 ...	Beginning	9 29	8 51	9 22	9 20	9 9
− 199, Sept. 11 ...	Middle	12 22	12 3	12 34	12 28	12 18
− 173, April 30	Beginning	10 48	10 4	10 36	10 16	10 24
	End	13 31	12 45	13 12	13 20	13 3
− 140, Jan. 27 ...	Beginning	8 7	6 44	7 8	7 6	7 5
+ 125, April 5 ...	Middle	6 30	6 36	6 59	6 54	6 51

It is needless to point out there are no discrepancies of a few hours between the tabular and observed facts, and that the grave charge of the lack of accuracy is unsustained. The circumstances of two of these eclipses have been worked out by the author with some pretence of detail, employing his "new and corrected tables." For these two eclipses, − 382, Dec., and − 200, Sept., he gives the London mean times of the true full moon 15h. 56m. and 3h. 16m. respectively. There is no attempt to determine the exact phase observed, and it may be remarked that the longitude given for Babylon is grievously in error. These two eclipses have been selected with the particular purpose of demonstrating that no secular acceleration of the moon's motion exists. This selection, with this view, is unhappy. With regard to the earlier eclipse, it is very doubtful if it was really seen at Babylon. The account given in the "Almagest" ("Halma," p. 275) rather suggests that Athens, or one of the Ionic colonies, was the place of observation, since the description of the date is by means of the Greek calendar; and Hipparchus says that this eclipse with the two immediately following are added to the catalogue of the Babylonian eclipses as though they had been observed in that place (ὡς ἐκεῖ τετηρημένας γεγονέναι). This suggestion that the record of the eclipse was made elsewhere than at Babylon is strengthened by the addition of the note that "the moon set eclipsed." In an eclipse which commenced only half an hour before the setting of the moon, these words would have little meaning, but if the note was added by the observer at Athens, its purpose is intelligible, for the eclipse would be more than half over before the moon touched the horizon. It is very possible, therefore, that some allowance for longitude was made by Hipparchus, but with such a doubt overhanging the recorded time of observation, the selection of this eclipse from the long catalogue collected by Ptolemy gives a very doubtful support to any hypothesis. The second eclipse quoted was doubtless observed at Alexandria, but if Hipparchus is correctly rendered by Ptolemy, he is made to say that the eclipse began half an hour before the moon rose. The record, therefore, refers to a calculated, and not an observed, phenomenon, and on that ground alone should not have been selected.

But it is in solar eclipses, the total phase being confined to a comparatively narrow zone of country, that the feebleness of the author's method is most conspicuously exhibited. The eclipse known as that of Xerxes will serve for an example. To adequately explain the circumstances as recorded by Herodotus and Aristides has exercised the ingenuity, but baffled the efforts, of many experts. It offers no difficulties to Mr. Page, though we cannot think that his rendering will be generally appreciated. Herodotus's description runs, "The army having come out of their winter-quarters in the opening of spring." In the latitude of Sardis the opening of spring could hardly be put as late as April 18, but this is the date selected by Mr. Page, because on that day − 480 there was undoubtedly a total eclipse of the sun. The writer does not mention, what is equally the fact, that the shadow of the moon first touched the earth in the Indian Ocean, passed over the Himalayan peninsula, through China, and disappeared in the Pacific. Such a path is totally inadequate to explain the further description of Herodotus, that "night came on instead of day."

A still greater absurdity is introduced when the author wishes to prove that the death of Augustus happened in the year 13, by means of a solar eclipse which is said to have occurred just before the death of that Emperor. He

finds that there was a solar eclipse on 13, April 28, and an attractive woodcut is given showing the track of the shadow passing over Rome. As a matter of fact, this eclipse began in the Pacific, touched the continent of America about Vancouver, and passed over Canada to the Atlantic: the whole of its path is confined to "regions Cæsar never knew." But the list of false deductions is too long and too uninteresting to pursue any further: exact astronomy can lend no support to the chronological system here developed.

WILLIAM E. PLUMMER.

THE EVOLUTION OF SEX.

The Evolution of Sex. By Prof. Patrick Geddes and J. Arthur Thomson. With 104 Illustrations. (London: Walter Scott, 1889.)

THIS book, say the authors in the preface, has "the difficult task of inviting the criticism of the biological student, although primarily addressing itself to the general reader or beginner." In attempting to meet these two interests the authors have aimed high: they have aimed at producing a classic. They have brought to the task—as indeed their names guarantee—a wealth of knowledge, a lucid and attractive method of treatment, and a rich vein of picturesque language. The illustrations are pertinent, and sometimes very good. The index and table of contents are copious, and the summaries and references to literature at the end of each chapter are most useful. In matters of history they are especially good, and advanced biological students will find the abstracts of the views of Eimer, Weismann, Brooks, Hertwig, Haeckel, Wallace, Spencer, Geddes, and many others exceedingly useful. But as writers for the general public the authors have serious if not prohibitive disadvantages.

General readers demand, with right, that those who speak to them with the voice of authority shall give them the authoritative views. Controversial matter they are only remotely interested in, and when it cannot be avoided they must have it carefully distinguished from matter beyond controversy. These authors are controversialists from the first page of their book to the last: they are partisan controversialists offering their wares and their wisdom as accredited doctrine and determined result. This is no quarrel with the views of the authors. Prof. Geddes and Mr. Thomson are workers well able to command the attention of biologists for their contributions to any controversy. It is a quarrel with the offering of personal views, generalizations, and theories as final, in a series "designed to bring within the reach of the English-speaking public the best that is known and thought in all departments of modern scientific research."

As is the fashion with neo-Lamarckians, the authors delight in obtruding their misconceptions of Darwin. Take, for instance, the following statements:—

"Arguing from the bad effects of close-breeding among higher animals, Darwin and others have called attention to the numerous contrivances among plants which are said to render self-fertilization impossible. It must again be said that this survival of a very old way of explaining facts—in terms of their final advantage—is not really a causal explanation at all" (p. 74).

Or, again, on p. 27:—

"As a special case of natural selection Darwin's minor theory (*i.e.* sexual selection) is open to the objection of being teleological, *i.e.* of accounting for structures in terms of a final advantage. It is quite open to the logical critic to urge, as a few have done, that the structures to be explained have to be accounted for before, as well as after, the stage when they were developed enough to be useful. The origin, or in other words, the fundamental physiological import, of the structures, must be explained before we have a complete or adequate theory of organic evolution."

Now there can be no doubt of the question here at issue. Readers of NATURE may remember that some time ago (NATURE, December 12, 1889, p. 129) Prof. Ray Lankester *à propos* of Cope's supposed contribution to the theory of natural selection,[1] asked: "How can Mr. Cope presume to tell us this? Who has ignored it? When? and where?" It is clear that Prof. Geddes and Mr. Thomson imagine that Darwin has ignored this, and that he has done so in his theory of sexual selection, and in his accounts of contrivances in plants to prevent self-fertilization. In a set of works the definite and reiterated purpose of which is to show (1) that variations do occur, (2) that from these, by selection, varieties, species, organs are elaborated and adapted, it is fortunately easy to find chapter and verse conclusive against the view that Darwin could have imagined that selection teleologically causes the variations that give it scope. Will Prof. Geddes and Mr. Thomson refer to the "Descent of Man" (the writer has the second edition before him)? On p. 240 it is written:—

"Not only are the laws of inheritance extremely complex, but so are the causes which induce and govern variability. The variations thus induced are preserved and accumulated by sexual selection."

Will Prof. Geddes and Mr. Thomson refer to the "Fertilization of Orchids" (also second edition)? On p. 284 it is written:—

"Thus throughout nature almost every part of each living being has probably served in a slightly modified condition for diverse purposes, and has acted in the living machinery of many ancient and distinct specific forms."

Or, again, on the same page:—

"This change" (labellum assuming its normal position) "it is obvious might be simply effected by the continual selection of varieties which had their ovaries less and less twisted; but if the plant only afforded varieties with the ovarium more twisted, the same end could be attained by the selection of such variations until the flower was turned completely round on its axis."

Can there be the faintest suspicion that the man who wrote these sentences did not distinguish between the material for selection and the causes producing that material? One more quotation from the authors to show how they misunderstand Darwin's spirit and writings:—

"The first of these is the still curiously prevalent opinion that, when you have explained the utility or the advantage of a fact, you have accounted for the fact, *an opinion which the theory of natural selection has done more to foster than to rebuff. Darwin was indeed himself characteristically silent* in regard to the origin of sex as well as of many other 'big lifts' in the organic series" (p. 126).

[1] The key-note of Cope's imagined contribution was, "Selection cannot explain the origin of anything."

What do the authors mean? Their erudite and careful statements of the position of many foreign writers emphasize their failure to represent the position of the author of the "Origin of Species."

The authors think that the problems and questions relating to sex, problems and questions carefully and ingeniously analyzed by them, "are in final synthesis all answerable in a sentence." Morphological questions are at base, they say, physiological; and physiological questions are ultimately referable to the metabolism of protoplasm, as Prof. Burdon-Sanderson pointed out last autumn. This metabolism is double: it consists on the one hand of anabolic, constructive, elaborative processes—processes attended with the storage of energy; and on the other hand of katabolic, destructive, disintegrating processes—processes attended with the liberation of energy. These processes are complementary; in living protoplasm they seem for the most part coincident. Losing sight of the coincidence the authors have seized on the antithesis; the idea has grown upon them till they see a rhythm of anabolism and katabolism swinging through organic nature and producing—well, producing nearly everything.

Take, for instance, secondary sexual characters. Males are frequently lithe, active, aggressive, gorgeously coloured and decorated. Females are often sluggish, vegetative, passive, and soberly coloured. These characters, according to Geddes and Thomson, occur because males have a male or katabolic diathesis, because females have a female or anabolic diathesis.

"Brilliancy of colour, exuberance of hair and feathers, activity of scent glands, and even the development of weapons, are not and cannot be (except teleologically) explained by sexual selection, but in origin and continued development are outcrops of a male as opposed to a female constitution" (p. 22).

It is impossible to follow in detail and state the innumerable objections to this explanation. Do the authors suppose a male diathesis explains the ascending series of horn and antler development? Can it in any way account for "interference" colours, which play so large a part in the adorning of males? Are women less female when they have radiant complexions and abundant tresses? What physiological reason is there for believing that skeletal weapons and scent glands, or the crystals in anthers, are due to the katabolism of "exuberant maleness," while menstruation and lactation are means of getting rid of "anabolic surplus?"

Parthenogenesis occurs in groups of animals where the anabolic rhythm is dominant. Sex itself appears when katabolic conditions preponderate. And this is why flowers so often are situated at the end of the vegetative axis; this is furthest from the source of nutrition; the flower occupies a katabolic position, and is often the plant's dying effort (p. 226). Alternation of generations is a special example of the rhythm. Thus, but the authors do not cite this example in this connection, the tiny sexless and spore-bearing stalk parasitic on the moss-plant is the anabolic vegetative generation, while the conspicuous moss-plant is the sexual or katabolic generation—the generation peculiarly connected with starvation! It is obvious that the authors are nothing if not original. But the real value of the book must not be lost sight of in quotations from it. The chapters on the "Determination of Sex," on "Sex Elements," and on "Growth and Reproduction," are very suggestive. But indeed, to biologists the greater part of the book and its theories must be useful and suggestive. It is only the general public that must be warned off.

It is very much to be regretted that the authors have included a discussion of certain social and ethical problems absolutely unconnected with the title of their book. If such matters are to be discussed *coram populo*, it is only fair that explicit information should appear on the title-page.

P. C. M.

THE QUICKSILVER DEPOSITS OF THE PACIFIC SLOPE.

Geology of the Quicksilver Deposits of the Pacific Slope. By G. F. Becker. Pp. 486, and Atlas of xiv. folio Plates. (Washington: Government Printing Office, 1888.)

AMONG the numerous mineral treasures of California none are of more interest than the deposits of mercury ore which occur at intervals along the greater part of the Coast Range from the Mexican boundary to Clear Lake, in lat. 39° N., a distance of more than 200 miles. This region, together with the district of Steamboat Springs in Nevada, has been carefully examined by the division of the United States Geological Survey under the charge of Mr. G. F. Becker, and the results are now presented in another of the handsome quarto series of monographs published by Major Powell, the head of the Survey.

The discovery of mercury in California preceded that of gold; the most productive locality, New Almaden, near San José, at the south end of the Bay of San Francisco, having been known for about 65 years, while the actual mining was commenced under a grant from the Mexican Government shortly before the cession of the country to the United States. In its earlier years the mine was extremely profitable, and the long judicial controversy that ensued before the title was satisfactorily established occupies a prominent place among the records of American mining litigation. The maximum production of 47,194 flasks of 76½ pounds each was realized in 1865, but in 1886 it was reduced to 18,000 flasks, the total for the period 1850-86 being 853,259 flasks, or about two-thirds of the produce of the Spanish Almaden. The total produce of the Californian mines, which was about 80,000 flasks in 1877, declined to 30,000 in 1886.

The second mine in point of importance, known as New Idria, is about 70 miles in a south-easterly direction from New Almaden, the ore, cinnabar, occurring under conditions similar to those in the latter mine—namely, in very irregular groups of fissures in metamorphic strata, which pass into others containing Neocomian fossils of the genus *Aucella*. These were succeeded by other Cretaceous and Tertiary formations up to the Miocene, the close of the latter period being marked by an upheaval and the commencement of volcanic activity. The ore deposits are closely related to the latter, and are probably nearly all, if not entirely, of post-Pliocene origin.

In the Clear Lake region, in lat. 39° N., which adjoins the group of volcanic cones known as Mount

Konocte (or Uncle Sam) hot springs and solfataras are abundant in a small area of basalt of comparatively recent origin. The most important of these, known as the Sulphur Bank, was at first worked for sulphur, but, on getting below the surface, cinnabar was found in the decomposed basalt, and for some years it produced large quantities of mercury, up to 11,152 flasks in 1881; but latterly the yield has fallen off, being only 1449 flasks in 1886.

The Redington Mine, adjoining Knoxville, about 25 miles south-east of Clear Lake, was discovered in making a cutting for a road, and has been worked since 1862, and has produced nearly 100,000 flasks of mercury, a quantity which has only been exceeded by the mines of New Almaden and New Idria. In 1886 the yield had fallen to 409 flasks, the immense irregular body of ore at the surface having changed in depth to some narrow veins following fissures in the metamorphic Neocomian strata. These are to a large extent converted into serpentine; and a black opal, known as quicksilver rock, accompanied the ore, which was remarkable as consisting largely, in the upper workings at least, of amorphous black sulphide of mercury, or meta-cinnabar, a mineral that was there recognized in quantity for the first time. This deposit is considered to be the result of the action of hot springs in connection with an adjacent mass of basalt—springs which are now dormant except in so far that sulphur gases are given off and sulphur crystals are deposited in the old workings, where a comparatively high temperature, exceeding 100° F., prevails.

The Steamboat Springs in Nevada, near the Comstock lode, have been also studied by the author. These, although presenting no deposits of commercial value, are interesting from the light they cast upon the phenomena of the formation of mineral veins, and have therefore been carefully investigated by several observers, including the late Mr. J. A. Phillips, F.R.S., and M. Laur, of the École des Mines. The author considers that the main source of the ore in the Comstock lode is the diabase forming the hanging wall, and that the mineral contents were extracted from this pre-Tertiary eruptive mass by intensely heated waters charged with alkaline carbonates and sulphides rising from great depths, and that a similar origin may properly be attributed to all the cinnabar, pyrites, and gold found in the mercury-mines of the Pacific slope, having been brought in as solutions as double sulphides of metal and alkalies. The original source must have been either the fundamental granite of the country, or some *infra*-granitic mass, it being extremely improbable that they were extracted from any volcanic rock at or near the surface. In connection with this subject, the author has made a series of interesting experiments on the relations of the sulphide of mercury to that of sodium, which show that mercuric sulphide is freely soluble in aqueous solutions of sodium sulphide, although the contrary has repeatedly been asserted. Mercuric sulphide may be precipitated from sulpho-salt solutions in many ways, particularly by excess of sulphuretted hydrogen, by borax and other mineral salts; by cooling, especially in the presence of ammonia, and by dilution. In the latter case, a certain quantity of metallic mercury separates as well as the sulphide, indicating one of the methods by which the native metal has been produced in Nature.

In addition to the mines specially described, the author has extended his study of the subject to a consideration of the principal mercury-mines other than those of America, partly from personal investigation in Spain and Italy, and partly with the help of other observers and published accounts. He expresses a very decided opinion against the supposed substitution origin of the Almaden deposits, considering them to be essentially of a vein-like character, the cinnabar being deposited in fissures or interstitial cavities in sandstone previously existing. This latter conclusion is substantially similar to that arrived at by the late Mr. J. A. Phillips and the present writer, in a microscopic study of the Almaden ores made some years since. The details of the foreign deposits have been very carefully collected, the comparatively new discoveries of Avala in Servia, and Bakmuth in Southern European Russia, being included. The latter mine, which, at the time the book was completed, was not at work, has since become of considerable importance. The ore, cinnabar, occurs as an impregnation of a bed of carboniferous sandstone from 14 to 17 feet thick, with an average yield of 154 pounds per ton—about 7 per cent.—and the reduction works have a productive capacity of about 10,000 flasks annually.

In conclusion, it is scarcely necessary to state that the whole of the details illustrating the subject have been worked out with the care and fulness which have characterized the author's former monograph on the Comstock lode. Whether mercury-mining in California may be in a declining state, or destined to a revival of its former prosperity at a future time, there can be no question of the high value of the record of the results hitherto obtained, which is contained in the volume it has been our pleasant task to notice. H. B.

OUR BOOK SHELF.

Illustrations of some of the Grasses of the Southern Punjab, being Photo-lithographs of some of the Principal Grasses found at Hissar. By William Coldstream, B.A., Bengal Civil Service. With 38 Plates and 8 pages of Introduction. (London: Thacker and Co. Calcutta: Thacker and Spink. 1889.)

THIS work contains a series of thirty-eight photo-lithographs of the grasses used for agricultural purposes in the southern portion of the Punjab. The tract of country to which it relates lies to the west of Delhi, between the Jumna on the east and the Sutlej on the west. It constituted till recently the civil district of Hissar, which has now been broken up. It has an area of 8500 square miles, and a population of a million and a half. Except along the streams and canals the soil is sterile and sandy, and the crops depend upon the periodical rains. The staple cereals are *Sorghum vulgare* and *Penicillaria spicata*. In its centre is situated the great Government cattle-farm of Hissar, where for many years cattle of the finest Indian breeds have been reared by Government, principally for the supply of the ordnance and transport departments, but also to some extent for distribution through the country, with the aim of improving the commoner indigenous kinds. The *Bir*, or grass-lands, of this great farm are of very wide extent, and in the rainy season a large number of grasses, of more or less value as fodder, grow luxuriantly over its vast parks. The farm has altogether an area of above sixty square miles, and it is mainly from this that the species figured by Mr. Coldstream are taken.

The book is modelled upon the "Fodder-grasses of India," published not long ago, in two volumes, by Mr. Duthie, the director of the botanical department of Northern India, and to Mr. Duthie the author is indebted for the botanical determination of the species. He gives the native name of each plant, and a short account of the extent and manner in which it is used, and as most of them have a wide dispersion, this will be found useful in other dry sub-tropical regions. Out of thirty-seven species, the two great tropical tribes are represented, *Paniceæ* by twelve species, and *Andropogoneæ* by ten, and only three species fall under *Festuceæ*, the tribe to which most of our North European pasture grasses belong. The plates are lithographed from photographs, and do not contain any dissections. Plate III., called *Panicum Crusgalli*, is clearly not that species, but a form of *P. colonum*, another variety of which is figured on Plate II. Mr. Coldstream also has got entirely wrong with his two species of *Cyperus*, figured on p. 38. The left-hand figure, called *Cyperus species*, is evidently *Cyperus Ivia*, Linn., a common weed throughout India in rice-fields. The left-hand figure, labelled *Cyperus Tria*, is not in flower. There is no such plant known to botany; *Tria* is doubtless a mistake for *Ivia*. The figure is quite unrecognizable, but from the native name appended, "Motha," it is most likely *Cyperus rotundus*.

J. G. B.

Elementary Dynamics of Particles and Solids. By W. M. Hicks, M.A., F.R.S. (London: Macmillan and Co., 1890.)

In this excellent treatise, extending over nearly 400 pages, the author introduces to the student the principles of dynamics. Although the book is issued under the latter title, it will be found to differ considerably in its treatment from the majority of text-books on the same subject. For instance, the two subjects of statics and kinetics have been considered together, the former being regarded as a special case of the latter. Again, the discussion of force is reserved until an attempt has been made to give an idea of mass and its measurement; thus a preliminary study of momentum finds an early place.

Although the mathematical acquirements of the student of these pages may be limited to a knowledge of the elements of algebra and geometry, he will be able to readily follow the methods adopted in establishing the various results. This the author has kept in view throughout his work, except in a few cases where, in the hope of rendering it useful to a larger circle of readers, he has had recourse to the trigonometrical ratios for examples which he has worked out.

The volume is divided into three portions (1) rectilinear motion of a particle; (2) forces in one plane; (3) plane motion of a rigid body.

One cannot read the first few chapters without observing the care taken by the writer in trying to impart to the student a correct and precise idea of the fundamental units. That this is a very important matter all will agree who have had any experience in teaching or testing students. The most deplorable state of ignorance sometimes exhibited by them, in giving their results in all manner of absurd units, should encourage both teacher and author to make a special effort when dealing with the question of units, fundamental or otherwise.

As the subject of statics is included, an opportunity has been taken of introducing the method of drawing stress diagrams for loaded framework; this will be valuable to engineering students.

Notwithstanding that the writer has forbidden himself the use of the integral calculus, he has been able to establish (in some cases very neatly) many useful results in the two chapters on centre of gravity and moment of inertia, which should be read with care.

Neatness in method characterizes the book throughout and an unusually large number of examples will be found at the end of each chapter.

The work is based on a series of lectures delivered by the author at the Firth College, Sheffield, and many details for which time can generally be found at the lecture table have in this case found their way into the book.

These will help to lessen the individual difficulties of students, and their views of the subject will be enlarged thereby. There can be little doubt that the text-book will have a deservedly favourable reception.

G. A. B.

Catalogue of the Fossil Reptilia and Amphibia in the British Museum (Natural History). Part III., containing the Order *Chelonia.* By Richard Lydekker, B.A., F.G.S., &c. (London: Printed by Order of the Trustees, 1889.)

Mr. Lydekker is to be congratulated on having added one more to the valuable series of catalogues of the palæontological collections in the British Museum which he has compiled during the last few years. Like his previous catalogues, the present work indicates an enormous amount of careful and accurate work, which, however, is of such a special kind that it cannot easily be summarized in a short review.

The extreme difficulty of correlating the fossil forms of Chelonia with the recent, on account of the fragmentary character of many of the remains, is indicated by the fact that, out of the 52 genera and 131 species or varieties described, the author has only been able to place with certainty 18 genera and 10 species amongst existing forms. The classification adopted is to a great extent that followed by Mr. Boulenger in his catalogue of recent Chelonians. The work is illustrated by 53 woodcuts, and abundant references to the bibliography of the group are given. It must be added, as stated in the preface, that "the collection which forms the subject of this Catalogue is particularly rich in Chelonians from the Purbeck Beds of Swanage, the Cretaceous of England and Holland, the Eocene Tertiaries of Warwick, Sheppey, Hampshire, the Isle of Wight, and the older Pliocene of the Siwaliks of India." The last-named beds have yielded the largest tortoise known (*Testudo* [*Colossochelys*] *atlas* of Falconer), the carapace of which measures about six feet in length.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Systems of "Russian Transliteration."

As one who takes an interest in the Russian tongue, quite apart from the value of the scientific papers published in that language, I may perhaps be allowed to express my regret that the author of "A Uniform System of Russian Transliteration," published in your issue of February 27 (p. 397), has departed in almost every point where it is possible to do so from the system of transliteration which has been in use in England for about a century, and which has, moreover, the advantage of being almost identical with that current in France.

A system of transliteration may be founded on one of two bases—namely, the *empirical*, in which little or no account is taken of the sound of the letters in the foreign language, and the *rational*; in the latter the letters of the foreign language are, where possible, represented by letters or groups of letters which have as nearly as may be the same sound as the original. For instance, В in Russian would be represented by *B* in English, these two having the same sound. It seems to me that the latter is the most convenient system, and the one which ought to be

generally adopted; the author of this new "uniform system," however, has chosen the other course.

If the author of the "uniform system" had been contented with tabulating the system of transliteration which has been so long in use, he would have earned the gratitude of those devoted to literature, as well as of those who cultivate science. As it is, I am afraid he has merely given the world of art and letters an opportunity for gibes at what they are sometimes pleased to call the narrowmindedness and pedantry of scientific men.

I may, perhaps, be permitted to give a few examples of the defects of the new system; Г in Russian has three sounds, one nearly resembling the English *g*, another very like *h*, and a third guttural sound, to which there is nothing analogous in our tongue. The author proposes to get over this by transliterating Г by *gh!!* The eminent chemist Hemilian thus becomes masked as Ghemilian, whilst Gustavson appears as Ghustavson, and a well-known political character, Gortchakoff, is altered to Ghorchakov'. For comparison, I give these names, and a few others, as transliterated in accordance with the two systems:—

Present system,	New system.
Hemilian	Ghemilian.
Gustavson	Ghustavson'.
Gortchakoff	Ghorchakov'.
Alexéeff	Aleksyeev'.
Gregoreff	Ghrighor'ev'.
Ogloblin	Oghloblin.
Mendeléeff	Mendelyeev'.
Chroushtchoff	Khrushchov'.
Michael	Mikhail.
Joukovsky	Zhukovskiĭ.

Geographical names are even more weird; for example, it becomes somewhat difficult to recognize under the disguise of Nizhniĭ Novghorod and Volgha, the town of Nijni Novgorod and the River Volga. Such words as "Journal" and "Chemie," when occurring in titles, can be at once recognized; this can scarcely be said of them if the new system of transliteration is used, as they become "zhurnal" and "Khimii" respectively.

It is much to be regretted that the Royal Society, the Linnean Society, and the Geological Society should have pledged themselves to adopt this novel "system of transliteration," instead of adhering to the one which has been so long in use. As a Fellow of the Royal Society, I feel very great regret that the Council are going to adopt this system in their publications, as it will seriously detract from the value of their supplementary "Catalogue of Scientific Papers" now in the press, at all events as far as Russian literature is concerned.

No protest of mine, however, can be half so forcible as the unconscious sarcasm of the author himself, in his paper, where he says that "an expression of grateful thanks is due" to two Russians "who have assisted in the arrangement of the system." The names of the Russians are then given, and if my readers will take the trouble to study them by the light of the table for transliteration by the new system, he will see how they express their appreciation of the author's labours by *carefully avoiding* every one of the novelties he has introduced.

CHARLES E. GROVES,
Editor of the *Journal of the Chemical Society*.

Burlington House, March 17.

HAVING in view the increasing importance of Russian to literary and scientific men, it becomes very desirable to have a uniform system of transliteration, such as that recently proposed in your columns.

But, in order to be useful, everyone must agree to conform to it, nor should any such system be adopted off-hand without full discussion of any points which may seem susceptible of improvement.

It seems to me objectionable to indicate the semi-vowels (ъ and ь) by a simple ', and to omit them altogether at the end of a word. They really correspond, to a certain extent, to our *e* (mute); and I would suggest that it would be better to indicate them by a full letter—perhaps *ĕ* for one and *ê* for the other.

March 11. W. F. KIRBY.

ONE or two points in the criticisms on this subject call for some notice before the publication of a more detailed account of the system.

As regards Mr. Kirby's suggestion, the transliteration of the semi-vowels was discussed, but it was not thought advisable to exaggerate their importance by using two letters for them, especially as their use is becoming discontinued in Russia.

When recommending a uniform system, we did not imagine that Mr. Groves or anyone else would infer that this was intended to limit the right of Russians who dwell in England or who write in English to spell their names as they please; we have not asked Messrs. Kelly to apply it to all Russian names in the Post Office Directory or the Court Guide; we should never think of altering such names in ordinary correspondence. Even in catalogues and records, for which this system is intended, the familiar form should of course be quoted with a cross reference, as recommended by us in the clause dealing with proper names.

Mr. Groves asks why we have not tabulated "the system which has been in use in England for about a century." Our efforts began with an attempt to discover such a system, and resulted in the tabulation of a large number of systems, including that employed by Mr. Groves in the *Journal of the Chemical Society;* since, however, no two authors agree in the English symbols intended to represent either the sounds or letters of Russian words, we endeavoured to frame a system combining as far as possible the features of those already in use in England and America.

We are much obliged to Mr. Groves for supplying further illustrations of the desirability of using *gh* for Г; the letter has, of course, more than the three sounds to which he limits it.

The uniformity of "the system which has been so long in use" may be illustrated by the following examples, in which we confine ourselves to the names of chemists, and to the words quoted by Mr. Groves:—

Consulting the "Imperial Gazetteer," Lippincott's "Gazetteer," and Keith Johnston's "Atlas" alone, we find Nijni, Nijnei, Nishnii, Nizhnee, Nijnii, and Nischnii-Novgorod.

One journal is given in Bolton's "Catalogue of Chemical Journals" as

Zhurnal russkova khimicheskova i fizicheskova;

in the *Geological Record* as

Jurnal rosskoi chimitcheskago i phizitcheskago;

and in Scudder's "Catalogue of Serials" as

Zhurnal; russkoye khimitcheskoye i fizitcheskoye.

Hence it is difficult to see why Nizhniĭ and Zhurnal should be unintelligible.

In the Royal Society Catalogue, the *Geological Record*, and Chemical Society's Journal, the same name is spelt Jeremejew, Jeremejeff, Jereméeff. Which of these words represents the pronunciation?

In the Chemical Society's Journal, Wroblewski and Flawitzky correspond to the Wroblevsky and Flavitzsky of Armstrong and Groves' "Organic Chemistry."

The same journal frequently quotes the name Markownikoff, where the same Russian letter (and sound) is denoted both by *w* and *ff*, while in the examples of Mr. Groves it is also represented by *v*; here, of course, and in similar cases, the name comes through a German channel.

Mr. Groves transliterates a few names; since, however, in his "rational" system one Russian letter has more than one English equivalent (*v*, *ff*), and one English letter (*e*) has more than one Russian equivalent, while the sound is not correctly represented (*o*, *é*), it is obvious that this is neither "rational" nor a system (it does not profess to be "empirical"; perhaps Mr. Groves will now call it the "graphic method").

Since, moreover, the system recommended by Mr. Groves is not used by him in the Chemical Society's Journal, we hope that he may yet see his way to adopting the one which has now been accepted by so many of the leading English Societies.

H. A. M.
J. W. G.

"Like to Like"—a Fundamental Principle in Bionomics.

THE following letter has been intrusted to me for seeing through the press, and therefore I deem it desirable to state that it does not constitute the writer's reply to Mr. Wallace's criticism of his paper on "Divergent Evolution." This reply, as previously stated (NATURE, vol. xl. p. 645), will be published by him on some future occasion.

I cannot allow the present communication to appear in these columns without again recording my conviction that the writer is the most profound of living thinkers upon Darwinian topics, and that the generalizations which have been reached by his twenty years of thought are of more importance to the theory of evolution than any that have been published during the post-Darwinian period. GEORGE J. ROMANES.

London, March 10.

I FOLLOW Prof. Lankester in the use of bionomics to designate the science treating of the relations of species to species. If the theory of evolution is true, bionomics should treat of the origin, not only of species, but of genera, and the higher groups in which the organic world now exists.

In his very suggestive review of "Darwinism," by Mr. A. R. Wallace, in NATURE of October 10, 1889 (p. 566), Prof. Lankester refers to "his (Mr. Wallace's) theory of the importance of the principle of 'like to like' in the segregation of varieties, and the consequent development of new species." Prof. Lankester has here alluded to a principle which I consider more fundamental than natural selection, in that it not only explains whatever influence natural selection has in the formation of new species, but also indicates combinations of causes that may produce new species without the aid of diversity of natural selection. The form of like to like which Mr. Wallace discusses is "the constant preference of animals for their like, even in the case of slightly different varieties of the same species," which is considered not as an independent cause of divergence, but as producing isolation which facilitates the action of natural selection. If he had recognized this principle, which he calls selective association, as capable of producing in one phase of its action sexual and social segregation, and in another phase sexual and social selection, he would perhaps have seen that its power to produce divergence does not depend on its being aided by natural selection.

Mr. Wallace's view is very clearly expressed in the following passages, though I find other passages which lead me to think that the chief reason he does not recognize segregation as the fundamental principle in divergence is that he has not observed its relations to the principle of like to like. He says :—"A great body of facts on the one hand, and some weighty arguments on the other, alike prove that specific characters have been, and could only have been, developed and fixed by natural selection because of their utility" ("Darwinism," p. 142). "Most writers on the subject consider the isolation of a portion of a species a very important factor in the formation of new species, while others maintain it to be absolutely essential. This latter view has arisen from an exaggerated opinion as to the power of intercrossing to keep down any variety or incipient species, and merge it in the parent stock" ("Darwinism," p. 144).

I think we shall reach a more consistent and complete apprehension of the subject by starting with the fundamental laws of heredity, and refusing to admit any assumption that is opposed to these principles, till sufficient reasons have been given. Laws which have been established by thousands of years of experiment in domesticating plants and animals, should be, it seems to me, consistently applied to the general theory of evolution. For example, if in the case of domesticated animals, "it is only by isolation and pure breeding that any specially desired qualities can be increased by selection" (see "Darwinism," p. 99), why is not the same condition equally essential in the formation of natural varieties and species? If in our experiments we find that careful *selection* of divergent variations of one stock does not result in increasingly divergent varieties *unless free crossing between the varieties is prevented*, why should it be considered an exaggeration to hold that in wild species "the power of intercrossing to keep down any variety or incipient species, and merge it in the parent stock," is the same. Experience shows that *segregation, which is the bringing of like to like in groups that are prevented from crossing*, is the fundamental principle in the divergence of the various forms of a given stock, rather than *selection, which is like to like through the prevention of certain forms from propagating*; and I think we introduce confusion, perplexity, and a network of inconsistencies into our exposition of the subject, whenever we assume that the latter is the fundamental factor, and especially when we assume that it can produce divergence without the co-operation of any cause of *segregation* dividing the forms that propagate into two or more groups of similars, or when we assume that segregation and divergence cannot be produced without the aid of diverse forms of selection in the different groups. The theory of divergence through segregation states the principle through which natural selection becomes a factor promoting sometimes the stability and sometimes the transformation of types, but never producing *divergent* transformation except as it co-operates with some form of isolation in producing segregation; and it maintains that, whenever variations whose ancestors have freely intergenerated are from any combination of causes subjected to persistent and cumulative forms of segregation, divergence more or less pronounced must be the result. The laws of heredity on which this principle rests may be given in the three following statements:—

(1) Unlike to unlike, or the removal of segregating influences, is a principle that results either in extinction through failure to propagate, or in the breaking down of divergences through free crossing.

(2) Like to like, when the individuals of each intergenerating group represent the average character of the group, is a principle through which the stability of existing types is promoted.

(3) Like to like, when the individuals of each group represent other than the average character of the group, is a principle through which the transformation of types is effected.

In my paper on "Divergent Evolution" (Linn. Soc. Journ., Zoology, vol. xx. pp. 189–274), I pointed out that sexual and social instincts often conspire together to bring like to like in groups that do not cross, and that in such cases there will be divergence even when there is no diversity of natural selection in the different groups, as, for example, when the different groups occupy the same area, and are guided by the same habits in their use of the environment. There is reason to believe that under such circumstances divergence often arises somewhat in the following way. Local segregation of a partial nature results in some diversity of colour or in some peculiar development of accessory plumes, and through the principle of social segregation, which leads animals to prefer to associate with those whose appearance has become familiar to them, the variation is prevented from being submerged by intercrossing. There next ari es a double process of sexual and social selection, whereby both the peculiar external character and the internal instinct that leads those thus characterized to associate together are intensified. The instinct is intensified, because any member of the community that is deficient in the desire to keep with companions of that kind will stray away and fail of breeding with the rest. This process I call social selection. The peculiarity of colour or plumage is preserved and accumulated, because any individual deficient in the characteristic is less likely to succeed in pairing and leaving progeny. This latter process is sexual selection. It can hardly be questioned that both these principles are operative in producing permanent varieties and initial species; and in the circumstances I have supposed, I do not see how the process can be attributed to natural selection. Varieties thus segregated may often develop divergent habits in their use of the environment, resulting in divergent forms of natural selection, and producing additional changes; but so long as their habits of using the environment remain unchanged, their divergencies cannot be due to natural selection.

Mr. Wallace's very interesting section on "Colour as a Means of Recognition," taken in connection with the section on "Selective Association," already referred to, and another on "Sexual Characters due to Natural Selection," offers an explanation of "the curious fact that prominent differences of colour often distinguish species otherwise very closely allied to each other" (p. 226). His exposition differs from mine in that he denies the influence of sexual selection, and attributes the whole process to natural selection, on the ground that "means of easy recognition must be of vital importance" (p. 217). The reasoning, however, seems to me to be defective, because the general necessity for means of easy recognition is taken as equivalent to the necessity for a specialization of recognition marks that shall enable the different varieties to avoid crossing. In the cases I am considering, there is, however, no advantage in the separate breeding of the different varieties, and even in cases where there is such an advantage (as there would be if the variety had habits enabling it to escape from competition with the parent stock, but only partially preventing it from crossing with the same), it does not appear how this advantage can prevent the individual that is defective in the special colouring from following and associating with those that are more clearly marked. The significant part of the process in the development of recognition marks must be in the failure of such individuals to secure mates, which is sexual selection; or in the unwillingness of the

community to tolerate the company of such, which might be called social selection.

It is often assumed by writers on evolution that permanent differences in the methods in which a life-preserving function is performed are necessarily useful differences. That this is not so may be shown by an illustration drawn from the methods of language. The general usefulness of language is most apparent, and it is certain that some of the laws of linguistic development are determined by a principle which may be called "the survival of the fittest;" but it is equally certain that all the divergences which separate languages are not useful divergences. That one race of men should count by tens and another by twenties is not determined by differences in the environments of the races, or by any advantage derived from the difference in the methods. So easy recognition of other members of the species is of the highest importance for every species; but difference in "recognition marks" in portions of a species separated in different districts of the same environment is no advantage. Under the same conditions, habits of feeding may become divergent; but, since any new habit that may be found advantageous in one district would be of equal advantage in the other district, the divergence must be attributed to some initial difference in the two portions of the species.

I have recently observed that, of two closely allied species of flat-fish found on the coasts of Japan, one always has its eyes on the right side, and the other always on the left. As either arrangement would be equally useful in the environment of either species, the divergence cannot be considered advantageous.

Osaka, Japan. JOHN T. GULICK.

Self-Colonization of the Coco-nut Palm.

THE question whether the coco-nut palm is capable of establishing itself on oceanic islands, or other shores for the matter of that, from seed cast ashore, was long doubted; and if the recent evidence collected by Prof. Moseley, Mr. H. O. Forbes, and Dr. Guppy, together with the general distribution of the palm, be not sufficient to convince the most sceptical person on this point, there is now absolutely incontrovertible evidence that it is capable of doing so, even under apparently very unfavourable conditions.

In the current volume of NATURE (p. 276) Captain Wharton describes the newly-raised Falcon Island in the Pacific; and in the last part of the Proceedings of the Royal Geographical Society, Mr. J. J. Lister gives an account of the natural history of the island. From this interesting contribution to the sources of insular floras we learn that he found two young coco-nut palms, not in a very flourishing condition, it is true; but they were there, and had evidently obtained a footing unaided by man. There were also a grass, a leguminous plant, and a young candle-nut (Aleurites), on this new volcanic island—a very good start under the circumstances, and suggestive of what might happen in the course of centuries.

W. BOTTING HEMSLEY.

On Certain Devonian Plants from Scotland.

I AM indebted to Mr. James Reid, of Allan House, Blairgowrie, Scotland, for the opportunity to examine a collection of fossil plants obtained by him from the Old Red Sandstone of Murthly and Blairgowrie in Perthshire, some of which have been noticed by Dr. Geikie in his "Text-book of Geology."

The collection is remarkable for the striking resemblance of the matrix and the contained vegetable debris to those of the lower part of the Gaspé sandstones of Logan, and the species of plants are, so far as can be determined, the same.[1]

Psilophyton princeps largely predominates, as in Gaspé, and is represented by a profusion of fragments of stems and branches, and more rarely by specimens of the rhizoma and of the sporocarps. *P. robustius* is represented by fragments of stems, but is less abundant, and *Arthrostigma gracile* by some portions of stems. On the whole the assemblage is exactly those of the sandstone beds of the lower division of the Gaspé sandstones. There is nothing distinctively Upper Devonian in the collection.

The collection also contains two slabs of dark-coloured sandstone from Caithness, one of which contains what appears to be a fern stipe similar to those of the genus *Rhodea*. Another shows a remarkable plant having apparently a short stem giving origin to a quantity of crowded leaves which are long, narrow, and parallel-sided, and show only a very faint linear striation. This plant is identical both in the form and arrangement of the leaves with that found in the Devonian of Canada, and which I have named *Cordaites angustifolia*. I have, however, already stated in my Reports on the Flora of the Erian of Canada (Geological Survey of Canada, 1871 and 1882), that I do not consider this plant as closely related to the true Cordaites, and that I have not changed the generic name merely because I am still in doubt as to the actual affinities of the plant. Mr. Reid's specimens would rather tend to the belief that it was, as I have already suggested in the reports above cited, a Zostera-like plant growing in tufts at the bottom of water.

Some of the sandstone slabs from Murthly contain specimens of rounded objects referable to *Pachytheca* (Hooker), a genus of uncertain affinities but characteristic of Silurian and Lower Devonian beds on both sides of the Atlantic. One of these is perfectly spherical with a shining surface, and 2·75 mm. in diameter, the others have been broken so as to show a central cavity or nucleus about 1 mm. in diameter, and with a thick carbonaceous wall partly pyritised and showing obscure radiating fibres. Prof. Penhallow, of McGill University, has kindly examined these, and has compared them with slices of Pachytheca from the Wenlock limestone, kindly communicated by Mr. Barber, of Cambridge, and with specimens presented by Prof. Hicks from the Silurian of Corwen and with specimens in the author's collection from the Silurian of Cape Bon Ami; and also with the excellent figures in Mr. Barber's paper in the *Annals of Botany*. He has not been able, however, to arrive at any conclusions beyond the probable general similarity in structure of the various forms, which may, however, as Mr. Barber suggests, have differed in their nature and origin. The only thing certain at present seems to be that these puzzling organisms had a thicker outer coat of radiating fibres, and of so great density that it was less liable to compression than the other vegetable tissues with which it is associated.

A few small specimens sent more recently by Mr. Reid contain some curious but not very intelligible objects from the same beds. One is a stem coiled at the end very closely in a circinate manner. In form it resembles the circinate vernation of *Psilophyton princeps*, but is much larger. It may belong to *P. robustius*, or possibly to a fern, but is too obscure for certain determination. Several others appear to represent flattened fruits or sporangia of obovate form and of large size. One has a stalk attached with what seems a rudiment of a bract, and another shows obscure indications of having contained round or disk-shaped bodies about 2 mm. in diameter. All show minute longitudinal striation. I have not previously met with bodies of this kind in the Devonian, and can only suggest that they may represent the fructification of some unknown plant, possibly that to which Pachytheca belonged.

J. WM. DAWSON.

Montreal, March 5.

Exact Thermometry.

I AM glad to observe that Prof. Sydney Young and myself are now in substantial agreement as regards the tension theory of the ascent of the zero in thermometers, and approximately in agreement as regards the actual cause of the ascent in the neighbourhood of the ordinary temperature.

Some time ago, in connection with an investigation of melting-point, I devoted three years to an examination of the properties of the mercurial thermometer. Among other conclusions which then seemed to me probable, the application of the known plasticity of glass under pressure to account for the enormous ascent (in lead-glass) of the zero at high temperatures appeared of some value. I have never advanced it as a mature theory, and am perfectly open to correction on the subject; but neither Prof. Crafts (with whom I at that time discussed the matter), nor any subsequent experimenter, has submitted the suggestion to a crucial examination.

Prof. Young's experiments (NATURE, March 27, p. 489) are very interesting as far as they go; but the kind of glass of which his thermometers are constructed is not that which brings out the peculiarities of the material in their most striking development. This, indeed, has long been known. It may well be that, in German soda-glass, the plasticity is masked by a preponderating tendency of the harder or more crystalline silicates of the bulb to set. Much could be done towards settling the question as to plasticity, if three thermometers of lead-glass—one vacuous,

[1] See papers by the author, Journal Geol. Society, London, 1859, and Proceedings Geol. Society, Edinburgh, 1877.

one open to the air, and one with air sealed in—were heated together and successively to 100° C., 120°, 150°, 200°, 250°, 270°, and 300°, and the zeros observed. Even then, there still would remain to be explained the strange depression which I noticed in several sealed thermometers of lead-glass in the neighbourhood of 270°. At present, I regard the suggestion as neither proved nor disproved.

We are, in fact, only beginning to learn what silica and silicates are. I have quite lately, for example, found a critical point in the action of heat upon fire-clays, similar to the 270° point in the zeros (before referred to) of my lead-glass thermometers; and a similar point is known to exist in the relation of the refractive index of quartz to temperature. Results of this kind show clearly that thermometry is by no means an easy subject. Indeed, I might define it as a mixture of very complicated chemistry with very complicated physics.

Glasgow, March 28. EDMUND J. MILLS.

The Shuckburgh Scale and Kater Pendulum.

BY permission of Prof. T. C. Mendenhall, Superintendent of the United States Coast and Geodetic Survey, and of Weights and Measures, I enclose to you for publication, if deemed suitable, a note relating to an abstract of a paper by General J. T. Walker, R.E., F.R.S., published in NATURE of February 20 (p. 381).

As the subject-matter refers to U.S.C. and G.S. Bulletin No. 9, I take the liberty of enclosing it also.

O. H. TITTMANN.

United States Coast and Geodetic Survey, Office of Weights and Measures, Washington, D.C., March 13.

Last summer the United States Coast and Geodetic Survey published an investigation, Bulletin No. 9, on the relation of the yard to the metre.

As the result of this investigation, values were deduced for the length of certain historic standards in England which differed very materially from the values previously assigned to them in metric measures.

Thus the length of the Royal Society's platinum metre, certified by Arago to be $17{\cdot}59\,\mu$ too short, was found to be only $7\,\mu$ too short.

This metre was compared by Captain Kater with a certain space (0–39·4 inches) on the Shuckburgh scale, and this space was in turn compared with his pendulum. It is therefore of interest to know whether the value deduced in the investigation referred to is accurate. It is the object of this note to call attention to a surprising verification of the deductions contained in Bulletin No. 9. Using the equation for the platinum metre found in that paper, namely—

$$\text{Platinum Metre} = 1\text{ m.} - 7\,\mu + 9{\cdot}126\,\mu,\ t\,\text{C}^\circ,$$

we find

$$\text{at } 15^\circ{\cdot}98\text{ C., P.M.} = 1 + 138{\cdot}8\,\mu;$$

but at this temperature Captain Kater found the space on the Shuckburgh scale

$$(0\text{–}39{\cdot}4\text{ inches}) = \text{P.M.} + 0{\cdot}02400\text{ inch, or } 0{\cdot}6096\text{ mm.},$$

whence the space in question of the Shuckburgh scale $= 1{\cdot}007484$ m., and using for the coefficient expansion $18{\cdot}85 \times 10^{-6}$ for 1° C., we have at 16°·67

$$\text{the space} = 1{\cdot}0007614\text{ m.}$$

NATURE of February 20 (p. 381) publishes an abstract of a paper by General J. T. Walker, R.E., F.R.S., "On the Unit of Length of a Standard Scale by Sir George Shuckburgh, appertaining to the Royal Society," in which he states that the Shuckburgh scale was taken to Paris and compared with one of the standard bars of the International Bureau of Weights and Measures, by Commandant Defforges. The result of this comparison reduced to 16°·67 C., and as given by General Walker is

$$\text{the space} = 1{\cdot}0007619\text{ m.}$$

This agreement is perfect, more so, in fact, than the circumstances allow one to expect.

The agreement implies the correctness of the new values deduced in Bulletin No. 9 for the Ordnance metre and the platinum metre of the Royal Society, and gives the value of the metre as equal to 39·3699 inches as therein computed from Baily's and Sheepshank's comparisons, which established the relation between the Imperial yard and the space on the Shuckburgh scale.

It is to be noted that General Walker, ignoring Baily's and Sheepshank's comparisons, and adhering to the Clarke value 39·3704 + inches, deduces the (the writer of this thinks) erroneous conclusion, that the space on the Shuckburgh scale equals 39·400428 inches, the value according to their comparisons being 39·399896 inches. If to this value be added 0·04090 inch, the amount by which the distance between the knife-edges of the Kater pendulum exceeds the space 0–39·4 inches, the resulting length of the Kater pendulum at 16°·67 C. is 39·44080 inches, a value practically identical with that published by Kater, which is 39·44085 inches.

The Green Flash at Sunset.

THE explanation of the bluish (?) green flash of light sometimes seen at sunset given in your note last week (p. 495) does not seem to me to be a sufficient explanation of all the observations. If the phenomenon were due simply to refraction it would last for only a fraction of a second, and the colour would be much more blue than green. But, so far as my own observations go, the colour may last for several seconds, and is a bright pea-green, exactly similar to that shown by the sun many degrees above the horizon in South India in September 1883. To produce that green, as I have shown elsewhere, all that is required is the absorption due to a great thickness of vapour, combined with a certain amount of dust—water dust or other.

I saw a very pretty example of this last July when off the coast of Vancouver, B.C. The air was very moist and the rain-band correspondingly strong, while fine dust was supplied by the land breeze carrying with it particles from the burning forests inland. The sky was cloudless, but the haze was thick enough to allow one to look at the sun while it was still some degrees above the horizon, and the disk appeared of a brilliant golden-red, gradually changing to yellow, and, finally, while part was still above the horizon, it became a bright pea-green. The spectrum was similar to that figured in my paper on the green sun (R.S.E. Trans., xxxii. 389).

A few days later I had a view of the sunset from the Selkirks, where the air was very dry, the rain-band slight, but the haze considerable. The colours of the sun's disk were much less brilliant, and never passed beyond the stage of a reddish-copper tint. C. MICHIE SMITH.

73 George Street, Edinburgh, March 31.

Foreign Substances attached to Crabs.

I MUST of course accept Prof. McIntosh's interpretation of his own statement, and admit that he has found *Molgula arenosa* frequently in the stomachs of Cod and Haddock. This Ascidian differs from the majority of its class in having allocryptic habits, but I have not yet made a sufficient number of experiments to be satisfied as to its edibility. It has also been a considerable difficulty to me that the extensive investigations of Brook and Ramsay Smith lend no support at all to the opinion that this Ascidian forms an article of food for ground-feeding fish. In any case the matter, though of much interest, is not one for discussion here, since *Molgula arenosa* is never one of the "foreign substances attached to crabs."

The statement made by Mr. Holt that "*Actinia mesembryanthemum* is a favourite food of the Cod," was so inconsistent with our knowledge of the habits and distribution of the two species that, as I expected, the grounds for his assertion prove to be entirely fallacious. My statement with regard to the offensiveness of Actinians to fishes was made after prolonged observation of the habits of the living animals and after experiment, while Mr. Holt bases his objection on the ground that the St. Andrews fishermen find *A. mesembryanthemum* to be a successful bait for Cod. One might as well argue that because bits of red flannel or of tobacco-pipe are highly successful baits in whiffing for Mackerel, therefore these substances form a "favourite food" of this fish. A moment's reflection also would have shown Mr. Holt that an Anemone impaled upon a fish-hook is a much less dangerous creature than one under natural conditions and with tentacles expanded.

During the past week an interesting observation of Eisig's has come under my notice which corroborates the view that the association between Crabs and Anemones is of primary importance for the protection of the Crabs. Eisig observed (see Journ. R.M.S., iii., 1883, p. 493) that an *Octopus* in its attacks upon a Hermit Crab would instantly retreat upon being touched by the stinging organs of the Actinian associated with it.

Plymouth, April 5. WALTER GARSTANG.

THE THAMES ESTUARY.

ALTHOUGH it is not practicable to say precisely where the river ends and the estuary commences, it will be sufficient for general purposes if the westward, or inner, boundary of the Thames estuary is assumed to be a line from Southend to Sheerness, the northern boundary as the coast of Essex, and the southern the coast of Kent; and it may be said to extend eastward to the meridian of the Kentish Knock light-vessel. The area inclosed between these lines is upwards of 800 square nautical miles, and the whole of the space is encumbered with banks, between which are the several channels leading to the river.

As the shores of Essex and Kent are low, and have no natural features by which they may be distinguished at a distance, and as a great part of the estuary is out of sight of land, even in the clear weather so rare in this country, it is evident that artificial marks in considerable number are required to make navigation at all practicable between the banks. In early times, when vessels were small and of light draught, few marks were necessary, but with increasing trade, necessitating vessels of heavy draught, new channels have to be marked farther from shore, and the demand for additional security to navigation has especially increased of late years, so that now there are no less than 3 lighthouses, 11 light-vessels, 8 gas buoys, 10 beacons, and 117 ordinary buoys marking the channels at present in use; and the demand for additional marks is likely to increase rather than diminish, for the deepest channels through the estuary have not yet been buoyed, and the changes in progress seem to favour the opinion that before many years some of them will have to be opened up to facilitate traffic.

In endeavouring to give an account of the changes in the channels of the estuary, it is difficult to obtain any authentic records earlier than the commencement of the present century. If such records exist, they are not at the Admiralty or Trinity House, the earliest surveys worthy of notice being those of Mackenzie, Graeme Spence, and Thomas, between 1790 and 1810; but no thorough investigation appears to have been taken up until Sir Francis Beaufort was Hydrographer, when, under his instructions, Captain Bullock surveyed the whole estuary between 1835 and 1845. Since then, Calver re-surveyed the whole of the southern part in 1862–63, and examined the northern banks in 1864, and lately the *Triton* has re-surveyed all the important channels and delineated the banks, and from these several surveys some idea can be obtained of the condition of the estuary at different epochs, and of the changes that are taking place.

These changes seem to be of two kinds; viz. permanent changes and periodic changes.

Before, however, describing the changes in progress, it will be well to give a general description of the estuary; and, to render the description more intelligible, three plans have been constructed, the first showing the whole estuary on a small scale with the tracks followed by vessels; the second being a diagram showing the state of an obstruction in a channel at different epochs, a characteristic permanent change; whilst the third plan shows the state of the Duke of Edinburgh Channel from the time of its first opening out to the present date, to illustrate what seems to be a channel opening and closing periodically.

It is worthy of notice that all the banks of the estuary are of sand intermixed with shells: even the foreshore consists mostly of sand, between high and low water marks; in two places only is it of shingle (viz. off Whitstable and at Garrison Point, Sheerness); and in a few places, near the entrance of the rivers discharging into the estuary, there is a little mud, whilst in the vicinity of Margate there are some ledges of chalk. The sand is very fine, and although, when dry, it possesses a tolerably hard surface, directly it begins to be covered it is all alive. When beacons are erected on any of the banks, or a ship gets on shore, the tidal streams scour out the sand in the immediate neighbourhood, and cause the wrecks to sink and finally disappear. Although without actual boring it is not possible to give the exact depth of these sands, it is probable that they are upwards of 60 feet thick, for channels of that depth have opened out across the sands and again closed up, so that the bank has been dry at low water where 60 feet formerly existed; and the Goodwin Sands, in the Downs, which have been bored, proved to be 80 feet in thickness. All the banks, and the channels between them, trend in a north-east and south-west direction: this is doubtless due to the fact that the stream outside the estuary is running to the northward whilst the tide is ebbing from the river, and, consequently, the ebb stream in the estuary is deflected to the north-eastward.

The channels into the estuary, therefore, must be classed under two headings: (*a*) those which follow the main line of the flood and ebb streams, and (*b*) those which do not follow the general stream of the tide.

In the former category are the Warp, West Swin, Middle Deep, East Swin, Barrow Deep, Oaze Deep, and Black Deep; in the latter are the Middle Swin, Queen's Channel, Prince's Channel, Alexandra Channel, Duke of Edinburgh Channel, Gore Channel, &c., which are all more or less of the nature of swatchways across the main line of the sand-banks of the estuary. In the Black and Barrow Deeps, which are the deepest and straightest channels through the estuary, the ebb stream runs 7 hours and the flood 5 hours, and the ebb is much stronger than the flood, the stream setting fairly through. In the Duke of Edinburgh Channel, the deepest swatchway of the estuary, the streams at the north and south ends are of a rotatory character, revolving with the hands of the clock.

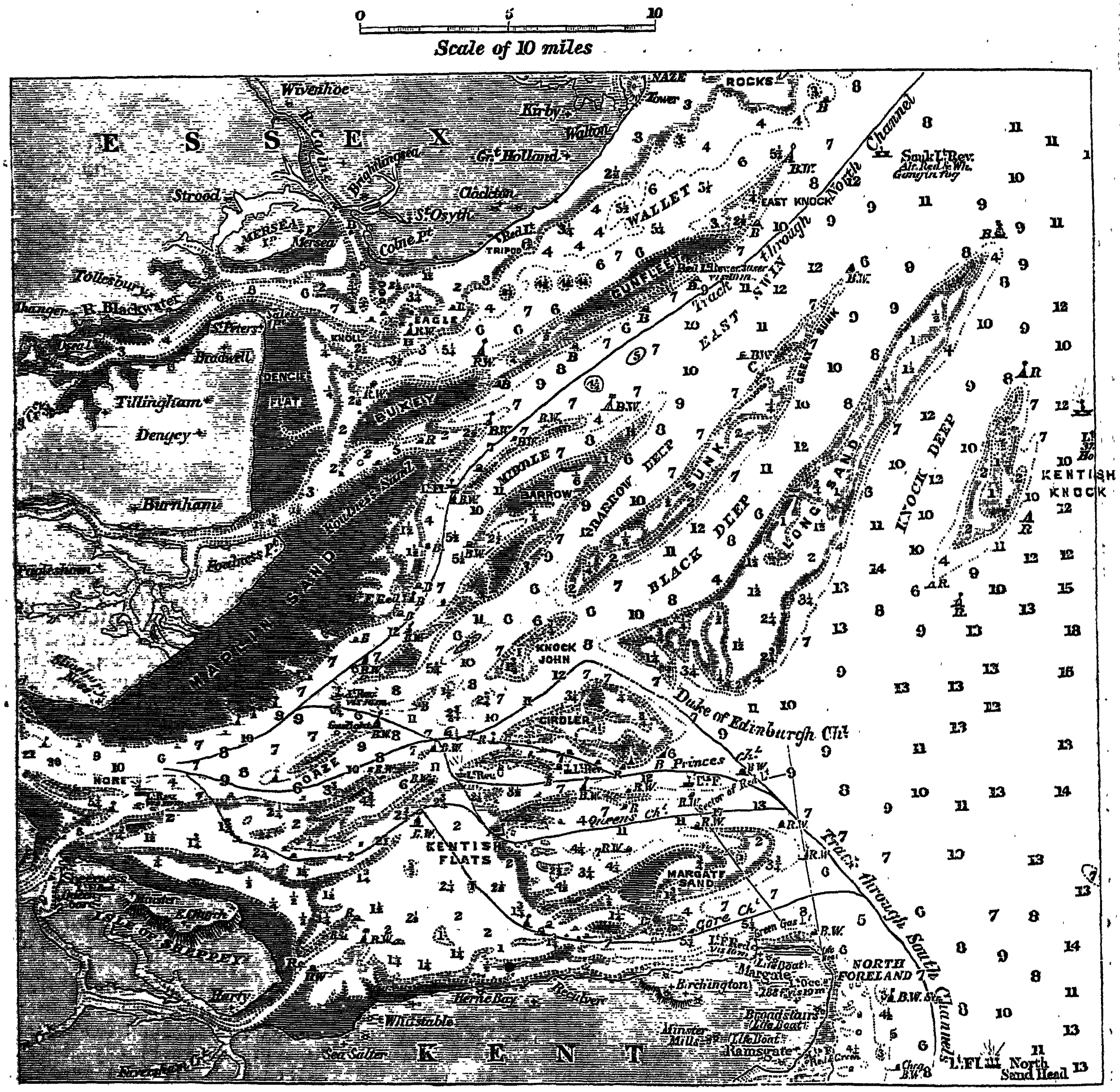
I would here explain that in a large space like the Thames estuary the difficulty of buoying the various channels increases very considerably with their distance from the shore. With permanent marks erected on the shore, it is easy to place buoys in selected positions, not far from land, in fairly clear weather. But when the distance from the shore has increased so that the marks erected on the land cannot be seen, we have either to erect other marks on the sand-banks and carry out a triangulation, or we are dependent on floating bodies (fixed by land objects) to fix other floating bodies farther off. That this is an eminently unsatisfactory method will be evident when it is stated that each time the Kentish Knock light-vessel has been satisfactorily fixed, the position has been very different from that supposed. When fixed by Calver in 1864, she was found to be one mile N.E. ½ N. of her charted position; and when fixed by the *Triton* last year, she was found to be one mile and a half S.E. by E. of her supposed position.

The errors probably creep in somewhat in the following way. Something goes wrong with the light-vessel after she has been satisfactorily fixed: a collision takes place, the fog-siren gets out of order, or one of the many things happens which necessitates the vessel being taken into port. A temporary light-vessel is substituted, and she is anchored in almost precisely the same position as the other, but probably before her mushroom bites the ground it has dragged somewhat. By the time the other vessel is repaired and brought out, the temporary one may be a cable or so away from the original position. As the weather is usually thick, the permanent vessel has to be anchored as nearly as practicable in the position of the temporary craft, and her mushroom may drag somewhat before biting the ground, &c. Thus a series of errors creep in without there being adequate means of checking the position of the light-vessel, and within the last few years the *Triton* has found the Leman and Ower light-vessel one mile away from her charted position, the

Dudgeon light-vessel about one mile from her supposed position, and the Outer Dowsing light-vessel nearly two miles from the charted position.

All these light-vessels are either out of sight of land, or can only be seen from an elevated position on the shore on rare occasions.

It is therefore naturally the object of the Elder Brethren of the Trinity House to utilize the channels closest to the shore, and, as these channels are also the most direct into the Thames, the northern channel following the general trend of the Essex coast, and the southern that of the Kentish coast, no other channels would require marking if the depth in these was sufficient for the traffic. Hitherto the one northern channel has been enough, but this is steadily shoaling, as will be described further on; but the southern channels are mostly shoal, and one after another has had to be opened up as the size of the vessels and their draught of water increased, until there are now five buoyed channels off the Kentish coast, two of which are lit; but only one can be termed a deep-water channel,



PLAN I.—THAMES ESTUARY. (Depths in Fathoms.)

and this would seem to be the very channel which opens and closes periodically, as will be shown subsequently. Should this prove to be the case, there will be intervals during which there will be no deep-water channel into the river on the south side of the estuary.

By a reference to Plan I., showing, on a small scale, the whole estuary, it will be seen that the northernmost channel, viz. that close to the coast of Essex, is named the Wallet, and that this is separated by a series of banks, termed Buxey and Gunfleet, from the channel next it. These banks, which are collectively 18 miles long, are dry for the most part at low water; there are, however, two narrow passages across them, one separating the Buxey from the Gunfleet, called the Spitway, and the other separating the Buxey from the Dengie flat (extending from the Essex coast). The Spitway, which, when sounded in 1800, had a depth of nine feet, has remained at that depth until recently, but now has only a depth of 5 feet at low water; the channel between the Buxey sand and Dengie flat has about 12 feet, and is merely an

outlet for the River Crouch. It will therefore be seen that the Wallet is really only a channel to the Rivers Colne, Blackwater, and Crouch, and is of no importance as a channel towards the Thames. It was last surveyed by Staff-Captain Parsons in 1877, and as its features have not materially changed since 1800, it will probably not be surveyed again for many years, unless the swatchways across the Gunfleet should deepen or others open up of sufficient importance to render the Wallet useful as a traffic channel. There were formerly other swatchways across the Gunfleet, but these are now closed.

The channel next the Wallet is named the King's Channel, or Swin; the eastern part is named East Swin; the central part Middle Swin, and the inner part West Swin. This is the channel through which all the traffic between London and the northern ports of the Kingdom passes, and it is almost always crowded with shipping. The East Swin is bounded at first by the Gunfleet sand to the north-westward and the Sunk sand to the south-eastward, and is 3 miles wide; but 8 miles within its entrance two other banks commence—one, the Barrow, being very extensive, upwards of 13 miles in length and 2 in breadth; and the other, the Middle or Hook sand, a narrow ridge about 6 miles long, extending along the north-west face of the Barrow sand, and leaving a channel nowhere less than $\frac{5}{8}$ of a mile wide between them. It will thus be seen that 8 miles within the entrance of the East Swin it is split up into 3 channels; the northernmost retaining the same name, the channel between the Middle, or Hook sand, and the Barrow being known as the Middle Deep, whilst the channel between the Barrow and Sunk sands is known as the Barrow Deep. The Middle Deep rejoins the Middle Swin, but the Barrow Deep and West Swin both run into what is known as the Warp. The Swin is well buoyed and lighted throughout, but the Middle and Barrow Deeps have not yet been buoyed. In fact, it has hitherto not been necessary to do so, as the least water in the main channel of the Swin has, up to recently, been ample for all that has been required; but a steady shoaling has been taking place in a critical part of this channel since 1800, and it now seems to be only a question of time before the Middle Deep will have to be marked.

To illustrate the changes in progress here, Plan II. has been constructed, showing the condition of the critical part of the navigation in the Swin each time it has been thoroughly surveyed. By this diagram it will be seen that in 1800 the ruling depth in the channel between Foulness sand and the Middle or Hook sand was 35 feet at low water. Forty-three years later, a bar, on which the depth at low water was 28 feet, had formed between the Foulness sand and the Middle. In 1864 the depth had decreased to 24 feet, and, in 1889, to 21 feet, showing a steady decrease since 1800 of about one foot in every six years. The deposit is of sand, shells, and mud. This is the only shallow part of the Swin; and as it is evident that, so far as our knowledge extends, we may expect it to continue to decrease in depth, and as even now, with strong south-west winds prevailing in the North Sea, it is by no means rare for the tide to fall 3 feet below the level of low water ordinary springs, so that the depth would be reduced to 18 feet, it is clear that vessels of heavy draught will either have to wait for tide or use another channel. Already our small armoured vessels of war have to time themselves to reach this obstruction by half-tide. Fortunately, the Middle Deep is an alternative channel with ample depth in it, which only requires to be buoyed, and this can readily be done. This Deep seems to be in a better condition now than it has been for 50 years, for, when surveyed by Bullock, in 1843, there was a bar of 25 feet at its east end. This had disappeared when it was surveyed by Calver in 1864, and there was then a channel of two cables in width between the edges of the 30 feet contour lines of soundings surrounding the Middle sand and Barrow. There is now a channel four cables in width between those contour lines in the narrowest part of the Deep.

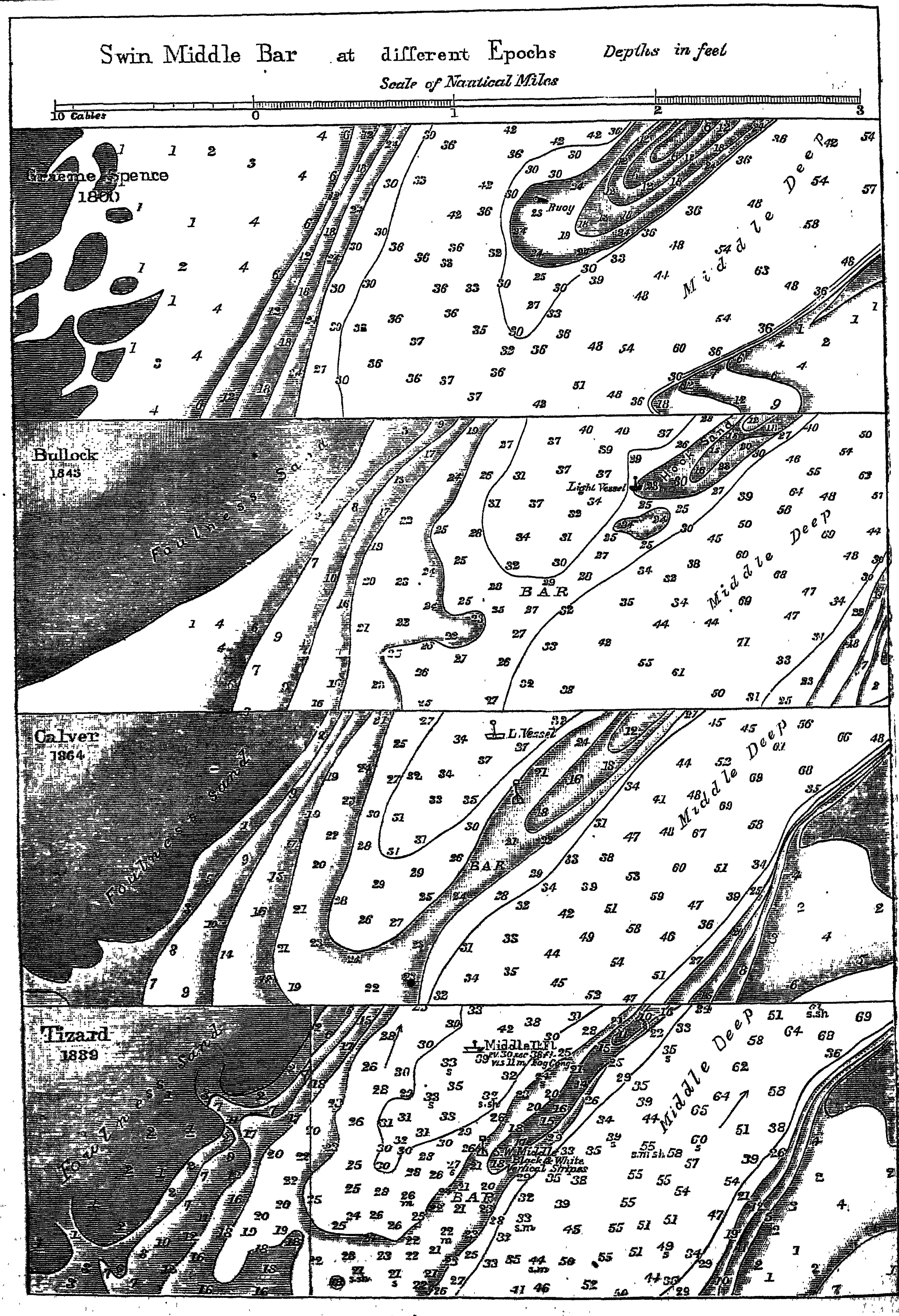
The Barrow Deep, referred to as the third channel branching away from the East Swin, is deep throughout, and without obstruction. It varies somewhat, as shown by the different surveys, but is an excellent highway, which only requires buoying to be available for traffic. At present the London County Council are allowed to empty rubbish in this Deep, which seems rather a pity, as there is no knowing what may be the result eventually, more especially as we have at present no observations to show to what depth the tidal scour is of service. Any interference with the channels, likely to cause an obstruction, should be avoided.

The Sunk sand, which is the south-eastern boundary of the Barrow Deep and the north-western boundary of the Black Deep, has undergone great alterations since originally surveyed in 1800. In that year it is shown as a long sand which really extended from the present north-east end in one continuous line of shallow water to the inner end of the Oaze sand, a distance of 26 miles. On it were many dry patches, named Great Sunk, Little Sunk, Middle Sunk, Knock John, &c., and the only passage across was a three-fathoms channel at low water at the eastern end of the Oaze. When surveyed by Bullock, 1835-45, this chain of sands had altered very considerably, and had several channels or swatchways across it —a swatchway of 22 feet at low water between the Great and Little Sunk sands: a swatchway of 60 feet at low water between the South-West Sunk and the Knock John sands; a 35-feet channel $1\frac{1}{2}$ mile wide between the Knock John and North Knob sands; and a swatchway of 26 feet between the North Knob and the Oaze. When surveyed by Calver, 1862-64, this series of banks had again altered: the swatchway between the Great and Little Sunk sands had only 12 feet in it at low water; the swatchway between the South-West Sunk and the Knock John had shoaled to 40 feet; but the channel between the Knock John and North Knob had deepened to 45 feet, and a narrow channel of 40 feet at low water had opened out between the Oaze and North Knob.

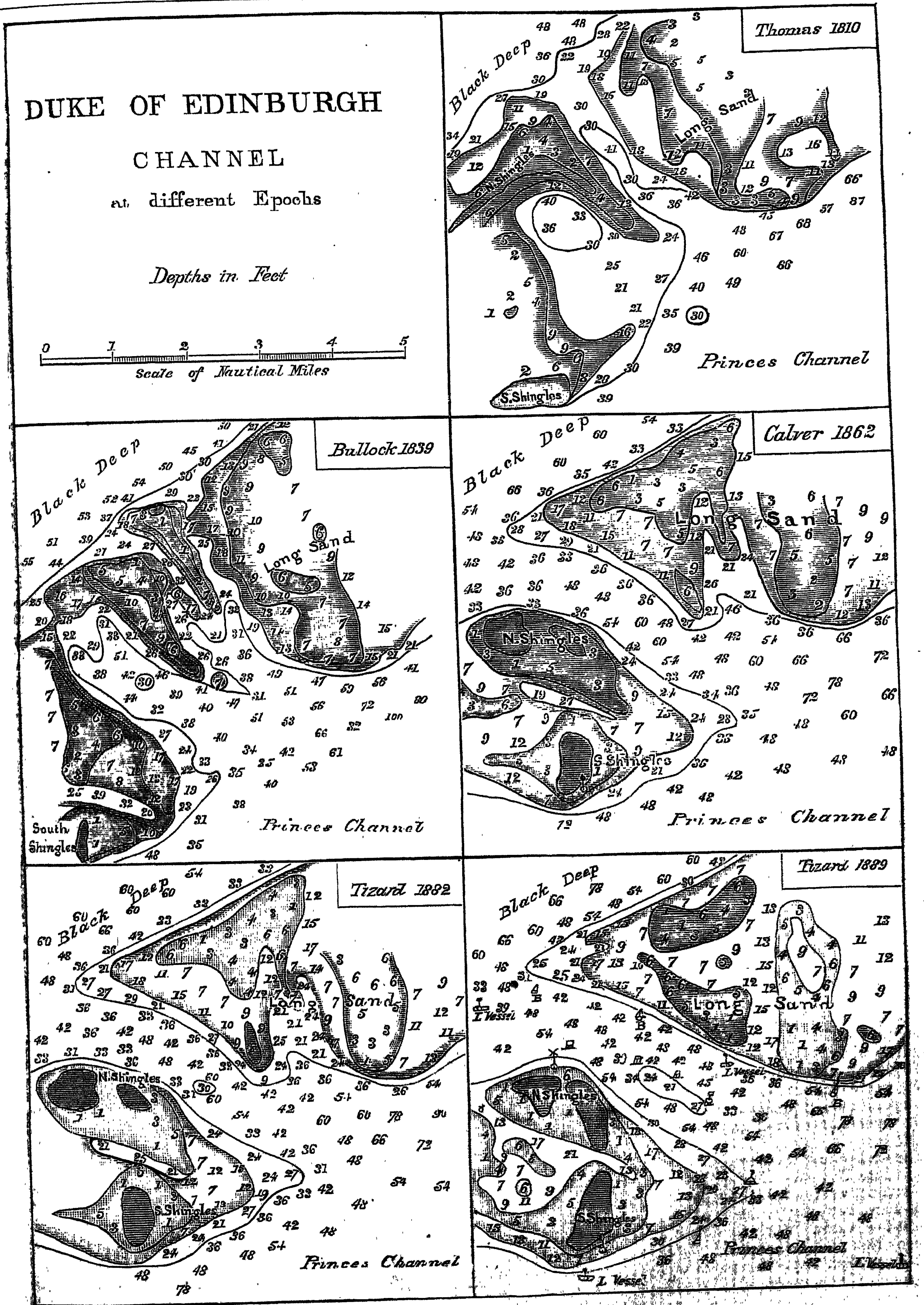
In 1888-89, when surveyed by the *Triton*, the swatchway between the Great and Little Sunk sands had entirely disappeared; the swatchway between the South-West Sunk and the Knock John sands had narrowed and shoaled to 29 feet; the channel between the Knock John and North Knob shoals had decreased to 24 feet, whilst the channel between the North Knob and the Oaze had increased its width to one mile, with about the same depth (viz. 40 feet) at low water. In fact, the chain of sands known as the Sunk, Knock John, Knob, and Oaze, which were, in 1800, one continuous bank, after breaking up into separate patches, again show signs of resuming the form they possessed when originally surveyed, the only deep channel across them now being between the Oaze and North Knob.

The Black Deep is the channel bounded to the north-westward by the chain of sands just described, and to the south-eastward by another chain of sands named Long Sand: Shingles, Girdler, and the flats extending from the Kentish shore. It is a deep-water channel, the inner part of which has been buoyed since 1882, and lighted since December last, as it communicates by a deep-water swatchway, named the Duke of Edinburgh Channel, with the deep water off the North Foreland, and so forms a convenient outlet for the heavy-draught vessels bound southward from the Thames. There seems to be some tendency to shoal in the north-east end of the Black Deep, but it has only once been sounded—viz. by Bullock, in 1843; and we have not yet quite completed our examination of it throughout, so that no thorough comparison is yet practicable.

The chain of sands which bound the south-east side of


Swin Middle Bar at different Epochs
Depths in feet
Scale of Nautical Miles
10 Cables
0
1
2
3
Graeme Spence
1800
Buoy
Middle Deep
Bullock
1843
Foulness Sand
Light Vessel
Knock Sand
BAR
Middle Deep
Calver
1864
L. Vessel
Foulness Sand
BAR
Middle Deep
Tizard
1889
Foulness Sand
Middle L.Fl.
S.W. Middle
Black & White
Vertical Stripes
BAR
Middle Deep

PLAN II.


DUKE OF EDINBURGH
CHANNEL
at different Epochs
Depths in Feet
0 1 2 3 4 5
Scale of Nautical Miles
Thomas 1810
Black Deep
Long Sand
N. Shingles
S. Shingles
Princes Channel
Bullock 1839
Black Deep
Long Sand
South Shingles
Princes Channel
Calver 1862
Black Deep
Long Sand
N. Shingles
S. Shingles
Princes Channel
Tizard 1882
Black Deep
Long Sand
N. Shingles
S. Shingles
Princes Channel
Tizard 1889
Black Deep
Long Sand
N. Shingles
S. Shingles
Princes Channel

PLAN III.

the Black Deep formerly extended in one continuous line from the Kentish coast to the Long Sand Head, a distance of over 30 miles. Across this chain of sands there have always been shallow swatchways which communicated by somewhat circuitous channels with the deep water of the estuary. These are now 5 in number: (1) the Gore Channel, which passes close to Margate and then across the Kentish flats; (2) the Queen's Channel, which, passing between the Margate sand and Tongue sand, also leads across the Kentish flats; (3) the Prince's Channel, which leads between the Tongue sand on the south side, and the Shingles and Girdler sands on the north side, into the Black Deep; (4) the Alexandra Channel, which leads from the Prince's Channel to the Black Deep; and (5) the Duke of Edinburgh Channel, which leads from the deep water of the North Sea into the Black Deep. All these channels are buoyed. In the Gore Channel (sometimes called the South Channel), which has been in use from early times, the depth at low water is 10 feet. The shallow grounds shift backwards and forwards, but there seems to have been always as little as 10 feet at low water in some parts of this channel. In the Queen's Channel, which was buoyed in the last century, the least depth in passing over the Kentish flats is 13 to 14 feet at low water. In Prince's Channel, which was buoyed in 1846, and lighted in 1848, the least depth is 20 feet at low water, but there is a patch of 17 feet at its western end in the centre of the channel which seems to be always in this channel though not always in the same position. It is shown by Bullock in 1839, by Calver in 1862, and by the *Triton* in 1880. The Alexandra Channel, which is a swatchway between the Shingles and Girdler sands, had no existence in 1800, the Girdler and Shingles forming with the Long Sand a continuous chain at that date. In Bullock's survey of 1839, the Alexandra is shown as a blind inlet on the north side of the Prince's Channel, which was cut off from the Black Deep by a ridge over which the depth was 7 feet at low water. When surveyed by Calver in 1862, the least depth in the channel was 20 feet; and when surveyed by the *Triton* in 1888, the least depth was 23 feet. It is, however, much narrower now than in 1862, and if it continues to decrease in width will not be available for traffic, as there is not now much more than room for two large vessels to pass each other, and bad steerage might cause an accident.

Of the Duke of Edinburgh Channel, which is a broad swatchway at present dividing the Long Sand from the Shingles Sand, we have a tolerably complete history; and as this would seem to be a channel which opens and closes periodically, Plan III. has been constructed to show its condition each time it has been surveyed. The first record we have of it is on an old chart of 1794, when it is shown as a 9-feet swatchway, and is named "Smugglers' swatch." When surveyed by Thomas, in 1810, it was named "Thomas's New Channel," and there was then a narrow passage carrying 30 feet at low water between the Long Sand and Shingles. In 1839, when surveyed by Bullock, and named "Bullock Channel," this 30-feet swatchway of Thomas's was obstructed by a bank in the middle, which dried at its north end, leaving a passage of 15 feet on its east side, and a very narrow gat of 25 feet on its west side, but one mile farther west a new channel was opening out, the shoalest water in which was 16 feet. This appears as an inlet into the sand-bank on Thomas's chart.

The next time it was surveyed was by Calver, in 1862, at which date Thomas's Channel had closed completely, but the channel west of it had opened out and become a wide deep-water swatchway, the least depth in which was 42 feet at low water. Early in 1882 it was thought advisable to buoy this channel, and the *Triton* was ordered to examine it, when a 30-feet patch was discovered near its centre. In the autumn of 1887, this patch was reported to have shoaled; and in 1888, when examined again by the *Triton*, it was found to be upwards of a mile in length with 22 feet on it. In October 1889, the channel was again examined, when the least depth on the central patch was found to be 21 feet, and it had a tendency to shallow to the eastward. The channel was buoyed in the summer of 1882, and re-named by the Elder Brethren of the Trinity House "Duke of Edinburgh," after the Master of the Trinity House. It was lighted in December 1889.

The various surveys seem to show that the estuary has a tendency for the most part to return to the condition it was in about 1800. In that year there were no deep-water swatchways across the banks, and the channels that opened up subsequently seem now to be all closing again. At any rate, those in use as ship channels evidently will require constant watching.

Should the Duke of Edinburgh Channel close, and none other open out, it will materially interfere with the heavy traffic into the estuary from the southward, for it will necessitate either waiting for high water or passing round outside into the Black or Barrow Deeps, which will have to be buoyed and lighted to make them readily accessible.

There is one other shoal, the "Kentish Knock," which may be said to belong to the estuary. This is a sandbank about 6 miles in length and 2 in breadth, on the south-east side of the outer part of the Long Sand. Its shape and area, within the contour-line of five fathoms, would appear to be fairly constant; but it had a swatchway across the north end, when surveyed by Calver in 1864, which has now entirely disappeared. Between the Kentish Knock and Long Sands is a channel, two miles wide, named the Knock Deep. At the north end of this channel the soundings are much shoaler than when surveyed by Bullock. In some cases the difference is as much as 12 feet.

Although the general tendency of the banks in the estuary seems to be to revert to the condition they were in about the year 1800, it is not possible to predict that this will certainly be the case. If, as seems probable, the condition of the estuary is due to the action of the sea in casting up banks, and of the tidal flow in cutting channels through the banks thus formed, it is evident that much will depend on prevailing types of gales. There can, however, hardly be a doubt that any diminution of the volume of the water running into and out of the estuary would diminish its power of making deep-water channels, so that any action tending to decrease the flow into and out of the various rivers should be avoided if possible; as although it is conceivable that a given type of strong winds, extending over a lengthened period, might have the effect of closing the various swatchways across the banks, it does not follow that a cessation of these winds would cause the channels to be again opened out if the volume of the tidal flow was seriously diminished.

T. H. TIZARD.

NOTES.

THE respect in which science is held in France was once more exhibited in a very striking way at Saint Sulpice, Paris, on Tuesday, in connection with the funeral service of M. Hébert, Professor of Geology, member of the Institute, and honorary *doyen* of the Faculty of Sciences. Deputations from the Institute and Faculty of Sciences were present, and the Paris correspondent of the *Times* says that all the great scientific and literary institutions of Paris were represented. At the cemetery of Montparnasse, where the interment took place, speeches were delivered by M. Gaudry, in the name of the Institute; M. Darboux, in the name of the Faculty of Sciences; M. Marcel Bertrand, in the name of the Geological Society; M. Jannery, in the name of the Normal School; and M. Bergeron, in the name of the old pupils of M. Hébert.

GERMAN papers announce the death of Dr. Karl Jacob Loewig, Professor of Chemistry at the University of Breslau, Director of the Chemical Laboratory, and author of many eminent works on chemistry. He was born at Kreuznach on March 17, 1803, and died at Breslau on March 27.

THE "Inspectors' Instructions" relating to the Code of 1890 have been issued this year with remarkable promptitude. The document is one of great importance, and it is satisfactory that all who are interested in popular education will have ample time to study it before the various questions connected with the new Code are discussed in Parliament.

THIS week the National Union of Teachers has been holding its 21st Annual Conference at the Merchant Taylors' School, London. The meetings began on Monday, when the President, Mr. H. J. Walter, delivered his inaugural address. Speaking of the new Code, Mr. Walter said the teachers of the country would accept and welcome it; and although they reserved their right to criticize the details freely, and unhesitatingly to state that in many points the Code was capable of improvement, "they would work loyally with the Education Department in the endeavour to show such an improvement in the education of the country that the public would be ready to listen with attention and respect when teachers made suggestions for further changes and advance in the same direction."

M. GASTON BONNIER has been elected President of the Botanical Society of France for the year 1890, and MM. E. Roze, A. Michel, J. Poisson, and J. Vallot, Vice-Presidents.

THE International Exhibition of Geographical, Commercial, and Industrial Botany, proposed to be held at Antwerp, has been postponed till next year.

AN International Exhibition of Horticulture, which will be largely of a scientific character, will be held in Berlin from April 25 to May 5.

AN Electro-technical Exhibition is to be held at Frankfort-on-the-Main next year. It will be divided into twelve sections.

SOME exhibits in the Science Department (under the direction of the Rev. Dr. West and Mr. C. Carus-Wilson) of the Bournemouth Industrial and Loan Exhibition, opened on the 7th inst., are worthy of special notice. Among these are a collection of British and foreign oysters lent by the Poole Oyster-fishing Company, and a collection of birds' eggs, for which Mr. R. G. H. Gray has received a special prize. The first prize has been awarded to Mr. E. H. V. Davies, who exhibits an interesting collection of recent and fossil local shells. The various stages in the process of developing photographs are illustrated in a series exhibited by Mr. Jones. In the Geological Section, large specimens of fluor-spar have been lent by Dr. West, who also contributes a collection of Eocene fossils from the London, Hampshire, and Paris basins. Mr. C. Carus-Wilson shows a case of remarkably well-preserved fossils of various geological ages, including a gigantic shark's tooth (*Carcharodon*) from Rio; also, garnets in quartz, and samples of musical sands. Leaves from the Bournemouth Beds are well represented by Mr. Bennett's collection. In the Entomological Section, Mr. McRae's collection of British Lepidoptera attracts much attention; the Rhopalocera and Macro-Heterocera are nearly all represented, a large number having been bred by Mr. McRae from larvæ obtained in or near Bournemouth. A special prize has been awarded to Mr. Harding for a large astronomical telescope constructed entirely by himself. The Exhibition will close on the 21st inst., when the prizes will be distributed by the Duchess of Albany.

THE Royal Microscopical Society will hold its first evening *soirée* in its new rooms, 20 Hanover Square, on Wednesday, April 30, at 8 p.m.

M. LECLERC DU SABLON has been appointed to a Professorship of Botany at Toulouse, and is succeeded in his post of assistant naturalist to the chair of Organography and Vegetable Physiology at the Museum of Natural History at Paris, by M. Morot.

DR. LUDWIG KLEIN has been appointed Professor of Botany in the University of Freiburg-in-Breisgau.

M. PAUL MAURY has been attached to the Geographical Exploring Commission of the Mexican Republic in the capacity of botanist, and is about to depart for Mexico on a botanical expedition.

THE plans of the Danish expedition to the east coast of Greenland are now complete. Lieut. Ryder will command a party of nine, and during next summer, as soon as the ice permits, they will go by steamer to the east coast, and then devote two years to the investigation of the district between lat. N. 66° and 73°. At the end of that time they will be fetched by the steamer from Denmark.

THE French Society "Scientia" informs its members that its next dinner, on April 30, will be presided over by M. C. Richet and by M. de Lacaze-Duthiers, in whose honour the dinner is to be given. The last dinner was given in honour of Francis Darwin.

AT the general monthly meeting of the Royal Institution, on April 7, the special thanks of the members were returned for the following donations to the fund for the promotion of experimental research: Mr. Ludwig Mond, £100; Mr. Lachlan M. Rate, £50.

AT the Royal Institution the Hon. George C. Brodrick will begin a course of three lectures, on the place of Oxford University in English history, on Tuesday (April 15); Prof. C. V. Boys will begin a course of three lectures, on the heat of the moon and stars, on Thursday (April 17); and Captain Abney will begin a course of three lectures, on colour and its chemical action, on Saturday (April 19). The evening meetings will be resumed on Friday (April 18), when Sir Frederick Bramwell will give a discourse on welding by electricity.

THE Marlborough College Natural History Society, according to its latest Report, is in a most flourishing condition. The year 1889 was for the Society "one of continued prosperity and progress." On April 9, 1889, the Society completed its twenty-fifth year, and the members afterwards commemorated the occasion by an excursion to Stonehenge.

DR. VON DANCKELMAN has contributed to *Mittheilungen aus den deutschen Schutzgebieten*, vol. iii., an important paper on the climate of German Togoland, and of the neighbouring districts of the Gold and Slave Coasts. The observations are drawn from all available sources, from those first made by Dr. Isert at the then Danish settlements in 1783–85, down to the most recent observations by English, French, and German observers. A good deal of information exists, comparatively speaking, from this part of West Africa, and among the best of the observations are those made in 1888–89 by the German officials at Bismarckburg (lat. 8° 12′ N., long. 0° 34′ E.), at an altitude of about 2330 feet above the sea. A comparison of the tables given for the various colonies shows that the highest air pressure occurs in July and August, and the lowest in February and March. The monthly range is small, amounting to less than 0·2 inch. Temperature varies considerably with the position relatively to the coast. While at Akassa, on the coast, the mean daily range is only about 10°, at Bismarckburg it is double that amount. And during the hot season the range is double what it is in the cool season. Rainfall also varies with position relatively to the coast. The rainy seasons are March to June,

and September to November. Dr. von Danckelman gives valuable statistics about the harmattan, which is generally understood to be a cold wind. He shows, however, that during the periods of this wind the temperature both in the morning and evening is warmer than on other days, and that the mean daily temperature is nearly 2° warmer. The air on these occasions is so dry that the hygrometric tables are not low enough for the reduction of the observations. On one occasion the relative humidity was only 9 per cent., with a temperature of 94°.

WE have received from Mr. D. Dewar his "Weather and Tidal Forecasts for 1890." The author has previously published similar forecasts for past years, and they are said to be mainly based upon the simple idea that the prevailing westerly movement of the air in the two great belts in the north and south temperate zones is due to the continued westerly (west to east) movement of the sun and the moon, and it is claimed that the probable weather, while referring generally to the northern hemisphere, is chiefly applicable to the British Isles and neighbourhood. We have made a rough comparison of the forecasts with the actual weather experienced in the British Isles during the first three months of this year. The weather predicted by Mr. Dewar for January largely consists of cold and anticyclones, whilst the actual weather experienced was conspicuous for the absence of cold, with the exception of the first two or three days, and its mildness probably exceeded that of any January during the last half-century. At Greenwich the thermometer did not once fall below the freezing-point after the 3rd. Considering February as a whole, the forecasts were rather more successful. In March, the early part of the month was to have been mild, except in the north. The first few days were colder than in any March during the last half-century, except in the north, where milder weather was experienced. The weather predicted for the remainder of the month consists almost wholly of cold and snow, whereas the weather was exceptionally mild, and the Greenwich temperature on the 28th has only twice been exceeded in March during the last fifty years.

IN the current number of the *Zoologist* it is stated that a wealthy Berlin manufacturer has a shooting near Luckenwald, where the Wapiti, *Cervus canadensis*, has been acclimatised. Between January 20, 1889, and January 20, 1890, seven of these animals were shot there, one of them having a head of fourteen points.

DR. W. KING, Director of the Geological Survey of India, has commenced, in the current number of the Records of the Survey, the publication of the provincial index of the minerals of India, which is intended as a help towards the compilation of an annual statement showing the quantities and value of mineral products in British India, for the publication of the mining and mineral statistics of the Empire. Dr. King's classification is of a broad and popular nature. The provinces or Presidencies and Native States are taken in alphabetical order, and the mineral products of each are set down with notes as to the quantity, quality, and output. The mineral products themselves are divided into "Important Minerals," "Miscellaneous Minerals," "Gem Stones," and "Quarry Stones." Under the first head are included only coal, iron ores, gold, petroleum, and salt. Under the second head come metallic ores, borax, gypsum, asbestos, soapstone, sulphur, and the like. "Gems" include amber, beryl, diamond, garnet, jade and jadeite; while clays, limestones, marbles, kunkar, slate, &c., are grouped as quarry stones. The first instalment of the list ends with the Central Provinces. This index may help to dispel the common idea that India is rich in minerals. The greater part of the entries are mere indications of the reported existence of ores, while those which note a regular production of any commercial importance are few and far between.

IN one of the Bombay Natural History Society's papers, Mr. G. Carstensen, Superintendent of the Victoria Gardens, Bombay, makes a bold suggestion for facilitating the study of botany in India. His experience, he says, has taught him that the study of botany is far more popular in the northern countries of the European Continent than in British possessions, and he cannot help thinking that this fact may be clearly attributed to the difference in the botanical terminology. While the terms used in English works on botany are too frequently quite unintelligible for the layman, because they are in most cases Anglicized Latin words, the terms used by German and Danish authors are generally easily comprehended, because they are translated into the mother language, refer to objects of daily life, or are derived from the language itself. He therefore proposes that the Botanical Committee of the Bombay Society be requested to revise the existing terminology, and to substitute English and intelligible terms for the more unintelligible ones. He gives a few examples of the English substitutes he proposes. The natural arrangement of plants consists of two large divisions, Phanerogams, or "flower-plants," and Cryptogamous plants, or "spore-plants." "Flower-plants" are again divided into Dicotyledons, or "two-seed-leaved." The "two-seed-leaved" in the same way are divided into Angiosperms, or "seed-vessel-plants," and Gymnosperms, or "naked-seeded plants," and so on. For the "natural orders" he would substitute existing or new English names, and for "genera" he would substitute "forms." In a complete flower the calyx would become the "cup," the sepals "cup-leaves," the corolla the "crown," the petals "crown leaves;" the cup and crown together, now known as the perianth, would be the "floral cover," and so on through the andræcium and gynæcium, and the whole anatomy of the plant. The adoption of this method would, Mr. Carstensen thinks, "vastly increase the number of students of botany, and in the end would materially further the progress of this unfortunately neglected science."

THE subject of dreams seems to demand more thorough study than it has yet received from science. An American, Dr. Julius Nelson, of New York, has lately published the results of an examination he made of some 4000 of his dreams. He finds that the dreams of evening generally follow great physical or mental fatigue, and are associated with the events of the day. The same applies to night dreams, which, however, have more of a terrifying element in them. The most remarkable and pleasant are the morning dreams, occurring after complete rest of the brain. Fancy then appears to have its widest range and activity, working marvellous transformations, and giving clear vision of the past and the future. Dr. Nelson further finds that the vividness of his dreams is subject to regular fluctuations of 28 days, and that they also vary with the seasons, so that they are very vivid in December, and least vivid in March and April. An old popular superstition attaches special importance to dreams in the twelve nights from Christmas to January 6, and it is suggested that this is perhaps because dreams at that time have been found very vivid and distinct.

THE skin of Arctic voyagers, after the long night of winter, often appears pale, with a tinge of yellowish-green, on return of sunlight. The nature of this phenomenon, was, at the instance of Prof. Holmgren, studied by Dr. Gyllencreutz, in the expedition of 1882–83, and the results are given in a German physiological journal. Holmgren pointed out that the phenomenon might be subjective, due to a change in colour-sense through the long darkness; or objective, due to changes in pigment of the blood; or both. An examination of the colour-sense of the men before and after the polar night revealed no

change in this. The blood was examined by measuring the position of absorption bands of hæmoglobin with a given thickness of layer, and estimating their darkness. No change in the quality of hæmoglobin was detected, but the quantity, in some individuals, judging by changes in the width and darkness of the bands, was lessened towards the end of winter. Holmgren suggested, as an *experimentum crucis* with regard to the question of a subjective or objective cause, that someone should exclude himself from sunlight a month longer than the others: and to this infliction the engineer Andrée submitted. When he left his prison, his skin had a greyish-yellow tint. The conclusion arrived at is that the change of skin is due to an anæmic-chlorotic condition, possibly that of incipient scurvy.

WE have received Tylar's "Photographic Calendar" for the year 1890. It comprises, among other advantages, practical hints selected from the best contributors, and various reproductions of several of the pictures that gained prizes in the competition held last year. There is also an extended list of the author's specialities, as well as those of other dealers; and throughout there is a variety of useful information handy for reference. The prize list is more varied and comprehensive than that given last year.

THE "Photographers' Diary and Desk-book" for the year 1890, which is issued by the proprietors of the *Camera*, is a very handy and useful volume. Developing and other formulæ are printed in large type, capable of being read in the dim light of the dark room. A series of dark-room procedures has been added, including the work of developing the negative, silver printing and toning, platinotype printing (cold, hot, and sepia processes), Blanchard's platinum black process, and bromide printing. A selection of the most important and useful of the recent improvements in photographic apparatus is given, with several illustrations, preceded by some particulars of the objects of the Photographic Convention of Great Britain, with a list of its officers. The diary portion, interleaved throughout with blotting-paper, gives ample space for the daily record of photographic work.

THE Royal Horticultural Society has issued the first part of vol. i. of its Journal. This part includes reports of the Vegetable Conference held at Chiswick on September 24, 25, and 26, 1889, and of the Chrysanthemum Conference held at Chiswick on November 5 and 6, 1889.

THE Transactions of the Congrès Colonial and the Congrès d'Hygiène et de Démographie, held in Paris last summer, have been issued. The Transactions of the latter Congress cover over 1200 octavo pages, and include many really useful papers.

MICHEL TROJA was one of the first surgeons who experimented (1775) on the regeneration of bone. His book, "De Ossium Regeneratione," has just been published, for the first time, in French.

THE last Annual Report of the Dutch Colonies in the East Indies contains references to several subjects of scientific interest. The military surveys were carried out on the west coast of Sumatra and in Dutch Borneo. In the former a large area was mapped on a scale of 1 : 20,000, and in Borneo a flying survey of 1 : 200,000 was made over a considerable district. Triangulation and cartographical work were continued in Sumatra; various maps were finished in Batavia; and the parts of the great map of Netherlands India, including the Residencies of Madura and Pasuruan, were put in hand at the Hague. The members of the Hydrographic Department were busy on the coasts of Java and Madura; an astronomical station was established on the Sunda Islands; and the study of the languages of the archipelago was continued by gentlemen appointed for the purpose—Balin, Javanese, Old Javanese, Macassar, Bugin, &c. There are 182 meteorological stations in working order, 100 in Java and Madura, 34 in Sumatra, 6 in Billiton and Banka, 9 in Borneo, 17 in Celebes, 2 in Bali, and the remainder at other points in the archipelago. Of scientific expeditions of various kinds a long list is given. These include geological investigations in Sumatra and Flores, botanical on Key Islands, ethnological in the Balta region of Sumatra, ethnological, botanical, and zoological, on the east coast of Borneo. An arrangement has been made, by which in each year one student from home will be able to spend some months in the famous Buitenzorg Botanical Gardens.

ANOTHER paper by Drs. Curtius and Jay upon hydrazine, N_2H_4, describing a new and very simple method of obtaining this recently isolated base from the ammonia addition compound of aldehyde, $CH_3 . C \left\langle \begin{matrix} H \\ OH \\ NH_2 \end{matrix} \right.$, is communicated to the latest number of the *Berichte*. The first step consists in acting with sodium nitrite upon a cold slightly acidified aqueous solution of aldehyde-ammonia, by which a nitroso-compound of the composition $C_5H_{11}O_2 . C \begin{matrix} \diagup H \\ \diagdown\!\!\diagdown N.NO \end{matrix}$ is formed. The reaction probably completes itself on the lines of the following equation—

$$3CH_3 . C \left\langle \begin{matrix} H \\ OH \\ NH_2 \end{matrix} \right. + NO . OH = C_5H_{11}O_2 . C \begin{matrix} \diagup H \\ \diagdown\!\!\diagdown N-NO \end{matrix} + 2H_2O + 2NH_3.$$

About 300 grams of aldehyde ammonia are dissolved in a little ice-cold water, and neutralized with cold dilute sulphuric acid. About 40 c.c. more of the dilute acid are then added, and afterwards a concentrated solution of 70 grams sodium nitrite in iced water. The liquid at once becomes turbid owing to separation of minute yellow globules of the nitroso-compound, termed nitroso-paraldimine, on account of its derivation from paraldehyde, the triple polymer of common aldehyde. This nitroso-paraldimine is a lemon-yellow liquid possessing an intense camphor-like odour. Its molecular weight has been determined by Hofmann's density method, and found to correspond with the formula above quoted. It decomposes at its boiling-point, but may be readily distilled in steam or *in vacuo* without suffering change. The imine itself, corresponding to the nitroso-compound, has also been isolated. The hydrochloride, $C_5H_{11}O_2 . C \begin{matrix} \diagup H \\ \diagdown\!\!\diagdown NH . HCl \end{matrix}$, is obtained when moist hydrochloric acid gas is passed through an ethereal solution of nitroso-paraldimine, in the form of a mass of white needles. From this hydrochloride the free base, paraldimine, $C_5H_{11}O_2 . C \begin{matrix} \diagup H \\ \diagdown\!\!\diagdown NH \end{matrix}$, may be obtained by treating its ether solution with silver oxide. Paraldimine is a clear colourless liquid of a sharp odour resembling that of paraldehyde. It solidifies to white crystals in a freezing mixture. It boils almost without change at 140° C., but polymerizes to a white solid on standing in a sealed tube for some weeks. Water or alcohol decompose it into paraldehyde and ammonia. Its hydrochloride, which is readily formed from the base with great evolution of heat by leading dry hydrochloric acid gas over the pure liquid, may be converted into the nitroso-compound by treating with a strong solution of sodium nitrite. The nitroso-compound itself, on reduction with zinc dust and dilute sulphuric acid, at once yields hydrazine sulphate, $N_2H_4 . H_2SO_4$. The course of the reaction is better seen when the gentler reducing mixture, zinc dust and glacial acetic acid, is allowed to act upon an ethereal solution of nitroso-paraldimine. An amide termed amidoparaldimine, $C_5H_{11}O_2 . C \begin{matrix} \diagup H \\ \diagdown\!\!\diagdown N . NH_2 \end{matrix}$, is then first formed, and may be

isolated as a strongly basic volatile liquid, which yields a very hygroscopic hydrochloride with hydrochloric acid. On boiling this hydrochloride with dilute sulphuric acid, it is decomposed, with assimilation of the elements of water, into paraldehyde and hydrazine—

$$C_5H_{11}O_2 \,.\, C\begin{matrix}\diagup H \\ \diagdown N\,.\,NH_2\end{matrix} + H_2O = C_5H_{11}O_2 \,.\, C\begin{matrix}\diagup H \\ \diagdown O\end{matrix} + N_2H_4.$$

The hydrate of hydrazine is readily obtained from the sulphate by simple distillation with alkalies.

THE additions to the Zoological Society's Gardens during the past week include an Egyptian Cat (*Felis chaus*) from North Africa, presented by Mrs. Florence J. Waghorn; a Stoat (*Mustela erminea* ♂), British, presented by Mr. Cuthbert Johnson; two Mantchurian Cranes (*Grus viridirostris*) from Corea, presented by Mr. Campbell; three Long-eared Owls (*Asio otus*), British, presented by Mr. W. Geoffrey N. Powell; a Black-faced Weaver-Bird (*Hyphantornis* sp. inc.), from South Africa, presented by Commander W. M. Latham, R.N., F.Z.S.; a Three-toed Sand Skink (*Seps tridactylus*), European, presented by Mr. J. C. Warburg; two Hybrid Deer (between *Cervus elaphus* ♂ and *Cervus sika* ♀), deposited; a Diana Monkey (*Cercopithecus diana* ♀) from West Africa, eight Undulated Grass Parrakeets (*Melopsittacus undulatus*) from Australia, purchased; a Rhesus Monkey (*Macacus rhesus*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at Greenwich at 10 p.m. on April 10 = 11h. 16m. 18s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G.C. 2386	—	—	11 15 47	+ 3 50
(2) 72 Leonis	5	Yellowish-red.	11 9 22	+23 42
(3) υ Leonis	4	Yellowish-white.	11 31 18	− 0 13
(4) δ Leonis...	2	White.	11 8 18	+21 8
(5) 152 Schj.	5	Red.	12 39 58	+46 3
(6) R Hydræ	Var.	Very red.	13 23 43	−22 43

Remarks.

(1) The General Catalogue description of this nebula is as follows: "Bright, pretty large, round, pretty suddenly much brighter in the middle." In 1869, Prof. Winlock observed the spectrum at Harvard College Observatory, and stated that it was continuous, with a possible bright line near λ 525. The nebula does not appear to have been spectroscopically examined by any other observer, so that further observations are required to confirm this result. If there really be a bright line as recorded, others may certainly be expected. Comparisons with the carbon flutings in the Bunsen or spirit-lamp flame spectrum should be made. It seems highly probable that many of the so-called "continuous" spectra of nebulæ really consist of bright lines or flutings superposed upon a continuous spectrum, as Dr. Huggins has stated that brighter parts have been suspected in some cases, and I myself have often noted irregularities, notably in the Great Nebula of Andromeda. In 1866 Dr. Huggins was careful to point out that his use of the term "continuous" was not to be understood to mean more than that, when the slit was made as narrow as the feeble light permitted, the spectrum was not resolved into bright lines.

(2) This star has a very fine spectrum of Group II. According to Dunér, the bands 2–8 are wide and dark, especially those in the red. This indicates, as I have pointed out on previous occasions, that the star is probably considerably advanced towards Group III., in which the bands will be replaced by lines. It will be interesting to know if any lines exist in the spectrum of the star at present, and, if so, what lines they are.

(3) A star of the solar type (Konkoly). The usual differential observations are required.

(4) A star of Group IV. (Gothard). Usual observations required.

(5) It is generally agreed that 152 Schj. is one of the finest examples of stars of Group VI. It shows the usual bands of carbon very strongly marked, and all of the secondary bands are well visible. We have certainly still a great deal to learn about stars of this group, and the present favourable position of a typical example may therefore be taken advantage of for further inquiry.

(6) At the last maximum of this interesting variable, Mr. Espin found that the F line was bright in its spectrum, the general spectrum being a very fine one of Group II. Mr. Espin also noted that the bright bands (probably the bright flutings of carbon) were relatively brighter as the star was on the increase, and weaker when its luminosity was decreasing. It is very important that a recurrence of these phenomena at the approaching maximum of April 11 should not escape observation, even though the star is not one which rises early in the evening at this time of the year. The period of the variable is about 434 days, but is apparently decreasing. In 1708 it was about 500 days. It varies from magnitude 4–5 at maximum to about 10 at minimum.

A. FOWLER.

THE APEX OF THE SUN'S WAY.—A determination of the amount and direction of solar motion is given by Mr. Lewis Boss in *Astronomical Journal* No. 213. This determination is an important one, because of the fact that, out of the 253 stellar motions used, only 49 are known to have been previously employed in a similar research, and it is by means of new material and variations of arrangements in its use that any general facts or laws are likely to be discovered. The stars whose proper motions have been utilized were given in No. 200 of the above journal, and are all contained in the Albany zone, which is 4° 20′ in breadth, and at a mean declination of 3° north of the celestial equator.

The method employed is substantially that proposed by Airy, and in the first solution five stars having proper motion greater than 100″ in a century were excluded, with the following results:—

	Mean magnitude.	Proper motion per century.	Maximum angular value of the solar motion for 100 years as viewed from the unit of distance.	R.A. of the apex of solar motion.	Decl. of the apex of solar motion.
		″	″	°	°
First series (135 stars)	6·6	21·9	12·39	280·4	+ 42·8
Second series (144 stars)	8·6	20·9	13·73	285·7	+ 45·1
Both series combined	7·6	21·4	13·09	283·3	+ 44·1
Probable errors	—	—	± 1·00	± 6·9	± 3·2

When stars are excluded whose proper motions per century amounted to 40″ or more, the following are the resulting values:—

		″	″	°	°
Single series (253 stars)	7·7	17·80	10·58	288·7	+ 51·5
Probable errors	—	—	± 0·60	± 7·2	± 3·2

The values of the several elements of solar motion, as determined by Struve and Bischof, are as follows:—

		″	″	°	°
Struve	6·0	8·00	4·36	273·3	+ 27·3
Bischof	7·5	47·58	33·67	290·8	+ 43·5
,, (using Argelander's method)			—	285·7	+ 48·5

By using the present declinations of the American ephemeris, Mr. Boss finds that the value given by Struve for the declination of the sun's way requires a correction of + 10°·4, thus making it + 37°·7, which is more in accordance with the other values given above.

The most probable co-ordinates of solar motion might therefore be assumed to be—

R.A. = 280°; Decl. = + 40°.

STABILITY OF THE RINGS OF SATURN.—The *Bulletin Astronomique* for February 1890 contains an interesting paper by M. O. Callandreau, on the calculations of the late Clerk-Maxwell, relative to the movement of a rigid ring around Saturn. It is well known that Laplace found it impossible for a homogeneous and uniform ring surrounding a planet to be in a state of stable equilibrium, and remarked that irregularities must exist in the

form of the ring, which, in combination with a slight eccentricity, secured its stability. Maxwell found that the irregularities of a ring possessing a permanent movement ought to be very sensible, and that the appearance of the rings of Saturn was incompatible with that required by his demonstration. He considered the case of a planet occupying the centre of the ring, whereas Laplace's hypothesis required a slight eccentricity. This question was not, however, treated separately, and M. Callandreau has subjected it to mathematical analysis. First, taking the case of a symmetrical ring when the centre of gravity will be on a symmetrical axis, and then the case required by Laplace, viz. that the centre of gravity is not exactly coincident with the geometrical centre, the author shows that the conditions stated by Laplace are not sufficient to ensure stability.

BROOKS'S COMET (*a* 1890).—This comet was observed at Paris on March 28 and 30. It was seen as a round nebulosity, about 40″ or 50″ in diameter, with a very pronounced central condensation, and was about the tenth magnitude.

BRIGHT LINES IN STELLAR SPECTRA.—The Rev. J. E. Espin reports the discovery of bright lines in the spectrum of θ_1 as well as in that of θ_2 Orionis, and possibly in that of S Coronæ as well.

ON THE DEFORMATION OF AN ELASTIC SHELL.[1]

THIS paper treats of the deformation of an elastic shell whose radii of curvature are everywhere great in comparison with the thickness, which is supposed uniform. The subject has been dealt with in a very able manner by Mr. A. E. H. Love in a recent paper (Phil. Trans., 1885), but it seemed desirable, on various grounds, that it should be attacked from an independent point of view. The method here followed is that explained in a former communication, "On the Flexure of an Elastic Plate" (December 1889). The results, as regards the general theory, are closely analogous with those of Mr. Love, and a comparison of the two investigations gives a physical interpretation to the various groups of terms which enter into his equations. There are some differences of detail, arising from a slight difference in the quantities chosen to express the flexural strains, but they are not practically important.

The great difficulty of the present subject, as contrasted with the theory for a plane plate, is, that we cannot draw an absolute line of demarcation between the deformations in which the cardinal feature is the extension of the middle surface, and those which involve flexure with little or no extension. This appears to arise mainly from the fact pointed out by Mr. Love, that it is in general impossible to satisfy the boundary conditions by a deformation in which the middle surface is absolutely unextended. But, this being admitted, the question remains in any specific problem, as to the amount and distribution of the extension, and, in particular, whether there are any modes of deformation (or of free vibration) in which, after all, it plays only a subordinate part. Mr. Love answers this question in the negative, in opposition to the views advocated by Lord Rayleigh in two well-known papers. In the present communication Mr. Love's argument is examined, and it is pointed out that cases may occur in which the extensions (though comparable with the flexural strains) may be confined to so small a region of the shell (near the edges) that their contribution to the total energy of deformation is insignificant.

In order to bring the matter to an issue in a definite instance, I have chosen the case of a cylindrical plate (such as a boiler-plate) bent by a proper application of force over its straight edges, so that the strained form remains a surface of revolution, the circular edges being free. The analytical work in this case is very simple, and the physical meaning of the various terms which occur is easily recognized. In the interpretation of the result it appears that a good deal turns upon the ratio which the breadth of the plate (in the direction of the generating lines) bears to a mean proportional between the radius and the thickness. If this ratio is large, the bending forces may be practically replaced by two equal and opposite couples uniformly distributed over the straight edges, and having these edges as axes. The strained form is almost accurately cylindrical; near the circular edges we have extensions of the same order as the flexural strains, but these rapidly die out (at the same time fluctuating in sign) as we press inwards, and the anticipation that their total energy would be small compared with that due to flexure is confirmed. In such a case, then, the approximate methods used by Lord Rayleigh, in which no account is taken of the conditions at a free edge, are fully justified. But if, keeping the radius and the thickness constant, we diminish the breadth of the plate until it is comparable with the mean proportional aforesaid, we get a sort of transition case between a plate and a bar, which cannot be satisfactorily treated except on the basis of the general equations. Finally, when the breadth becomes small in comparison with the mean proportional, the plate behaves like a curved bar, and an approximate treatment is again applicable.

In an appendix I have worked out, from the general equations of elasticity, the uniform flexure of an infinitely long cylindrical plate; this being, at present, the only case of flexure in which it appears easy to carry out the solution (on these lines) to a full interpretation.

[1] Abstract of a Paper read by Prof. Horace Lamb, F.R.S., before the Mathematical Society on January 9.

SCIENTIFIC SERIALS.

Timehri, being the Journal of the Royal Agricultural and Commercial Society of British Guiana (printed at the Argosy Press, Demerara, vol. iii., part ii., new series).—This interesting *brochure* contains matter of general interest, as well as information which might be expected in an agricultural and commercial journal. Specialization cannot be pushed to its extreme limits in a colony, and a Society of this nature naturally admits matter into its Journal which are not strictly either agricultural or commercial. Thus the papers on primitive games and on the wild flowers of Georgetown must be regarded, respectively, as of ethnological and purely botanical interest, but, nevertheless, occupy a great part of the number, especially if we leave out of consideration the reports of meetings and other official matter connected with the working of the Society. Fruit-growing in the Gulf States of America, Caracas as a place of resort, and a short paper on some scale insects inimical to vegetation are the principal topics of a distinctly economic value. The paper entitled the "Letters of Aristodemus and Sincerus" is a review of an old book published in 1785-88 in twelve volumes, dealing with the colonies of Demerara and Essequibo, and are therefore of great interest to the present population. In 1785 the colonies had just been given over by the French, who held them on behalf of the Dutch for about three years. No town existed up to that date in Demerara, but during the French occupation a little village had grown up in the neighbourhood of Brandwagt, which they called *la nouvelle ville*, or Longchamps. The fort on the east bank of the Demerara River (now called Fort William Frederick) was also built at the time, and named Le Dauphin, while another on the opposite side was called La Raine. From such historical, social, scientific, and economic materials a most interesting although somewhat diffusive number has been produced, showing evidence of mental activity and high culture, pleasant to see far away from the main centres of civilization. The style of the writing, the printing, and the illustrations are all of a high class. How far the London publisher, Mr. E. Stanford, of Cockspur Street, is responsible for the excellent "get up" of the volume we are unable to even conjecture; but we trust we may be permitted to say, without offence, that the number of *Timehri* before us is highly creditable to the literary talent and tastes of British Guiana.

Quarterly Journal of Microscopical Science, February.—On the anatomy of the Madreporia; V., by Dr. G. Herbert Fowler (plate xxviii.). Gives an account of the anatomy of *Duncania barbadensis*, *Galaxea esperi*, *Heteropsammia multilobata*, and *Bathyactis symmetrica*, and gives a figure of the typical structure of the genus Madrepora.—Contributions to the anatomy of earthworms, with descriptions of some new species, by Frank E. Beddard (plates xxix. and xxx.). This paper gives an account of the structure of three new species of Acanthodrilus, with remarks on other species of the genus. The new species are *A. antarcticus*, *A. rosæ*, and *A. dalei*. Further remarks on the reproductive organs of Eudrilus, with special reference to the continuity of ovary and oviduct.—On the certain points in the anatomy of Perichæta, with description of *Perichæta intermedia*, n.sp.—On the phagocytes of the alimentary canal, by Armand Ruffer (plate xxxi.). Concludes that the wandering cells of the lymphoid tissues of the alimentary canal have the power of proceeding to the free surfaces of such tissues, and of taking into their interior

lower micro-organisms and foreign matter (charcoal, &c.): there are both macro- and microphages; these are stages, the larger can swallow the smaller and digest them.—Notes on the hydroid phase of *Limnocodium sowerbyi*, by Dr. G. Herbert Fowler (plate xxxii.), records observations made during May 1888; neither medusoid or hydroid appeared in 1889; two hydroids and a budding medusoid are figured.—Note on certain terminal organs resembling touch corpuscles or end bulbs in intramuscular connective tissue of the skate, by Dr. G. C. Purvis (plate xxxiii.).—Note on the transformation of ciliated into stratified squamous epithelium as the result of the application of friction, by Drs. J. B. Haycroft and E. W. Carlier (plate xxxiii.).—On the development of the ear and accessory organs in the common frog, by Francis Villy (plates xxxiv. and xxxv.).—On *Thelaceros rhizophoræ*, n.g. et sp., an Actinian from Celebes, by P. C. Mitchell (plate xxxvi.). The Actinian here described was obtained by Dr. Hickson in a mangrove swamp in Celebes, by the side of one of the roots of a Rhizophora; the tentacles have compound hollow protuberances round the margins of the oral surface, with numerous small simple or compound hollow protuberances (rudimentary accessory tentacles) in radial lines on the oral disk.—Notes on the genus Monstrilla, Dana, by Gilbert C. Bourne (plate xxxvii.). Gives details of all the known species of this aberrant genus of Copepods.—On the maturation of the ovum, and the early stages in the development of Allopora, by Dr. Sydney J. Hickson (plate xxxviii.). Gives a general summary of events; the formation and fate of the trophodisc, the changes of the germinal vesicle, the formation of the embryonic ectoderm the history of the yolk, and general considerations.

SOCIETIES AND ACADEMIES

London.

Royal Society, March 27.—"The Variability of the Temperature of the British Isles, 1869–83 inclusive." By Robert H. Scott, F.R.S.

The material discussed has been the daily mean temperature derived from twenty-four hourly measurements of the thermograms at the seven British observatories during the period of their continuance, 1869–83.

The differences between the successive daily means have been extracted, irrespective of sign, and these values averaged monthly.

To the figures for the 7 observatories certain values have been added from Dr. Hann's paper in the *Sitzungsberichte* of the Vienna Academy for 1875 for Makerstoun and Oxford, the only British stations in Hann's list, and for Vienna, St. Petersburg, and Barnaul, as instances of Continental climates, as well as for Georgetown, Demerara, as an instance for a tropical station.

The figures for the 7 stations are much lower than those for Makerstoun and Oxford, probably owing to the fact that the means used in the two latter cases were not twenty-four hourly, nor for as many as fifteen years.

The highest variability on the mean of the year is at Kew (2°·7). Then follow Armagh, Glasgow, and Stonyhurst (2°·5), Aberdeen (2°·4), and Falmouth and Valencia (1°·9). The greatest absolute monthly value is 5°·4 for Glasgow, November 1880; the least, 0°·7, for Valencia, July 1879.

The mean values for each month are given.

The question of whether great changes are more frequently positive or negative has been investigated. Mr. Blanford states ("Climate of India") that in India (Calcutta and Lahore) sudden falls of temperature are more frequent and greater than sudden rises.

A preliminary inquiry showed that it was not interesting to investigate all changes, as the numbers showing + and − signs respectively were nearly equal.

The changes above 5° in the twenty-four hours were all examined, and the result showed that in these islands sudden rises of large amount are more frequent and more extensive in amount than sudden falls—the reverse to what obtains in India.

One instance of a rise of 23°·8 at Aberdeen, December 16, 1882, was the greatest recorded, and this disturbance was confined to the east of Scotland.

The figures were then examined for frequency. The values were arranged, irrespective of sign, according to their magnitude, in six subdivisions:—0·0°·9, 1·0–4°·9, 5·0–9°·9, 10·0–14°·9, 15·0–19°·9, 20·0–24°·9, and the totals divided by 15. The first two intervals taken together are equal to one of the others, but, as by far the greater number of the changes fell below 5°·0, it seemed well to see how many fell below 1°·0.

The range of changes is least at Falmouth and Valencia. In all cases the mean number of changes between 1°·0 and 4°·9 exceeds half the number of days in the month.

The daily mean values have also all been examined, with the view of discovering their distribution on the thermometer scale. Seven columns were taken, covering the space from 10° to 80°, of 10° each, excepting that the space from 20° to 40° was not divided equally.

In 1881, Stonyhurst had four days in January with a mean below 20°, and nineteen days in which the mean temperature was below 32°. At Aberdeen and Glasgow the cold was not so intense. Neither at Falmouth nor Valencia did the mean temperature ever fall below 20°. The hottest station is Kew. In the fifteen years it shows in all thirty-five days with a mean above 70°.

The figures were then divided by 15, to obtain frequency, as before, and the results shown. They are also shown graphically in a plate, but there all the curves do not appear. Those for Valencia and Falmouth agree almost exactly, except in July and August. Those for Armagh, Glasgow, and Stonyhurst are so close to each other, that one curve is taken to represent all.

Royal Microscopical Society, March 19.—Prof. Urban Pritchard, Vice-President, in the chair.—A letter from the President, regretting his inability to attend in consequence of a fall, was read.—Mr. J. Mayall, Jun., read a letter from Prof. E. Abbe, of Jena, announcing the donation of one of Zeiss's new apochromatic $\frac{1}{10}$ objectives of 1·6 N.A. He also sent a condenser of 1·6 N.A., and a flint glass slide containing mixed diatoms mounted by Dr. H. van Heurck, of Antwerp, together with a supply of flint glass slips and cover-glasses for use in mounting objects for examination with the new objective. It was of course understood that in order to exhibit the full power of the increased aperture it was necessary to employ a condenser of corresponding aperture, and the objects to be viewed must be mounted on slips with covers, and mounting and immersion fluids of correspondingly high refractive power. In order to further test this lens, a committee has been appointed. Mr. Mayall called attention to and described two microscopes by MM. Nachet and Pellin, of Paris, which were exhibited by Mr. Crisp.—Mr. Rousselet exhibited a number of Rotifers to show their abundance at this season of the year.—A specimen sent by Colonel O'Hara, supposed to be some kind of entozoon which had been passed in urine, was exhibited.—Prof. Bell gave a *résumé* of Mr. A. D. Michael's paper on the variations of the female reproductive organs, especially the vestibule, in different species of *Uropoda*, the author being unavoidably absent through illness.—Mr. C. H. Wright exhibited and described specimens of a new British Hymenolichen, *Cyconema interruptum*.—Mr. E. M. Nelson read a short note on the images of external objects produced from the markings of *P. formosum*.—A note was read from Dr. H. van Heurck correcting an error in his recent communication to the Society relating to the structure of diatoms.—Mr. Mayall read a translation of an article by Prof. E. Abbe on the use of fluorite for optical purposes, in which it appeared that the special qualities of the new apochromatic lenses were due to the employment of this mineral in their construction.—Mr. C. H. Gill read a paper on some methods of preparing diatoms so as to exhibit clearly the nature of the workings, which was illustrated by numerous photomicrographs.—Mr. P. Braham exhibited and described a new form of oxyhydrogen lamp adapted for microscopical purposes, the lamp being so mounted as to be used in any position above or below the object. Its application to photomicrography was demonstrated in the room.—Mr. Clarkson also exhibited one of the same lamps separate from the photomicrographic arrangement.—The next *conversazione* was announced to take place on April 30.

Zoological Society, March 18.—Prof. W. H. Flower, F.R.S., President, in the chair.—The Secretary exhibited (on behalf of the Rev. G. H. R. Fisk) a specimen of a White Bat, obtained at Somerset West, near Cape Town, believed to be an albino variety of *Vesperus capensis*.—Captain Percy Armitage exhibited and made remarks on two heads of the Panolia Deer (*Cervus eldi*), obtained on the Sittang River, Burmah. One of

these was of an abnormal form.—Mr. Sclater exhibited (on behalf of Mr. Robert B. White) examples of four species of Mammals, obtained in the Upper Magdalena Valley, in the department of Tolima, U.S. of Colombia.—Dr. Mivart, F.R.S., read a paper on the South-American Canidæ. The author called attention to the difficulties in the way of the correct discrimination of these animals, and to what appeared to him to be the unsatisfactory character of some of Burmeister's determinations and descriptions. Forms to which the names *fulvipes*, *griseus*, *patagonicus*, *entrerianus*, *gracilis*, *vetulus*, and *fulvicaudus* had been assigned were declared to be quite insufficiently discriminated from *Canis azaræ*. On the other hand, two very marked varieties, or possibly species, were noted and distinguished under the appellations *Canis parvidens* and *Canis urostictus*, the type of each of which was in the British Museum, both the skin and the skull extracted from it in each case.—Mr. R. I. Pocock read a revision of the genera of Scorpions of the family *Buthidæ*, and gave descriptions of some new South African species of this family.—Mr. F. E. Beddard read a paper on some points in the anatomy of the Condor (*Sarcorhamphus gryphus*).—A communication was read from Prof. R. Collett, containing the description of a new Monkey from North East Sumatra, proposed to be called *Semnopithecus thomasi*.

Geological Society, March 26.—J. W. Hulke, F.R.S., Vice-President, in the chair.—The following communications were read:—On a new species of *Cyphaspis* from the Carboniferous rocks of Yorkshire, by Miss Coignou, Cambridge. Communicated by Prof. T. McK. Hughes, F.R.S.—On composite spherulites in obsidian from hot springs, near Little Lake, California, by Frank Rutley, Lecturer on Mineralogy in the Royal School of Mines. The spherulites which form the subject of the present communication have been previously noticed, and it was then suggested that a smaller spherulitic structure was set up in the large spherules after their formation. In the present paper evidence was adduced in favour of a different mode of origin. It was argued that the small spherulitic bodies (primitive spherulites) were developed in the obsidian before it assumed a condition of rigidity, and that they floated towards certain points in the still viscid lava, and segregated in more or less spherical groups, though there is no evidence to show what determined their movements; furthermore, that from a point or points situated at or near the centre of each group, crystallization was set up, giving rise to a radiating fibrous structure, which gradually developed zone after zone of divergent fibres until the entire mass of primitive spherulites was permeated by this secondary structure—a structure engendering a molecular rearrangement of the mass, such as would obliterate any trace of structure which the primitive spherulites might have originally possessed. In a supplementary note the views of Mr. J. P. Iddings with reference to the spherulites in question were given. Mr. Iddings considers that the structures here described as primary are of secondary origin. The author stated in detail his reasons for adhering to the conclusions given in this paper. The Chairman said that the sequence of the different portions brought forward with so much care by the author is one which admits of much discussion. Rev. E. Hill said that the explanation of the divergence of these crystallizations was extremely interesting. As to which structure came first, it is difficult to determine. In the section exhibited under the microscope he agreed with Mr. Rutley as to the sequence. The question of molecular motion after consolidation in igneous rocks is a subject of great importance.—A monograph of the Bryozoa (Polyzoa) of the Hunstanton Red Chalk, by George Robert Vine. Communicated by Prof. P. Martin Duncan, F.R.S.—Evidence furnished by the Quaternary glacial-epoch morainic deposits of Pennsylvania, U.S.A., for a similar mode of formation of the Permian breccias of Leicestershire and South Derbyshire, by William S. Gresley.

PARIS.

Academy of Sciences, March 31.—M. Hermite in the chair.—M. de Jonquières, having presented a memoir containing the complete text and review of a posthumous work of Descartes, "De Solidorum Elementis," with a translation and commentary of the work, addressed a note giving some brief explanations of the matter contained in it. In communications made on February 10 and 17, the author endeavoured to show that Descartes knew and applied the relation between the faces, apices, and edges of a polyhedron, known as Euler's formula, and expressed as $F + S = A + 2$. The present communication seems to put the matter beyond doubt.—M. P. Schutzenberger, in reply to criticisms of M. Berthelot, adduces experiments pointing to the conclusion that the condensation of carbonic oxide by the silent discharge cannot be effected without the presence of water.—Some further remarks on the preceding communication, and on the desiccation of gases, by M. Berthelot. The author still holds the opinion that the water shown by M. Schutzenberger to be present in his condensed carbonic oxide may have passed through the glass tube under the action of the electric discharge.—A new method for the microscopical study of warm-blooded animals at their physiological temperatures has been devised by M. L. Ranvier, and consists of placing the microscope and the preparation under examination in a bath of warm water (36° C. to 39° C.).—Deformities of the feet and toes following phlebitis of inferior members; phlebitic club-feet, by M. Verneuil.—Observations of Brooks's new comet (*a* 1890), made at the Paris Observatory, by M. G. Bigourdan.—Observations of the same comet, made with the great equatorial of Bordeaux, by MM. Rayet and L. Picart.—Observations and elements of the new minor planet (289) discovered at the Nice Observatory on March 10, by M. Charlois.—On the position of the sun-spot of March 4, by M. Spoerer.—On the graphic statics of elastic arcs, by M. Bertrand de Fontviolant.—Theoretical and experimental researches on Ruhmkorff's coil, by M. R. Colley. The author has investigated the current which results from the superposition of two currents—one nonperiodic, diminishing according to the law of an exponential curve; the other periodic, and with progressively decreasing amplitude.—On the conductivities of the phenols and of oxybenzoic acids, by M. Daniel Berthelot. In this important paper the author gives the results of an examination of the three oxybenzoic acids by means of their electrical conductivities, and a research into the way they behave in the presence of one, two, or three molecules of soda. These acids having both phenol and acid functions, the conductivities of alkaline phenates were first determined.—The laws of annealing, and their consequences from the point of view of the mechanical properties of metals, by M. André Le Chatelier. These laws have been studied by heating metallic wires, hardened by a series of passages through a draw plate, to different temperatures and during different periods of time.—On the indices of refraction of salt-solutions, by M. B. Walter.—Action of hyposulphite of soda on silver salts, by M. J. Fogh. The amount of heat disengaged during the action of hyposulphite of silver upon various silver salts has been investigated.—M. V. Marcano, from his anthropological researches at Venezuela, gives evidence of the existence of metallurgy in South America previous to Columbus.—Influence of the chemical constitution of compounds of carbon upon the sense and variation of their rotary power, by M. Philippe A. Guye.—On the preparation and some of the properties of fluoroform, by M. Meslans. The density of the gas obtained is 2·44, and it is found to liquefy at 20° under a pressure of 40 atmospheres.—On some thiophenols derived from ordinary camphor, by M. P. Cazeneuve.—On the stranding of a whale on the island of Rhé, by MM. Georges Pouchet and Beauregard.—On the blood and the lymphatic gland of the Aphysia (sea-hare), by M. L. Cuénot.—On the method of union of sexual cells in the act of fecundation, by M. Léon Guignard.—On a new and dangerous parasite of the vine, by M. G. de Lagerheim. The description of the parasite is here given:—"*Uredo Vialæ:* Soris hypophyllis, solitariis majoribus vel dense gregariis minimis, solitariis in pagina superiore foliorum maculas parvas formantibus; uredosporis pyriformibus vel ovoideis 20μ–27μ longis, 15μ–18μ latis, membrana hyalina tenui aculeata et contentu aureo præditis, paraphysibus cylindricis curvatis incoloribus circumdatis. Hab in foliis vivis *Vitis* sp. parasitica in insula Jamaica, inter Kingston et Rockfort, Octob. 1889."—On the series of eruptions of Mézenc and Meygal (Velay); also a note on the existence of ægyrine in the phonolites of Velay, by M. P. Termier.—Composition of some rocks from the north of France, by M. Henri Boursault.—General results of a study of the carboniferous earths of the central plateau of France, by M. A. Julien.

BERLIN.

Physical Society, March 21.—Prof. du Bois Reymond, President, in the chair.—Dr. Brodhun described a new contrast-photometer, based on the principle of one he and Dr. Lummer had previously constructed (see NATURE, vol. xxxix. p. 336), and intended to compare by contrast the intensity of any

illumination with that of the standard light. Experiment had shown that the sensitiveness of the instrument is greatest when the difference of the contrasted illuminations is 3 per cent., and amounts then to ¼ per cent. He further gave an account of experiments which he and Dr. Lummer had made on the utilization of glow-lamps as standards of comparison. When fed by accumulators these lamps yield a light which only varies by 1 per cent. during a period of 200 hours provided the E.M.F. of the accumulators is kept constant. The authors are now busy with the endeavour to construct a standard glow-lamp for comparison with unknown sources of light. Dr. Lummer demonstrated Abbé's apparatus for testing transparent films with plane-parallel surfaces. After briefly describing the interference phenomena produced by thick plane-parallel glass plates, he explained how Tizeau's bands and Newton's rings are employed for testing the plates, using monochromatic sodium-light. The light passes through a reflecting prism and through a lens, and then falls on the plate, from which it is reflected and passes back by the same path to the eye, being now passed through a second lens by means of which the bands or rings may be seen. The occurrence of interference-bands is entirely dependent upon the thickness of the plate: if this is absolutely uniformly thick throughout, the interference phenomena show no change if the plate is moved from side to side in its own plane, and by so doing the parallelism of its sides may be rapidly tested.

AMSTERDAM.

Royal Academy of Sciences, February 22.—Prof. van de Sande Bakhuysen, in the chair.—Prof. Behrens added a number of reagents for microscopical analysis to those already known from former publications by himself and MM. Streng and Haushofer:—

For K and Na: sulphate of bismuth.
,, Ba, Sr, Ca: chloride of tin and oxalic acid.
,, Ba, Sr: bichromate of ammonium.
,, Sr, Ca, Mg: tartrate of sodium and potassium.
,, Al: fluoride of ammonium and sulphate of thallium.
,, Be: chloride of mercury and oxalic acid.
,, Ce, La, Di: oxalic acid, ferrocyamide of potassium.
,, Zn, Ca: acetate of aluminium and oxalic acid.
,, Zn, Cn, Co: sulphocyanide of mercury and ammonium.
,, Co, Ni: nitrite of potassium and acetate of lead.
,, Pb, Bi, Fe: bichromate of potassium and potash.
,, Bi, Sb, Sn: oxalic acid, chloride of rubidium.
,, Sb, Sn, Ti: chloride of barium and oxalic acid.

Details will soon be published, when the necessary finish has been given to the methods for separation, hitherto somewhat neglected.—M. Martin read a paper on the geology of the Kei Islands, and, in connection therewith, on the Australian-Asiatic boundary line. In accordance with the fact that in Great Kei we meet with nothing but a Tertiary formation, and that the nature of the rocks of Great Kei agrees with that of the coast of New Guinea, M. Martin inferred that this boundary line must be drawn geognostically, to the west of Great Kei and to the north-west of Timor.—Dr. Beyerinck treated of the luminous food and the plastic food of phosphorescent Bacteria. Of the six species of phosphorescent Bacteria hitherto known, four—viz. the alimental gelatine non-melting *Bacterium phosphorescens* and *B. Pflügeri* of luminous fish, and the Baltic phosphorescent Bacteria, *B. Fischeri* and *B. balticum*, require, besides peptone, a second carbonic combination, as glycerine, glucose, or asparagine, for their complete nourishment, *i.e.* to "phosphoresce" and grow. They may be called peptone-carbon-bacteria. The gelatine quick-melting phosphorescent bacteria from the West Indian Sea and the North Sea, *B. indicum* and *B. luminosum*, can phosphoresce and grow on peptone alone. They are, therefore, peptone-bacteria. Again, other bacteria can derive their nitrogen either from amids, the amid-bacteria, or from ammoniac, the ammoniac-bacteria. Also moulds, yeasts, and some Protozoa may be classed in this system. The *Bacterium Pflügeri* does emit light with peptone and glucose, but not with peptone and maltose, while the *Bacterium phosphorescens* emits light both with glucose and maltose. Now if we mix some starch in a phosphorescens-peptone-gelatine, obtained by mixing this gelatine with a very great number of *B. phosphorescens*, and place upon this some ptyaline, pancreas-diastase, or urindiastase (nefrozymase), fields of light make their appearance; if, however, we placed these same sorts of diastase on a Pflügeri-peptone-starch-gelatine, then no fields of light would appear, which proves that in this instance no glucose whatever is formed, as was lately believed to be the case. The development of luminosity is constantly accompanied by the transition of peptones into organized, living matter, under the influence of free oxygen, with or without the concurrence of another carbonic combination.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Among the Selkirk Glaciers: W. S. Green (Macmillan).—Flora Tangutica, fasc. i.: C. J. Maximowicz (Petropoli).—Enumeratio Plantarum Hucusque in Mongolia, fasc. i.: C. J. Maximowicz (Petropoli).—The Human Epic, Canto i.: J. F. Rowbotham (K. Paul).—Agende de Chimiste, Salet, Girard and Pabst (Hachette).—The Theory of Determinants in the Historical Order of its Development; Part i., Determinants in General: T. Muir (Macmillan).—The Microtomist's Vade-Mecum, 2nd Edition: A. B. Lee (Churchill).—Guide Pratique de L'Amateur Électricien: E. Keignart (Paris, Michelet).—Musiconomia o Leggi Fondamentali della Scienza Musicale: P. Crotti (Parma, Battei).—L'Éclairage Électrique Actuel, 2nd Edition: J. Couture (Paris, Michelet).—Das Reizleitende Gewebesystem der Sinnpflanze: Dr. G. Haberlandt (Leipzig, Engelmann).—Traité Ency. de Photographie, 15 Mars: C. Fabre (Paris, Gauthier-Villars).—Proceedings of the Aristotelian Society, vol. i. No. 3, Part 1 (Williams and Norgate).—Mind, April (Williams and Norgate).—Geological Magazine, April (K. Paul).—Quarterly Journal of Microscopical Science, April (Churchill).—Journal of the Royal Agricultural Society of England, 3rd Series, Part 1 (Murray).—Journal of the Royal Horticultural Society, vol. xii. Part 1 (London).

CONTENTS.

THURSDAY, APRIL 17, 1890.

THE GROWTH OF CAPITAL.

The Growth of Capital. By Robert Giffen. (London: G. Bell and Sons, 1889.)

THE popular conception of what statistics are is happily caricatured by a contemporary novelist, who describes an adept in that science stationing himself early in the morning at the entrance to a bridge, and, after scrutinizing the passengers for several hours, triumphantly reporting that exactly 2371 widows have crossed during the day. This arithmetic of the street is not the type of Mr. Giffen's calculations. His purpose is more philosophical, his method more elaborate.

The object which he seeks to measure is nothing less than the whole property, the accumulated exchangeable wealth, of the United Kindom. In this problem, to apprehend even the question requires an effort of intelligence. "Imagination shrinks from the task of framing a catalogue or inventory of a nation's property, as a valuator would make it." Reason points out that the grand total is not so much the value of the whole, which in its entirety cannot be considered saleable, as the sum of the values of all the parts, any one of which may be sold by its proprietor. The attribute of accumulation, as well as that of exchange, requires careful definition. Mr. Giffen, differing from some of his predecessors and contemporaries, does not regard the labourer himself as a species of capital. He does not, with Petty, attempt to determine the "value of the people," nor, with De Foville, to effect "the evaluation of human capital." However, some items which are of an incorporeal nature seem to enter into his account. Presumably, that part of the national capital which he reckons by capitalizing the income of public companies—multiplying it by a certain number of years' purchase—represents the value, not only of land and plant, but also of an immaterial something, which, in a broad sense, may be described as "custom" or "good will." Mr. Giffen doubts whether public debt should be admitted as an item of capital. He is certain that tenant-right should be excluded.

The uses of such a valuation are manifold. Mr. Giffen devotes a chapter to their enumeration. In the first place, it is desirable to compare our resources with our liabilities. It is satisfactory to find that the national debt compared with the national fortune is but a "bagatelle." The amount of a country's accumulations, and the rate of their increase, afford some measure of its capacity to endure the burdens of taxation, and, we may add, other kinds of pecuniary strain. It is observed by Newmarch, one of Mr. Giffen's predecessors in this department of statistics, that the investment in railways, which produced such convulsions in 1847–48, would have been in 1863 almost unfelt and insignificant in comparison with the yearly savings which were being made at the later epoch.

One use to which Mr. Giffen gives prominence may be thus described. The comparative growth of capital at different epochs serves as a sort of barometer of national prosperity. Of course those who use a barometer must remember that its indications of fair weather are but indirect and inferential. He who trusts the rising of the mercury when the north-east wind is blowing may get a wetting. So also with the metaphorical weather-glass. "The property test is useful as far as it goes, but it is not the only test," says Mr. Giffen. Elsewhere, in his address to the British Association, he has acted the part of a Fitzroy in considering together and interpreting in their connection the various tests and signs which economic meteorology affords. His object here is rather to perfect one particular instrument.

This barometrical use of capital may involve the necessity of correcting the estimates so as to take account of changes in the value of money. It may happen, it has happened, that in the last decade, as compared with the preceding period, the growth of capital estimated in money shows a falling off, while the increase of money's worth, of things, has not declined proportionately. To complete our measurement we must correct the measuring-rod. This is no easy or straightforward task. In the case of a real barometer we can mark the inches by reference to the standard yard measure, which is kept in the Tower. But a similarly perfect measure of value is, in the phrase of an eminent living economist, "unthinkable." The present generation finds itself, with respect to the variations in the value of money, in the sort of difficulty which must have occurred to the primæval man when first he may have noticed that a perfect measure of time was not afforded by the length of day and night, and before there had been constructed a more scientific chronometer. Even Mr. Giffen has to content himself with such rough and rather arbitrary corrections as the present state of monetary science affords.

As the object sought, the measure of accumulation, is somewhat hazy and difficult to envisage, so the method by which it is approached is indirect and slippery. The business man must not suppose that the estimates of a nation's capital can be totted up with the precision of a commercial account. The physicist is better prepared to appreciate the character of the computation, conversant as he is with observations which individually are liable to a certain error, while, put together, they afford certainty. But even physical observations, liable to a considerable yet calculable extent of error, hardly parallel the fallibility of these economic or, if we might coin a required word, *metastatistical* computations. In estimating that fallibility, we may usefully employ the analogy suggested by the theory of errors; but we must bear in mind the criticism to which this theory, even in its application to physics, was subjected by a witty mathematician: "After having calculated the probable error, it is necessary to calculate the probability that your calculation is erroneous."

The characteristic to which we draw attention is fully recognized by Mr. Giffen. Again and again he dwells on the rough and approximative character of his method, "the wide margin of error," and the "limit of information available." His cautions against reasoning too finely might have seemed superflous in their iteration, but that he doubtless anticipated the irrelevant criticism which each departmental statistician might direct against details—like the specialist in sculpture who, according to Horace, represents with peculiar accuracy the hair or nails, but *nescit componere totum*.

The futility of a penny-wise precision, and even of that criticism which sticks at a few thousand pounds where millions or tens of millions are the units of the scale, will be apparent when we consider the construction of the colossal account. The starting-point of the computation is afforded by the income-tax returns. The income under each head thus evidenced is multiplied by a certain number of years' purchase to form the corresponding item of capital. Thus, in the valuation of 1885 there is, under the head of "Houses," the income £128,459,000, which, being multiplied by 15, the number of years' purchase, gives £1,926,885,000 as the corresponding entry of capital. Again, under "Farmers' Profits," the income is £65,233,000, which, being capitalized at 8 years' purchase, makes £521,864,000 capital. Now, of course, neither are the income-tax returns perfectly accurate, nor can the number of years' purchase proper to each category be assigned with precision. A further element of uncertainty is introduced when, in the case of "Trades and Professions," we reduce the income-tax return by a somewhat arbitrary factor, one-fifth, in order to take account only of that income which results from accumulated property as distinguished from personal exertion. Where the income-tax is no longer available for our guidance, the procedure becomes even more precarious. Thus "Movable Property not yielding Income," such as furniture of houses and works of art, is estimated as amounting to half the value of "Houses," that is, £960,000,000. Even the most faithful follower of Mr. Giffen may be staggered when with reference to such entries he reads—

"The estimates of the income of non-income-tax paying classes derived from capital of movable property not yielding income, and of Government and local property, are put in almost *pro formâ* and to round off the estimates, and not with any idea that any very exact figures can be stated."

But whoever carefully considers the principles on which Mr. Giffen has assumed the different coefficients entering into his computation—principles set forth more fully in a former essay—will be satisfied that he has in no case run a risk of overrating. We may therefore accept his estimate of the national capital in 1885 as a figure round indeed, but not exaggerated. That figure is £10,000,000,000.

Greater precision may be attainable where there is required, not the absolute amount of capital in 1885, but the ratio of that amount to the corresponding estimate for 1875, in order to compare the growth of the national resources during that decade with the growth at a previous period. We shall now be assisted by the important principle which Mr. Giffen thus notices:—

"According to well-known statistical experience, the comparison of the growth or increment may be reasonably successful, if the same method is followed on each occasion in working out the data for the comparison, although these data themselves may be unavoidably incomplete."

Let us put our *quæsitum* in the form of a fraction, thus:—

$$\frac{\text{Lands in 1885} + \text{Houses in 1885} + \text{\&c.}}{\text{Lands in 1875} + \text{Houses in 1875} + \text{\&c.}}$$

(using lands, &c., as short for value of lands, &c.). It is evident that any source of inaccuracy which exaggerates or diminishes both the numerator and denominator in the same proportion is not operative on the result. If all the data were based on income-tax returns, and the same proportion of property escaped the net of the collectors at each epoch, the result would be undisturbed.

But all the data are not based on the income-tax; nor, even if there were no increased stringency in the collection of the tax as a whole, or any other general derangement, could it be supposed that the defalcations under each head observed an exactly uniform proportion. To estimate the effects of this unequal distortion, it will be convenient to alter our statement by putting in the numerator, instead of lands in 1885, the expression—

$$\text{Lands in 1875} \times \frac{\text{Lands in 1885}}{\text{Lands in 1875}},$$

with corresponding changes for the other entries. Thus the *quæsitum* may be considered as a sort of mean—a weighted mean—of the ratios between the several items for the two years. In this expression the influence which the two elements, the absolute quantities used as *weights* and the ratios, exercise upon the error of the result is different. The influence of error in the absolute quantities would be comparatively small, if those quantities were tolerably equal and the ratios not more unequal than they are. But, unfortunately, the absolute quantities are extremely unequal. Out of the twenty-six items, "Lands" and "Houses" together make up more than a third of the sum-total. By a formula adapted to the case, it may be calculated that, if each of the twenty-six quantities be liable to an assigned error per cent. (exclusive of such mistakes as, affecting the numerator and denominator of the result in an equal proportion, disappear in the division), then the percentage of error incident to the total result is not likely to be less than $\frac{3}{8}$ths of the error affecting each of the parts. That is, abstracting the inaccuracy of the ratios, which are of the form—

$$\text{Lands in 1885} \div \text{Lands in 1875}.$$

Now any error in the ratios is more directly operative on the result than the same degree of error in the absolute quantities. But, on the other hand, it may be that the error actually affecting the ratios is particularly small, owing to the favourable operation of that general principle which we have just now cited from Mr. Giffen's pages. The estimate of inaccuracy must, however, be increased to some extent by the error of the ratios. Altogether it would seem that the whole chain or coil is not so much stronger than the particular links or strands as is usual in the calculation of probabilities. It would be a moderate estimate that the percentage error of the compound ratio is not less than a half of the error on an average affecting each of the components—lands, houses, &c.—in either year.

What degree of error, then, shall we attribute to each of these items? A precise determination of this coefficient is, as we have already observed, impossible. It would be interesting to collect the estimates of competent authorities. As a mere conjecture, for the sake of illustration, let us entertain the supposition that the error (the effective error in the sense above explained) of any one item is as likely as not to be as much as 5 per cent., and

may just possibly be 20 per cent. Then we should ascribe half this degree of inaccuracy to the figure 1·175, which, according to Mr. Giffen's computation, is the ratio of the total capital in 1885 to the total capital in 1875. It would be conceivable that the real increase, as measured by some superior being, is not $17\frac{1}{2}$ per cent., but as little as 7, or as much as 27, per cent. Perhaps the defect is a little more likely than the excess, if there exist any constant cause making for depression such as the increased stringency of the tax-collectors in later years.

The growth of 17 per cent. in the decade under consideration may appear surprisingly small compared with the 40 per cent. recorded for the preceding decade. The general accuracy of the contrast is, however, confirmed by a comparison of the growths in each item for the two decades. Mr. Giffen points out that in the former decade, unlike the latter, there are no growths downwards. Also the percentages which measure increase run mostly at a higher level for the earlier period. His detailed examination of the figures leaves nothing to desire. For a summary contrast between the two sets of percentages we might submit that a proper course would be to compare the *medians* of the respective sets of figures (the arithmetic means would not be suitable owing to the very unequal importance of the figures relating to such miscellaneous items). Operating in the manner suggested, we find as the median of the first set of percentage growths 50, and of the second 25, thus confirming Mr. Giffen's conclusion that the former movement is about double the latter.

The conclusion that in the last decade our progress has been only half what it was in the preceding decade is at first sight disappointing. But we must remember that as yet we have accomplished only part of our calculation. We have still to make a correction for the change in the value of money which may have occurred between the two periods. This is a problem familiar to Mr. Giffen. In his classical computations of the changes in the volume of our foreign trade he encountered and surmounted a similar difficulty. In that case he ascertained the change in the level of prices at which exports and imports ranged in different years without going beyond the statistics of foreign trade, and by operating solely on the prices and quantities of exports and imports. It might be expected, perhaps, that he would pursue an analogous course in constructing a measure for the change of prices affecting the volume of capital. He would thus have been led to adopt the very ingenious method of measuring changes in the value of money which has been proposed by Prof. J. S. Nicholson. But, however cognate that original idea may be to the theory of the subject, it will be found in practice not easy to apply to the present computation. At any rate, Mr. Giffen has taken his coefficients for the correction in question, not, as before, from the subject itself, but *ab extra*, from Mr. Sauerbeck, Mr. Soetbeer, and the *Economist*. Averaging their results, he finds that money has appreciated to the extent of 17 per cent. during the interval under consideration. This correction being made, the growth of capital in the period 1875–85 proves to be about the same as the growth in 1865–75.

The soundness of this conclusion is confirmed by some reflections which at first sight might appear open to criticism. After using the fall of prices to prove the increase of capital, Mr. Giffen turns round and seems to reason from the increase of capital to the fall of prices.

"If two periods are compared in which the increase of population is known to be at much the same rate throughout, and the increase of productive capacity may be assumed to be at the same rate, or not less, in one of the periods than in the other, then, if the apparent accumulation of capital in the one period proved to be less than in the other, it must be ascribed to some change in the money values."

This reasoning may appear circular to the formal logician. But, in the logic of induction, we submit that it is very proper for two arguments archwise to support each other. The consilience of different lines of proof is indeed an essential feature of the logic of fact, as formulated by J. S. Mill. We venture to interpret Mr. Giffen's double line of proof by the following parable. Has it never occurred to you, reader, on looking at your watch, and finding the hour earlier than you expected, to suspect that the instrument has played you false? You review what you have been doing; recollect, perhaps, that you began work or got up earlier than usual; and, on reflection, see no reason to distrust your watch. You test the watch by the time, and you measure the time by the watch. Similarly, Mr. Giffen is quite consistent when he measures the extent of the growth of capital by the extent of the fall in prices; and confirms the fact of a fall in prices by the independently inferred fact of a considerable growth of capital.

In connection with the fall of prices we should notice an important contribution which Mr. Giffen makes to monetary science by defining the ambiguous term "appreciation." The readers of NATURE who may be more familiar with physical than social science will smile when they understand that there has been in economical circles a stiff controversy on the following question: Whether, if there is not now in circulation a sufficient amount of money—in proportion to the quantity of commodities circulated—to keep up prices to a former level, the cause of the fall is the scarcity of gold or the abundance of goods. It is as if, when the shoe pinched, people should dispute whether the shoe is too small, or the boot too large. The mirth of the physicist seems for the most part justified. However, as Coleridge or somebody said, before we can be certain that a controversy is altogether about words, there is needed a considerable knowledge of things. The better class of controversialists in the matter before us have doubtless had a meaning, but a latent and undeveloped one, which it required our author, like another Socrates, to bring to birth. The issue appears unmeaning, as long as you consider the question in Mr. Giffen's phrase "statically," without reference to the rate at which the quantity of goods and gold are growing. But "dynamically," if goods and gold cease to move abreast, it is intelligible to attribute the separation between the two to the operation of one rather than the other. As we understand the matter, using our own illustration, let us liken the constant growth of goods to the uniform velocity of a boat carried onward by a steady stream; and the parallel increase of money to the movement of a pedestrian on the bank. If the pedestrian, after keeping abreast with the boat for some time, is at

length found to be behind it, it is reasonable to attribute the change to the man, and not the stream. But all turns upon the assumed steadiness of the stream's onward movement. Looking back on past experience, Mr. Giffen entertains the hypothesis of a constant or "normal" growth of property. But with respect to recent years, it would be possible to cite, from other high authorities, expressions of a contrary opinion. But, if the steady motion of goods is not accepted, presumably the issue between "scarcity of gold" and the opposed theory of appreciation will turn upon a comparison of the rates at which the rate of increase varies for money and commodies respectively—an investigation of *second differentials* which we could not regard as serious.

The difficulties of monetary theory do not attend some of the uses to which the estimate of national capital may be applied. It is not necessary to make a correction for the variation of money when we compare our own with a foreign country in respect of absolute quantity, and even growth, of accumulation. Our colossal capital compares not unfavourably with the capital of the United States, perhaps equal in amount, but much less per head. The £10,000,000,000 of the United Kingdom compares favourably with the £7,200,000,000 of France weighted by a heavy debt, and the surprisingly small £1,920,000,000 of Italy.

The comparison of provinces, as well as nations, is also instructive. Mr. Giffen finds that Ireland has less than a twentieth of the property belonging to the United Kingdom. The property per head in Ireland is less than a third of what it is in England, and not much more than a third of what it is for Scotland. Upon these facts Mr. Giffen remarks:—

"Reckoning by wealth, England should have 86 per cent. of the representation of the United Kingdom, or 576 members out of 670; Scotland, by the same rule, should have about 64 only; and Ireland no more than 30. . . . There should be a representation of forces in Parliament, if we had perfectly just arrangements, and not merely a counting of heads. Nothing can be more absurd to the mind of any student of politics, who knows how forces rule in the long run, than the system now established, as between the metropolitan community of England and its companions in sovereignty, by which one of the companion communities, and that the least entitled to privilege, obtains most disproportionate power."

One of the most legitimate uses to which estimates of national capital can be put, is to ascertain the progress of wealth from age to age. In an historical retrospect, Mr. Giffen reviews the work of his predecessors, rescuing from an undeserved neglect more than one writer who had the courage and sagacity to employ what Colquhoun calls "approximating facts." The succession of estimates, from the age of Petty to the present time, appears to justify the hypothesis of a constant increase of property—a five-fold multiplication per century. Contemplating the long series of records, Englishmen may reflect with pride that the increased estimates are matched by an increasing power of handling them, that the growth of material prosperity has not been attended by a decline in statistical genius, and that the work of Petty is continued by one who is worthy to be compared with the founder of Political Arithmetic.

F. Y. E.

MERGUI.

Contributions to the Fauna of Mergui and its Archipelago. 2 Vols. (London: Taylor and Francis, 1889.)

THE materials which have been brought together in these volumes are now made accessible to those specially interested in the fauna of this group of islands in a connected form. The collections were made in 1881-82 by Dr. John Anderson, F.R.S., till recently Director of the Indian Museum at Calcutta, who brought the specimens to England with him, and placed the different groups in the hands of specialists for their proper identification and description. The result has been the publication of a number of faunistic papers in the Journal of the Linnean Society and elsewhere, and these papers are now published in the form of two volumes, well illustrated with plates, and containing altogether nearly two dozen distinct memoirs by recognized authorities in the different departments.

In the first volume Prof. P. Martin Duncan writes on the Madrepores, and in his concluding remarks calls attention to the remarkable distinctness of the existing as compared with the Miocene corals of the same area. Prof. F. Jeffrey Bell's paper on the Holuthuria comes next in order, and is followed by Mr. F. Moore's paper on the Lepidoptera, the collection in the last order containing 208 species of butterflies, and 64 species of moths. The Sponges are described by Mr. H. J. Carter, F.R.S., and the Ophiuridæ by Prof. Martin Duncan, who contributes also a special paper on the anatomy of *Ophiothrix variabilis* and *Ophiocampsis pellicula*. The Polyzoa and Hydroida are taken in hand by the Rev. Thomas Hincks. The Coleoptera have come off badly, if Mr. Bate's description of one new species (*Brachyonychus andersoni*) represents the whole of the material collected in this order. We suspect, however, that more will be heard about the Mergui beetles at some future period.

Dr. Anderson himself contributes the list of birds, which he regards "merely as a small supplementary contribution" to Messrs. Hume and Davison's labours in the same field. The list chiefly records the distribution in the outer islands of the archipelago of a few of the species recorded by these last authors. Dr. Hoek, of Leyden, writes on a Cirriped (*Dichelaspis pellucida*), which does not appear to have been observed since Darwin published his original description in his monograph. The shells—marine, estuarine, freshwater, and terrestrial—form the subject of a paper by Prof. E. v. Martens, of Berlin. Mr. Stuart Ridley has been entrusted with the Alcyonaria, and Prof. A. C. Haddon describes two species of Actiniæ. The Annelids are treated of by Mr. Frank E. Beddard, who includes in his paper an important section on the structure of the eyes in one of the species described. The Pennatulida are treated of by Prof. Milnes Marshall and Dr. G. H. Fowler, and the Myriopoda by Mr. R. I. Pocock, this being the first list of species recorded from the archipelago. The Comatulæ are described by Dr. P. Herbert Carpenter, the Echinoidea by Prof. P. Martin Duncan and Mr. W. P. Sladen, and the Asteroidea by this last author. These organisms, when referable to known species, "show variations which are sufficient to impart a character to the collection as a whole, and to indicate

the existence of local conditions whose action upon types of a more plastic nature than that of the series of forms so far collected would probably result in new morphological developments." Mr. Sladen further throws out the suggestion that the Mergui area "may be looked upon as a moulding ground wherein Malayan types assume a modified form." A description of the physical conditions prevailing in the localities where the Asteroidea were collected is contributed by Dr. Anderson, and adds much to the value of this paper. The paper on the Mammals, Reptiles, and Batrachians is by Dr. Anderson, the three classes being represented by 23, 53, and 12 species respectively. The whole of the second volume, containing over 300 pages and 19 plates, is devoted to the Crustacea, the author entrusted with this order being Dr. J. G. de Man, of Middleburg, Netherlands. It should be added that this part of the work relates only to the stalk-eyed Crustacea.

The names of the different specialists who stand responsible for their respective contributions are sufficient guarantee that Dr. Anderson and the Calcutta Museum have been the means, aided largely by the Linnean Society, of giving to the public a substantial and trustworthy contribution to the natural history of a much-neglected group of islands. The proximity of the archipelago to the main land of course precludes the possibility of expecting much in the way of insular forms. There is one paper, however, contributed by Dr. Anderson, and forming the second part of the first volume, which will be read with interest by anthropologists, as it contains a description of a peculiar race of sea gipsies called "Selungs," who frequent the archipelago and inhabit many of its islands. These people appear to be sufficiently distinct from those of the main land to warrant their being regarded as an insular race, probably having Malayan affinities. At any rate, all that we know about them at the present time is contained in the paper referred to, which is accompanied by two photographic groups of the people, a photograph of their boats, and a lithographed plate of their weapons and utensils. There is also a vocabulary of their language, which, according to General Browne, bears not the slightest affinity to Burmese, but which Dr. Rost reports to be distinctly Malayan.

R. M.

HOW TO KNOW GRASSES BY THEIR LEAVES.

How to know Grasses by their Leaves. By A. N. M'Alpine. (Edinburgh: David Douglas, 1890.)

THIS little book will be a valuable aid to agriculturists and agricultural students. It is small, and adapted for carrying in a side pocket. It comes out seasonably, as the time is fast approaching in which its teaching may be verified in the field. It fills a gap in our knowledge of grasses, as botanists usually decide species by the inflorescence, rather than by the leaves. Colour, habit of growth, and form of leaf, are, we know, somewhat variable characters, and cannot always be relied upon; and in questions relating to the absolute identification of species, no doubt, inflorescence is of first importance. There is, however, a practical knowledge which derives immense benefit from the kind of information contained in Mr. M'Alpine's work, and after having determined approximately the component parts of a pasture in the young state, it is open to the observer to wait for further proof in the spike or panicle, which will in due time appear. A grass-field contains a larger number of species, not only of grasses but of clovers, other leguminous plants, and miscellaneous herbage, belonging to the *Compositæ*, *Umbelliferæ*, *Rosaceæ*, and other natural orders. This book treats solely of the grasses, and clearly, and with the help of 200 figures, shows how any person may identify grasses in the leafy stage. "The difficulties connected with the identification of grasses in the flowerless condition," says Mr. M'Alpine, "are not at all so great as usually supposed." This is good news from the botanist of the Highland and Agricultural Society of Scotland, Professor of Botany in the New Veterinary College, Edinburgh, and translator of Stebler's "Best Forage Plants." The great and varied knowledge of Mr. M'Alpine, is in itself a guarantee that the distinctions he has traced between the blades and stems of grasses are not of a hasty or flimsy character. Many of them are new to us, but others we have noticed ourselves, and know them to be correct. Any one furnished with a copy of this little book, and a small magnifier, will find that an additional interest will be communicated to walks in the fields, and the question as to the nature of the growing herbage of pastures may be satisfactorily answered. An eye trained to observation will be able to detect slight differences better than the eye which sees not, but we feel confidence that a careful examination of the plates and the letterpress of this little book will, if used in the field, be in itself a training in habits of observation. The book should be in the hands of every agricultural student, as it in due time will become the basis of questions at examinations. The facts that Mr. M'Alpine is himself a teacher, and that Prof. Wallace, of Edinburgh University, has written the preface, point to this conclusion.

The price for so small a book (3*s*. 6*d*.) certainly appears very heavy; but if it is called for in sufficient numbers, we shall doubtless soon hear of a cheaper edition. The demand for books of this class is small, as most farmers do not read more than is good for them, and the subject is not of great interest to the general reading public.

The classification adopted by Mr. M'Alpine is not that of genera and species. For example, rye-grasses (*Lolium*) and meadow fescue (*Festuca*) are grouped together, as having red bases to their stems; crested dog's-tail grass is peculiar for a yellow stem base; meadow fox-tail, for a dark or almost black stem base; Yorkshire fog, for having a white sheath, with red veins. These colours at the base of the stem, taken together with other characters, are used to identify the species, and the grasses which are known by the colours just enumerated form a group described as "characteristically coloured grasses." Group II. includes variegated grasses, whose leaf-blades are composed of alternate strips of white and green tissue. Group III. includes bulbous grasses, with low, flat ribs, such as Timothy grass and false oat grass. Group IV., cord-rooted grasses in hill pastures, such as mat grass and purple Molinia. Group V., acute sheathed grasses, so named on account of their sharp edges. The shoots are quite flat on the sides and the edges acute—such are cocksfoot and rough-stalked meadow grass. Group VII., bitter tasted grasses. Group VIII., bristle-

bladed grasses. Group X., hairy grasses. Group XII., ribless bladed grasses. Groups VI., IX., and XI. are separately dealt with, but those above-mentioned will sufficiently show the principle upon which the classification is made.

The figures (diagrams), showing the tapering, obtuse, flat, involute, or imbricate character of the herbage, are exceedingly plain and characteristic, and will be of great assistance to the observer in the field. The leaf-blades, stems, ligules, sheaths, &c., are well shown in cross-sections, and at length. JOHN WRIGHTSON.

OUR BOOK SHELF.

Facsimile Atlas to the Early History of Cartography, with Reproductions of the most important Maps printed in the Fifteenth and Sixteenth Centuries. By A. E. Nordenskiöld. Translated from the Swedish original by J. A. Ekelöf and Clements R. Markham. (Stockholm, 1889.)

IN this handsome volume there are 142 pages of letterpress in imperial folio, and 51 plates in double folio. It contains reproductions of about 160 of the rarest and most important maps printed before the year 1600. Among these are the 27 maps of Ptolemy, edited by Schweinheim-Buckinck in Rome, 1478 and 1490; maps from Berlinghieri's "Geographia," Firenze, c. 1478; Aeschler's and Übelin's "Ptolemy" of 1513: Reisch Margarita Philosophica, of 1503 and 1515; Lafreri's "Atlas," Romæ, c. 1570; Richard Hakluyt's "Petrus Martyr," Paris, 1587, and "Principal Navigations," London, 1599; maps of the world, by Ruysch, 1508, Bernardus Sylvanus, 1511, Hobmicza, 1512, Apianus, 1520, Laurentius Frisius, 1522, Robert Torne, 1527, Orontius Finacus, 1531, Grynæus, 1532, Mercator, 1538, Girava, 1556, de Judæis, 1593. We find also the first modern printed maps of the northern regions, of the Holy Land, of Central Europe (by Nicolas a Cusa), of France, of Spain, of England, of Russia; the first charts for the use of mariners published in print; 82 general maps, or maps referring to the New World; the first modern printed maps of Africa; the first map illustrating the distribution of religious creeds, &c.

As regards the text, chapters i.–iii. contain researches relating to the influence of Ptolemy on modern cartography, his merits and defects, and the different editions of his geography. Of the editions enumerated in bibliographical works, 27 spurious ones are neglected. In chapter iv. a review is given of ancient maps other than Ptolemaic, of the portolanos and their influence on modern geography. Chapter v. treats of the extension of Ptolemy's *Oikumene* towards the north and north-west, the pre-Columbian maps of Scandinavia and Greenland, the most remarkable of which is one discovered by Nordenskiöld himself in a library at Warsaw (reproduced on Tab. xxx.) Chapter vi. deals with the first maps of the New World, and the then recently discovered parts of Africa and Asia. Here the author draws attention to the hitherto neglected fact that maps from Vasco de Gama's second voyage were printed as early as 1513 (reproduced in the letterpress, Figs. 8–10). Chapter vii. gives an account of early terrestrial globes, and in chapter viii.—on map projection—the author corrects several errors generally adopted in the history of this part of cartography. In chapter ix. he deals with the end of the early period of cartography, and in chapter x. with the transition to, and the beginning of, the modern period. He brings out the importance of the work of Jacopo Gastaldi, Philip Apianus, Abraham Ortelius, and Gerhard Mercator, in the development of cartography. He also gives, besides a catalogue of the maps in Lafreri's "Atlas," a critical review of Ortelius's celebrated "Catalogus Auctorum tabularum geographicarum."

The work is based on Baron Nordenskiöld's private collection of ancient printed maps. This collection he began to make many years ago, and it is now rich in documents from the periods reviewed in the present "Atlas."

The maps have been excellently copied and printed, and the great care taken by the librarian, Mr. W. E. Dahlgren, has secured the correctness of the citations. All geographers who have a right to an opinion on the subject will agree that the work is indispensable to every library in which there is a department devoted to geography.

Light and Heat. By the Rev. F. W. Aveling, M.A., B.Sc. Second Edition. (London: Relfe Bros., 1890.)

THIS is a new edition of a text-book intended to prepare candidates for one of the science subjects of the London matriculation. It has been much improved since its first appearance, but it still treats the subject in a very superficial way. Although no one could seriously study the subject with this as a guide, it is certainly a useful summary of the main facts, and will probably be found serviceable by intending candidates. The coloured plate of spectra has been corrected, but surely this is superflous in a book which does not even describe an ordinary student's spectroscope. The author has fallen into the very common error of stating that the electric arc gives a continuous spectrum, and he also states that the lines in the spectra of the fixed stars are different from those which characterize sunlight; whereas in a great many cases they are practically identical.

There are numerous diagrams, but they are barely of a quality equal to those which would be produced by a student at an examination. The large collection of questions and answers will be very useful.

Warren's Table and Formula Book. By the Rev. Isaac Warren. (London: Longmans, Green, and Co., 1889.)

WE have in this small work a compact and trustworthy set of tables, facts, and formulæ. which come within the scope of an ordinary education. As a reference book, it should prove most useful, the information it conveys being concise and to the point. In addition to the usual tables of weights and measures, &c., we have an account of the physical and electrical units now in use, followed by the most important formulæ used in algebra, mensuration and trigonometry, and tables of exchange, principal units of value throughout the world, and comparative average values of some important coins, the last of which will doubtless be found useful to those travelling abroad. Some of the most important business forms, such as "Form of a Joint Promissory Note," "Form of Foreign Bill of Exchange," &c., are printed in full; and the work concludes with postal and telegraph rates. On the back of the cover are printed diagrams of a square decimetre and centimetre and a square inch, together with scales of centimetres and inches.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

"Panmixia."

THE somewhat strained argumentation which Mr. Romanes has devoted in your issue of April 3 (p. 511) to my defence of Mr. Darwin's position in regard to "cessation of selection" and "economy of growth" does not convince me of the justice of the former's claim to have originated new principles "unfortunately" (to use his own expression) too late for Mr. Darwin to have the advantage of correcting himself by their aid. In his letter of March 13 (p. 437) Mr. Romanes lays great stress in

criticizing Weismann upon what he calls "reversal of selection," which he now tells us is the same principle as "economy of growth." Yet in the earlier letter he entirely omits to credit Mr. Darwin with the recognition of that principle, and after carefully asserting that Mr. Darwin had overlooked the principle of "panmixia," he gives in an historical form what *he* (Mr. Romanes) had argued some years ago, and what *his* views were—including herein the principle of economy of growth, or more generally, reversed selection. Now that the oversight has been pointed out to him Mr. Romanes allows that "it is a matter of familiar knowledge that Mr. Darwin at all times, and through all his works, laid considerable stress upon the economy of growth (or more generally, reversed selection)."

Mr. Romanes makes an unreal separation between "cessation of selection" and "reversal of selection"; at the same time, for the mere purpose of *badinage*, he affects to suppose that I do not perceive any difference between them—a supposition which cannot be sincere in view of the statements in my letter of March 27. Cessation of selection is not a "principle" at all. It is a condition which alone cannot produce any important result. At the same time, what Mr. Romanes misleadingly calls "reversal of selection," viz. "economy of growth," cannot become operative in causing the dwindling of an organ until the condition of "cessation of selection" exists. The fact is—as Mr. Romanes insisted before it was pointed out in these pages that it was no new principle of his own discovery, and when he wished to lay claim to an improvement upon Weismann's exposition of "panmixia"—cessation of selection must be supplemented by economy of growth in order to produce the results attributed to "panmixia." And inasmuch as economy of growth as a cause of degeneration involves the condition of cessation of selection, Mr. Darwin, in recognizing the one recognized the other.

By the use of the term "the principle of the cessation of selection" Mr. Romanes has created an unnecessary obscurity. To say that a part has become "useless," or "has ceased to be useful to its possessor" as Mr. Darwin does, is clearly the same thing as to say that it "has ceased to be selected"—selection and use being inseparable. Mr. Darwin states that such parts "may well be variable, for their variations can no longer be checked by natural selection." That is panmixia. It is true that Mr. Darwin did not recognize that such unrestricted variation must lead to a diminution in size of the varying part without the operation of the principle of "economy of growth." This was no strange oversight: he would have been in error had he done so. On the other hand, he did recognize that, given the operation of that principle, the result would amount to the dwindling and degeneration of parts which are referred to as rudimentary.

"Panmixia" as a term clearly refers to the unrestricted interbreeding of all varieties which may arise, when selection in regard to a given part or organ is no longer operative. The term, like its correlative "cessation of selection," does not indicate a principle but a natural condition: it does not involve the inference that a dwindling in the *size* of the organ must result from the inter-breeding; but simply points to a precedent condition.

I am by no means prepared to admit that panmixia alone (*i.e.* without economy of growth or other such factors) can be relied upon, as it is by Mr. Romanes, to explain the reduction in size of the disused organs of domesticated animals. I observe that in his letter on this subject to NATURE of April 9, 1874, Mr. Romanes does not attempt to attribute a dwindling action to "panmixia" alone, but assumes a limitation by economy of growth to any increase beyond the initial size of the organ which has become useless. Given this limitation and the condition of panmixia, the dwindling follows; but it is absurd to attribute the result, or any proportion of it, to the panmixia or cessation of selection alone. On the other hand, when we consider shape and structure, and not merely size, it is clear that panmixia without economy of growth would lead to a complete loss of that complex adjustment of parts which many organs exhibit, and consequently to degeneration without loss of bulk. That the principle of economy of growth is ever totally inoperative has not been demonstrated. E. RAY LANKESTER.

April 9.

Heredity, and the Effects of Use and Disuse.

ALL biologists will, I am sure, agree as to the desirability of a thorough testing of the hypotheses relative to the inheritance of the effects of use and disuse. As Mr. Spencer says, in the preface to "The Factors of Organic Evolution":—"considering the width and depth of the effects which acceptance of one or other of these hypotheses must have on our views of Life, Mind, Morals, and Politics, the question—Which of them is true? demands, beyond all other questions whatever, the attention of scientific men."

As experiments suggested by those who believe in the inheritance of the effects of use and disuse would hardly carry the weight to those who do not believe in this inheritance which experiments proposed by themselves would, I write to suggest the desirability of undertaking an investigation which, Prof. Weismann thinks, would prove one or other hypothesis. He states it in the following words on p. 91 of the English edition of his "Essays":—

"If it is desired to prove that use and disuse produce hereditary effects without the assistance of natural selection, it will be necessary to domesticate wild animals (for example the wild duck) and preserve all their descendants, thus excluding the operation of natural selection. If then all individuals of the second, third, fourth, and later generations of these tame ducks possess identical variations, which increase from generation to generation, and if the nature of these changes proves that they must have been due to the effects of use and disuse, then perhaps the transmission of such effects may be admitted; but it must always be remembered that domestication itself influences the organism,—not only directly, but also indirectly, by the increase of variability as a result of the suspension of natural selection. Such experiments have not yet been carried out in sufficient detail."

If Profs. Weismann, Romanes, and Lankester, would agree to some such experiment as the above as definitely proving the point in question (I say "definitely," for the sentence which reads "if the nature of these changes proves that they must have been due to the effects of use and disuse," seems rather to beg the whole question, even if the experiment were carefully carried out), there are two ways in which it might be effected. One is, that the British Association, which by devoting time to the discussion of the hypothesis has shown an appreciation of its worth, should at its next meeting appoint a committee, with a small grant for necessary expenses, to carry out the investigation. The other is, that it should be undertaken independently by the foremost of those on both sides who are interested in this question, and who would no doubt subscribe among themselves enough for the purpose in view—at least, speaking for myself, I should not object to contribute to the expenses of a properly planned investigation.

Regarding the place where the "wild ducks," or possibly some animal with a more frequent recurrence of broods, should be located for observation, I would suggest that the Zoological Society should be asked to afford space in their Gardens at Regent's Park. F. HOWARD COLLINS.

Churchfield, Edgbaston.

Galls.

THE difficulty raised by Mr. Wetterhan (NATURE, February 27, p. 394) appears at first sight a serious one, but I think it vanishes on examination. Supposing the attacks of the insects to be constant, trees in their evolution would have to adapt themselves to these circumstances, just as they have adapted themselves to the environment of soil, air, light, wind, and so forth. But the fallacy (as it seems to me) of Mr. Wetterhan's argument lies in the supposition that the life of an oak-tree as such, and the life of an insect, may rightly be compared. A tree is really a sort of socialistic community of plants, which continually die and are supplanted by fresh. Bud-variation is a well-known thing, and in oaks A. de Candolle found many variations on the same tree. Now is it unreasonable to suppose that internal-feeding insects might take advantage of such variation—or rather, be obliged to take advantage of it, if it were in a direction to benefit the tree? I will give two purely hypothetical instances, to illustrate the points involved. Imagine two oak-trees, each with three branches, and each attacked by three internal-feeding insec . The insects infesting one tree are borers; those on the other tree are gall-makers. The borers bore into the branches, which they kill while undergoing their transformations: the tree possibly does not die that year, but next year the progeny of the three, being more numerous while the tree is weaker, effect its destruction, and finally the insects perish for want of food. On the other tree, the gall-makers do no appreciable damage, and the tree is

able to support them and their progeny without great difficulty. Now a little consideration will show that the longer the life and the slower the reproduction of the trees, the greater will be the contrast. If the plant infested by the borers had been an annual herb, it might have contrived to perfect its seeds, and the death of the old stem would be but a natural and inevitable process, and fresh plants might have been produced in sufficient numbers to continue the species in spite of all insect-attacks. But in the case of trees—oak-trees especially, the rate of growth and reproduction is such that, unless the insect-borers can live in galls, they will destroy the plants entirely, and themselves in consequence. Indeed, I have no doubt, that if all the gall-makers now existing could suddenly be transformed into stem-borers, the genera *Quercus*, *Rosa*, and *Salix*, now so dominant, would shortly disappear from off the face of the earth. The other hypothesis—here assuming that the production of galls is due more to the tree than the insect—is this. Suppose an oak-tree with four branches, all attacked by internal-feeding insects. Two of the branches produce swellings in which the insects live, while the other two produce none, and the insects have to devour the vital parts. Now the two branches which produced no swellings would quickly be killed by the insects, but those which produced galls would live, and the more perfect the galls, the greater the insect-population they would be able to support. Hence the tree would finally, by the survival of its gall-producing branches, become purely gall-producing, and we may assume that its progeny would inherit the peculiarity.

I am aware that the above arguments will sound a little like those of the Irishman, who said he ought not to be hanged, because, "in the first place, he did not kill the man; in the second place, he killed him by accident; and thirdly, he killed him in self-defence,"—but I do not represent either of the above hypotheses as the precise truth of the matter, and I think they sufficiently illustrate the principles involved.

T. D. A. COCKERELL.

West Cliff, Custer Co., Colorado, March 16.

On the Use of the Edison Phonograph in the Preservation of the Languages of the American Indians.

THE present state of perfection of the Edison phonograph led me to attempt some experiments with it on our New England Indians, as a means of preserving languages which are rapidly becoming extinct. I accordingly made a visit to Calais, Maine, and was able, through the kindness of Mrs. W. Wallace Brown, to take upon the phonograph a collection of records illustrating the language, folk-lore, songs, and counting-out rhymes of the Passamaquoddy Indians. My experiments met with complete success, and I was able not only to take the records, but also to take them so well that the Indians themselves recognized the voices of other members of the tribe who had spoken the day before.

One of the most interesting records which was made was the song of the snake dance, sung by Noel Josephs, who is recognized by the Passamaquoddies as the best acquainted of all with this song "of old time." He is always the leader in the dance, and sang it in the same way as at its last celebration.

I also took upon the same wax cylinder on which the impressions are made his account of the dance, including the invitation which precedes the ceremony.

In addition to the song of the snake dance I obtained on the phonograph an interesting "trade song," and a "Mohawk war song" which is very old. Several other songs were recorded. Many very interesting old folk-tales were also taken. In some of these there occur ancient songs with archaic words, imitation of the voices of animals, old and young. An ordinary conversation between two Indians, and a counting-out rhyme, are among the records made.

I found the schedules of the United States Bureau of Ethnology of great value in my work, and adopted the method of giving Passamaquoddy and English words consecutively on the cylinders.

The records were all numbered, and the announcement of the subject made on each in English. Some of the stories filled several cylinders, but there was little difficulty in making the changes necessary to pass from one to the other, and the Indians, after some practice, were able to "make good records" in the instrument. Thirty-six cylinders were taken in all. One apiece is sufficient for most of the songs and for many of the short stories. The longest story taken was a folk-tale, which occupies nine cylinders, about "Podump" and "Pook-jin-Squiss," the "Black Cat and the Toad Woman," which has never been published. In a detailed report of my work with the phonograph in preserving the Passamaquoddy language, I hope to give a translation of this interesting story.

Boston, U.S.A., March 20. J. WALTER FEWKES.

Solar Halos and Parhelia.

A MAGNIFICENT display of solar halos and parhelia was witnessed here this afternoon, exceeding in beauty and brilliancy that observed on January 29, 1890, and described in NATURE, February 6, p. 330.

The phenomenon was similar to the one of January 29, except that the mock suns were distinctly outside the first circle or halo, at a distance of 5° or 6°, and were when first seen at 3 p.m. *above* the level of the true sun; a handkerchief stretched at arm's length from one to the other gave the blurred image of the sun several degrees lower.

At 3.49 the patch of white light appeared about 90° from the right mock sun and connected to it with a *curved* band of white light, concave side upwards. The right mock sun must then have been *below* the level of the sun, as the band appeared to pass upwards through it to the sun. This band only remained a few minutes; the right sun and zenith arc at the time were most intensely brilliant, with the colours exceptionally clear and vivid. The zenith arc, and the patch of white light, were the last to disappear at 4.22.

The cirro-stratus cloud during and after the display was rapidly advancing from the north.

Driffield, April 9. J. LOVELL.

Cambridge Anthropometry.

I HAVE read with much interest, in NATURE of March 13 (p. 450), Mr. Venn's very interesting article on anthropometry at Cambridge.

There is in his tables one rather peculiar feature, of which I find no notice taken in the text. It will be seen on reference to the tables that, while the other physical characteristics increase from A to B, and from B to C (weight and height being irregular, however), the *breath* is highest in A, less in B, and least in C; thus falling with the intellectual fall.

It is true that the difference in this as in most of the other characteristics is so slight as to be—as Mr. Venn says—practically negligible; but still the fact that this should steadily *fall* instead of rising with the other physical characteristics strikes me as peculiar. I should be glad therefore to hear if Mr. Venn has any comment to make on this phenomenon, or any explanation thereof to suggest. F. H. P. C.

April 4.

A Remarkable Meteor.

ON Thursday, April 10, at 10.40 p.m., I observed a meteor of extraordinary brilliancy shoot from a point just east of β Leonis. It travelled over about 10° in a north-westerly direction, and was visible for fully two seconds. Its apparent diameter, as nearly as I can judge, was about a quarter of that of the full moon; its colour, a very vivid pale green. J. DUNN.

Much Marcle, Herefordshire, April 11.

Earthworms from Pennsylvania.

NEARLY twenty years ago, a very aberrant earthworm was described by a French naturalist, who obtained it from Pennsylvania. I should be greatly indebted to any naturalists or travellers who may find themselves in that part of the United States, if they would collect some of these worms and send them to me. The most convenient mode of transmission would be to pack the living worms in *moist* earth with moss or grass, in a tin box perforated at one end: this should be inclosed in a wooden box. Both small and large worms should be collected: some might be preserved in strong spirit, but living specimens would be the most useful. W. BLAXLAND BENHAM.

University College, London, April 10.

Crystals of Lime.

SINCE the appearance of my letter on this subject (p. 515) I have found that similar crystals have been recently observed by Mr. J. Joly, and were described by him in the Proceedings of the Royal Dublin Society, vol. vi. p. 255. H. A. MIERS.

SAMPLES OF CURRENT ELECTRICAL LITERATURE.[1]

THESE four books are samples of the different classes of text-books of the present day. The first, as its title implies, is intended for workmen actually engaged in the electrical industries, and is therefore of the non-mathematical technical order. The second, on the other hand, is intended for the practical man who is not afraid of a differential equation, and is a very suitable book for a student of one of the higher technical colleges. The third is a mathematical treatise of the University type; while the fourth is intended for the general public unacquainted with mathematical or scientific principles, but anxious to learn something about this electricity and its distribution, which are now constantly being referred to even in the daily newspapers.

Of the four books, the second, on "Absolute Measurements in Electricity and Magnetism," is the most valuable, because the information it contains is correct, and much of it is not to be found in other books. On opening the first book, "Short Lectures to Electrical Artisans," we anticipated seeing how Dr. Fleming had struck out an entirely new line; but we must confess our disappointment at finding that the author has such a veneration for the authority of antiquity that he felt compelled to commence this book with a description of the loadstone. These lectures, we are told in the preface to the first edition, are on "subjects connected with the principles underlying modern electrical engineering," and were delivered "to the pupils and workmen associated with" Mr. Crompton's firm at Chelmsford. We presume, then, that the lectures were intended to enable workmen to make better dynamo machines, electromotors, &c., but as we never yet met with a piece of loadstone in any electrical factory in England or the Continent, we fail to see how the purpose of the lectures was served by their starting with an account of the "native oxide of iron" called the loadstone. Neither the loadstone nor the classical lump of amber, so dear to the hearts of the writers of electrical text-books, are workshop tools. The latter a workman may perhaps come into contact with as a mouthpiece to his pipe, but a piece of loadstone he will probably never even see out of the lecturer's hand. Apart from this academic start, Lecture I. is decidedly good; the author, for example, not merely mentions that an alloy of steel with 12 per cent. of manganese is nearly non-magnetic, but he gives the name and address of the firm from whom manganese steel can be obtained, and he follows the same wise course when explaining how ferro-prussiate photographic paper may be used for obtaining permanent records of magnetic lines of force.

But why give Rowland's curve connecting permeability and magnetic induction, since later experiments have shown that this curve is quite wrong for large magnetic inductions? The same mistake is made in Lecture III., where it is assumed that for a certain magnetizing force iron becomes saturated, so that no greater induction can be produced, no matter how much the magnetic force is increased.

Lectures II. and III. have many blemishes. The expression 50 ampères of current, on p. 24, is misleading; you cannot have 50 amperes of anything else but current. An ampere is the English name for a unit of current; why, then, put a grave accent over the name? One might as well in speaking of so many metres give this last word its French pronunciation? In justice, however, to Dr. Fleming, we should mention that the use of the grave accent over the word *ampere*, when used in English, is not peculiar to him. We wish, however, that he had been bold enough to Anglicize this word. In describing the construction of a simple mirror galvanometer, the *technical* reader ought to have been warned that, unless, in sticking the three magnets on the back of the mirror with shellac varnish, the shellac be put just at the middle only of each magnet, the mirror will be distorted and rendered useless. To say, when speaking of the induction of a current in a secondary coil by the starting or stopping of a current in the primary, that the interposition of "a plate of iron prevents it altogether," shows that the author has never tried the experiment.

On p. 30 is given a picture of the apparatus the author employs for ascertaining the laws of the production of a current in a coil by the insertion or withdrawal of a magnet. The magnet that is being moved has, judging from the figure, at least 1000 times the mass of the needle of the galvanometer, which is attached by two *very short* wires to the coil in which the current is induced. If an electrical artisan were to perform this experiment with the apparatus placed as in Fig. 17 of Dr. Fleming's book, he would probably ascertain the laws of magneto-electric induction with the same amount of accuracy as we once saw obtained at a lecture where the decisive, and applause-producing, swings of the galvanometer needle, on suddenly bringing up the magnet to the coil and removing it again, were certainly produced by the *direct* action of the magnet on the galvanometer needle, since it was observed at the close of the lecture that one of the wires going from the coil to the galvanometer had never been connected with the galvanometer terminal. And the same sort of criticism applies to Fig. 28, p. 57, representing the arrangement of apparatus for measuring the magnetization of the iron core of an electro-magnet by a current passing round its coil. The reader is told that the magnetometer, which is, of course, to be directly affected by the magnetism of the iron bar, is, for some reason unexplained in the book, to be put at a considerable distance from the bar, but he is not warned that the meter used for measuring the current passing round the electro-magnet (and which, of course, ought not to be directly affected by the magnetism of the bar) must on no account be placed, as in this figure, close to the powerful magnet.

On p. 32 the author says that a core of soft iron "acts like a lens, and concentrates or focusses more lines of force from the magnet on the primary coil through the aperture of the secondary." But this simile with a lens is but a repetition of an old error; a lens simply bends rays of light, and, so far from adding to the total amount of light, actually slightly diminishes this amount by absorption. A lens for light is like a funnel for a fluid, it directs the stream along a narrow channel, so that while the flow is on the whole diminished by friction the flow along a certain cross-section is much increased. But the insertion of an iron core into a coil traversed by a current vastly increases the *total* number of lines of force. The solenoid without the iron core is like a cistern with water in it which is being emptied with a pipe full of dirt, through which the water can only trickle; and the insertion of the iron core into the solenoid is like the cleaning out of the pipe, so that the stream of water now becomes vigorous and rapid. Even Dr. Fleming knocks his own simile on the head, for he states 27 pages further on, "The joint effect of the" (iron) "bar and coils is the sum of the effects of each separately." Fancy anyone saying that the joint effect of a lens and a candle was the sum of the effects of each separately.

We consider it archaic for Dr. Fleming to define the volt for practical men as the E.M.F. generated in one centimetre of wire moving with a velocity of one centimetre per second in a magnetic field of unit force. As well

[1] "Short Lectures to Electrical Artisans." 2nd Edition. By J. A. Fleming. (London: E. and F. N. Spon, 1888.)
"Absolute Measurements in Electricity and Magnetism." 2nd Edition. Revised and greatly Enlarged. By Andrew Gray. (London: Macmillan and Co., 1889.)
"The Theory and Practice of Absolute Measurements in Electricity and Magnetism." By Andrew Gray. (London: Macmillan and Co., 1888.)
"Electricity in Modern Life." By G. W. de Tunzelmann. (London: Walter Scott, 1889.)

might a kilogramme be defined for a French butcher as the weight of a cubic decimetre of distilled water at 4° C., and the butcher's business be absolutely stopped because he did not possess any distilled water and because the temperature of his shop was 20° and not 4° C. In fact, Lectures II. and III., although containing a large amount of valuable information, are professorial rather than practical.

On p. 74 a Ruhmkorff induction coil is correctly described, but in Fig. 36 on the same page the primary coil, with the vibrating interrupter and four cells in its circuit, is shown as consisting of many convolutions of fine wire, and the secondary of a few turns of thick wire. On p. 83 one centimetre is given as equal to 0·0328087 of a foot—that is, correct to *six significant figures*—while even in the second edition, "the call" for which "has afforded the opportunity to erase several typographical errors and to remove some other blemishes which had escaped notice and correction in the first edition," the previous statement is *immediately* followed by the announcement that one inch equals 2·500 centimetres, an equation which is only correct to *two significant figures*, the number expressed correctly to six significant figures being 2·53995. But why not use 2·5400, the value commonly adopted, and which is correct to four places of decimals? As a further example of the want of precision which runs through this book, it may be mentioned that on p. 9 a falling body acquires per second a velocity of 981 centimetres per second. Throughout the whole of p. 85, where the number is frequently mentioned, the body, as if a little tired, cannot get up a velocity of more than 980 centimetres a second. Proceeding, however, to the next page, the body, like the reader, turns over a new leaf, and hurries up its speed, for it acquires per second a velocity of 981 centimetres per second all through this page. Further on, however, in the book, the poor falling body gets tired again, for on p. 97 it cannot do more than the 980. On p. 87 we find the statement, "Hence one foot-pound = 1·356 *joules*, or one joule = ·7373 foot-pound," whereas a simple division shows that if the first part of the statement be correct, the second is not.

To say that "the work is numerically measured by the product of the displacement and the mean stress estimated in the direction of the displacement" is learned and academical, but might not the poor electrical artisan mix this up with the displacement of the factory hands that usually occurs when there is no stress of work?

On p. 99 it is stated that the "E.M.F. of Clark's cell = 1·435 true volt," but, as no indication has been given in this book that there is more than one volt, we are left in ignorance of the reason why the volts used to measure the E.M.F. of a Clark's cell have to be *so especially true*, and why 10^9 C.G.S. units, which is the volt that has been previously used, is not good enough for this sort of measurement. On looking in the index for the definition of the "Ohm British Asssociation," we find ourselves referred to p. 136, and the reader is left to wonder what is a "B.A.U." of resistance used some forty pages previous to this. Similarly the "Legal Ohm" is spoken of and its value given in terms of a "B.A.U." thirty-seven pages before the reader is told what a "Legal Ohm" is. For this the arrangement of the book and not the index is, of course, to blame. And while on this subject we should like to point out that the indexes of scientific books appear to furnish a conclusive proof of the inherent modesty of scientific writers. Take up some large and important treatise, and turn to the index. There you are told that the book contains almost nothing. On the title-page the publisher may have indiscreetly added after the author's name line after line of small print enumerating the various scientific and unscientific societies to which the author belongs, but in the index all pretension to such a wide acquaintance with science is disclaimed. You may have a distinct recollection of reading in this very book many pages on some special subject, but rack your brains as you will to discover under what heading in the index this subject may have been entered, not a reference to it can you find. Accumulators, storage cells, transformers, the volt, voltmeters, &c., seemed likely subjects to be treated on in "Short Lectures to Electrical Artisans," but the index says no; and it is only by carefully reading through the book that you discover that it contains much valuable information on these very points. We would suggest to the writers of scientific treatises, and also to those who communicate scientific papers to learned societies, that the practical man of to-day cannot possibly afford the time to read through ninety-nine things that he does not want to know about, before he can light on the one thing regarding which he is searching for information.

In speaking of Messrs. Crompton and Kapp's meter, on p. 115, Dr. Fleming says:—

"The only difficulty which arises in connection with such an instrument as this, is the tendency of a long thin iron wire of this kind to retain strongly residual magnetism and fail to de-magnetize itself, but this effect would only prevent the return of the indicating needle to zero when the current was stopped, but would not prevent the instrument from giving a definite and fixed deflection corresponding to a definite and fixed current passing through the coils." It was no doubt a somewhat delicate task for Dr. Fleming when lecturing to Mr. Crompton's staff to fully criticize Mr. Crompton's meters, but since actual published experiments on some of these meters show that, for the low readings, the apparent value of a given current differs by as much as 10 per cent., depending on whether the current is ascending or descending, we fail to see how the scientific knowledge of any artisans can be improved by their being told that no such error exists.

Fig. 50, p. 122, showing the level of the columns of water in stand-pipes attached to a horizontal tube through which water is flowing, was never drawn from an actual apparatus. The author has forgotten that the water has not merely to flow through the horizontal tube A*a*, but through the much longer vertical tube CA, and therefore, there is a much greater difference of level between the height of the water in the cistern and in the first stand-pipe, *aa'*, than there is between the level in this stand-pipe and in the next, *bb'*. If Fig. 50 were correct, it would follow that when a battery of even *large internal resistance* was sending a considerable current the difference of potentials at its terminals was equal to the E.M.F. of the battery. Not merely, then, is this opportunity lost of explaining to the readers that the difference of potentials at the terminals of a battery may be very much less than the E.M.F., but the information conveyed by the diagram is actually contrary to fact.

The statement that "Storage cells for lighting purposes cease to give a useful discharge when the electromotive force falls below two volts" is hardly consistent with the fact that, when storage cells are discharged at the current that is considered quite safe by the Electrical Storage Power Company, the E.M.F. for nine-tenths of the period of the discharge is slightly below two volts.

We have said enough to show that, although the book called "Short Lectures to Electrical Artisans" is written by one who, from his University and factory experience, has a large amount of valuable information at his command, the second edition reads far too much like an uncorrected proof of the first edition; and instead of the statements it contains possessing weight because they are made in the book, there is an uneasy feeling when reading its pages that any statement may be wrong, and requires to be checked. We trust, however, that the sale of this, the second edition, may be large and rapid, so that the author may have an opportunity of shortly bringing out as a third edition a book more worthy of his acknowledged power.

"Absolute Measurements in Electricity and Magnetism," by Prof. A. Gray, is a most interesting book to read. It opens with a detailed description of Gauss's methods for determining the horizontal intensity of the earth's magnetism, and with an account of the results of the measurement of the variation, produced by a unit field, on the magnetic moments of steel magnets of different sizes tempered to different degrees of hardness. If it be desired to determine the magnetic moment of a bar-magnet as well as the horizontal intensity of the earth's magnetism, which is of course necessary when variations of the magnetic moment of a bar are in question, Gauss's methods are admirable. But if the value of H is all that is needed, then the simpler method of employing an earth inductor with a ballistic galvanometer, which is described on pp. 317-21, might well be employed. It would, therefore, have been well to give a reference to this method in the first two chapters, which are mainly devoted to the determination of H.

Next follows a concise statement of the various ways of defining the absolute current, and a fairly complete chapter on standard galvanometers. In Chapters IV. and V., and in Chapter XI., to which reference is made, there is given the ablest description of the dimensions of the electric and magnetic units that we have ever read. It is both correct and comprehensible, which is saying a very great deal for an exposition of a subject which, as usually explained, generally leaves even a thoughtful student semi-dazed as to whether the dimensions are the dimensions of the unit, or the dimensions of a quantity measured in the unit. Indeed, the early reports of the Electrical Standards Committee of the British Association were actually wrong on the very subject of dimensions, so that "v" was regularly defined as the ratio of the electrostatic to the electromagnetic unit of quantity instead of as the reciprocal of that expression.

The volt, ohm, ampere, coulomb, watt, and joule are also explained and defined in Chapter V., and Prof. Gray gives Sir W. Thomson's expression "activity" for the rate of doing work. He does not mention, however, that the equally short word "power" is regularly employed with this signification.

Chapter VI. is devoted to the laws of the currents sent by galvanic cells through single and parallel circuits, and through any branch of a network like that of the Wheatstone's bridge. A neat proof is given of the arrangement of a given number of cells that sends the greatest current through a fixed resistance, and the reader is very properly warned against confusing the arrangement which develops maximum power with the most economical arrangement.

In Chapter VII. we have a complete description of Sir William Thomson's meters, but, as the book is a scientific treatise (in fact, a very good scientific treatise) and not an instrument-maker's catalogue, we think that the author would have done himself more justice had he described, in addition, some of the other many forms of electric meters in common use at the present day for carrying out the same measurements. Further, in view of the large experience that the author of this book has probably had with Sir W. Thomson's meters, it would have been well had there been a description not merely of the advantages of these instruments, but also of their disadvantages, a subject no one would be more willing to discuss than the inventor himself. On pp. 133-35 is given a very simple proof of the ordinary formula for the quadrant electrometer, but the reader is not here warned that the formula may give an answer many per cent. wrong in practice. On p. 302 it is stated that this formula may be slightly wrong if the aluminium needle of the electrometer be not accurately adjusted relatively to the quadrants, but this, we fear, is rather misleading, since it is further stated that "if the needle hangs at its proper level, and is otherwise properly adjusted, and the quadrants are close, the equation may be taken as accurate enough for practical purposes," a conclusion regarding which we understand there is grave doubt. In this chapter the very important subject of calibrating instruments by the use of the silver or the copper voltameters is fully entered into. The large amount of valuable work done on this subject by the author's brother, Prof. T. Gray, of which a description is given, endows this chapter with an authoritative character.

Chapter VIII. commences with the construction and use of the various forms of Wheatstone's bridges, the description of the modes of using them, and hints as to the care of a resistance box. The methods for calibrating relatively and absolutely the wire of a bridge devised by Matthiessen and Hockin, Foster, T. Gray, and D. M. Lewis are discussed at length, and specimens given of the actual results obtained at University College, North Wales, by the use of these methods. The ingenious bridges, which have been arranged by Sir W. Thomson, Matthiessen and Hockin, Tait and T. Gray, for measuring very low resistances, are fully entered into, and the construction of standard coils, the measurement of high resistances, and of the resistance of a battery finish a chapter of especial interest. The method of measuring the resistance of a battery, proposed several years ago by Sir Henry Mance, is condemned by Prof. Gray as being "so troublesome as to be practically useless," on account of "the variation of the effective electromotive force of the cell produced by alteration of the current through the cell which takes place when the key is depressed." We think that it should have been stated that this is not a defect especially of Mance's method, but of *all* methods for measuring the resistance of a battery based on the alteration of a steady current by the alteration of the resistance in the battery circuit. Would it not also here have been well to describe and discuss the condenser method of measuring a battery resistance, as it is the one to which the fewest objections can be raised?

Good as are all the chapters in this book, the next one, Chapter IX., on "The Measurement of Energy in Electric Circuits," is so good that it takes the palm. It commences with the practical methods of measuring the power and efficiency of motors and secondary batteries; the construction and employment of activity meters (watt-meters); and then discusses very fully the laws of alternate currents, the mathematical theory of alternate current generators singly, or coupled in parallel or in series; the theory of the action of an alternate current generator supplying current to an alternate current motor; the true method of measuring the power given to any circuit by an alternate current; and the error produced when an ordinary watt-meter is employed. The work of Joubert, Hopkinson, Potier, Ayrton and Perry, and Mordey on this subject is summed up in a masterly fashion. Chapter IX. is, in fact, the most complete exposition of many problems connected with the all-important subject—the electrical transmission of energy by *alternate* currents—that is to be found in any existing text-book, and especially in a small octavo text-book, that can be easily carried in one's coat pocket.

In Chapter X. the measurement of intense magnetic fields is dealt with, and a description is given of ingenious methods proposed by Sir W. Thomson for measuring the force on a conductor conveying a known current placed in the magnetic field, and so determining the strength of the field. The ordinary method of ascertaining the strength of a magnetic field by suddenly withdrawing a coil, of known area and number of convolutions, attached to a ballistic galvanometer, is described. But in order to ascertain the constant of the ballistic galvanometer, the author only gives the old method of observing the swing of the needle when a large coil is turned in the earth's field, a method which necessarily requires for its employment a previous knowledge of the strength of the earth's field at the place. A far simpler method of ascertaining the constant of a ballistic galvanometer is to charge a

condenser of known capacity with one or more Clark's cells, of which the E.M.F. at any ordinary temperature is now well known, and discharge the condenser through the ballistic galvanometer; or, if a sufficiently delicate ampere-meter be available, the ballistic galvanometer may be very accurately calibrated for steady currents, and then its constant for a sudden discharge is at once known by simply measuring, in addition, the periodic time of vibration of the needle and its logarithmic decrement.

The book concludes with an appendix giving the decisions arrived at in 1886 by the Electrical Standards Committee of the British Association, and the further resolutions which were passed at the meeting of the Electrical Congress in Paris last year, and subsequently agreed to by the British Association Committee. Then follow twelve sets of useful tables.

Although we have made a few suggestions that the author may perhaps like to adopt in publishing the third edition of his "Absolute Measurements in Electricity and Magnetism," we desire to emphasize our warm appreciation of this the second edition. On every page may be seen evidences of the firm grip of the subject so characteristic of the author's teacher—the teacher, in fact, of us all—Sir William Thomson; and did we know of higher praise than this we would give it.

"The Theory and Practice of Absolute Measurements in Electricity and Magnetism, Vol. I.," also by Prof. A. Gray, is a mathematical expansion of the *electrical* portion of his book on "Absolute Measurements, &c.," the mathematical treatment of the *magnetic* portion being reserved for Vol. II. of the larger work. As many of the remarks that we have already made regarding the smaller work apply equally well to the larger, it is unnecessary to criticize the larger book at any considerable length. The two books may be read quite independently of one another, since much of the descriptive matter is the same in both. If there be a fault in the larger work, we think that it arises from the author forgetting that a book intended initially for the University student can also be made of great value to the more practical electrician if first the subject-matter be arranged in propositions, or with distinct headings to the paragraphs, so that it is easy to find the proof of any particular fact; and, secondly, if complete proofs be given of important practical problems, instead of simply deducing them as special cases of more general problems. For example, a practical electrician may desire to see how the logarithmic formula for the capacity of a cable is arrived at. Now, there is no difficulty in giving a fairly short complete proof of this; but, on turning to Prof. Gray's "Theory and Practice, &c.," the electrician finds that he must first master the theory of charged ellipsoids; he sees several double integrals and several lines of long mathematical formula in small print, and he probably decides that he had better pass by that subject for the present. We hold that, since the pure science of electricity owes so much to its practical development, it is but fair that the pure mathematician should endeavour to repay this debt by stating his results and methods of proof in such a form that they can be most easily grasped by anyone who desires to use them, and not merely to get up the subject for examination purposes. The general mathematical investigations are also, of course, of great value, and we are therefore glad to see in this book a fairly complete mathematical treatment of Green's theorem, inverse problems, electric images, problems of steady flow in non-linear conductor, and variable linear flow, with its application to the speed of signalling in submarine conductors.

Very interesting information is given regarding the strength and torsional rigidity of the fine silk fibres used in suspending galvanometer needles, followed by the mathematical theory of oscillations, the description of the practical methods of measuring periodic times of oscillation and moments of inertia, and concluding with a comparison of unifilar and bifilar suspensions. The succeeding chapters on electrometers, the general measurement of resistance, the calibration of the wire of a metre bridge, the measurement of very low resistances, the measurement of very high resistances, the determination of specific resistance, contain what is given on these subjects in the smaller book amplified.

The last chapter, No. VIII., in this larger treatise, on capacity, is very complete. It gives a description of the most important investigations that have been made on the specific inductive capacity of solids, liquids, and gases, together with the mathematical theory of each experiment.

Although we cannot but feel that the smaller of the two books published by Prof. A. Gray is the more unique, the larger is a very creditable production, and will be valuable as a book of reference for those who desire to consult a shorter book on mathematical electricity than that of Messrs. Mascart and Joubert.

We now come now to the fourth book, "Electricity in Modern Life," by Mr. de Tunzelmann, which is written on an excellent basis, and contains a great deal of useful popular information, but it unfortunately also contains many unnecessary errors. For example, the statement on p. 11, that "a single cell of this kind," potash bichromate, "holding about a quart of solution, is capable of maintaining the light of a small incandescent lamp for some three or four hours," would rather disappoint a purchaser of a quart, or any size, bichromate cell, as he would find it most difficult to purchase an incandescent lamp that would glow with so small a difference of potential as *one* cell could produce. Again, to say in Chapter II., on "What we Know about Magnetism," "Weber's theory of magnetism may now be considered as raised from the rank of an hypothesis to that of an established fact," gives a totally wrong idea as regards our knowledge, or, rather, as regards our ignorance, of the mechanism of magnetism. "The face of the magnet that before pointed to the north," &c., is not exactly wrong; but can a face point towards anything? "If a current goes round the solenoid in the direction of the hands of a watch with its face directed towards the end from which the current flows, the end of the steel bar within the end of the solenoid at which the current leaves will be found to be a north pole and the other end a south pole," would lead the reader to imagine that the polarity of the core of an electromagnet depended partly on the direction in which the current flows *parallel* to the core, instead of depending, as is the fact, wholly on the way it flows *round* the core.

Chapter IV., on "Force, Work, and Power," is good, and the careful distinction drawn between work and power is forcible and apt. But why does the author limit the definition of a horse-power, 33,000 pounds raised 1 foot per minute, to the "indicated horse-power" of a steam-engine.

Chapter V. deals with the "Sources of Electricity." In describing the chemical action of a galvanic cell formed "of a plate of zinc and a plate of copper partly immersed in sulphuric acid," it is an obvious mistake to speak of the action as a simple liberation of hydrogen at the copper plate, and oxygen at the zinc, ard to omit all reference to the formation of zinc sulphate. The first part of the following statement has been experimentally disproved some fifteen years ago:—"If either the copper or zinc is immersed alone in dilute sulphuric acid, a difference of potential will be produced between the metal and the liquid; but if the two metals are immersed side by side into the liquid, then no electrification can be detected." A galvanic battery is defined by the author as "a series of galvanic cells so arranged that the zinc of

each cell is connected with the copper of the next cell." What, then, is a collection of galvanic cells arranged in parallel, in which the zinc of every cell is connected with the zinc and not with the copper of the next? Excluding these mistakes, this chapter is fairly good; the matter, however, is rather too condensed to be intelligible to a reader not previously acquainted with the subject.

Chapter VI. deals with "Magnetic Fields," and in order to lead up to the mapping out of a magnetic field, the mapping out of the gravitation field of force in which a comet moves is first explained. But it appears to us that, since the magnetic field can be easily mapped out with iron filings in the well-known way, while the conception of a gravitation field of force is a less simple matter to grasp, Mr. de Tunzelmann has in this case explained the easy by means of the difficult.

The next chapter, on "Electrical Measurement," is quite correct, but, in view of the great difficulty that is always experienced by a beginner in grasping the idea of measuring so intangible a thing as electricity, would not this subject have been made clearer if not merely the scientific definitions of the electrical units had been given, but in addition an illustrated description of the meters used to measure amperes, volts, &c.?

Chapter VII., on "Magneto and Dynamo Electric Machines," gives a short comprehensive description of the principles of these machines, but, in order that the reader might understand what a real dynamo was like, we think it would have been better if the author had given in this chapter at least some one of the illustrations representing real dynamos which appear in other parts of this book. The symbolical figures that are given are, as the author mentions, taken from Dr. Thompson's book on dynamo machinery, and are very clear, with one exception, that while in each case the direction of the current in the wires attached to the brushes is indicated by arrows, the direction in which the wire is coiled on the armature is omitted, hence such statements as "the arrows show the current in the circuit when the armature revolves as indicated by the position of the brushes," are just as likely to be wrong as right, and tell the reader nothing. When comparing the series dynamo with the shunt dynamo, the author says that the former "will not begin to excite itself until a certain speed has been obtained depending on the resistance of the circuit." From this the reader might easily be misled into thinking that the shunt machine did not possess a similar defect. Further, he states, as "the principal objection to shunt-wound machines," that the self-induction of the field-magnet coils leads to the result that "any variation in the speed produces its effect upon the lamps before the current in the existing circuit has had time to undergo a sensible change." But, as a matter of fact, the self-induction of the field-magnet coils of a shunt machine is an *advantage*, not a *disadvantage*; for suppose that the speed increases, then the E.M.F. increases, this causes the difference of potentials between the lamp-mains to increase, which not only sends a larger current through the lamps, but also through the shunt coils. This strengthening of the field causes an additional rise in the E.M.F. of the machine, and therefore in the terminal difference of potentials. Consequently the second objectionable rise is hindered, and not accelerated, by the self-induction of the shunt coils; hence self-induction of the field-magnet coils of a shunt machine makes the difference of potentials between the lamp-mains less quickly, and not more quickly, affected by a change in the speed of driving. In speaking of alternate-current dynamos, it is stated that "in some machines the armature remains at rest, and the field-magnets are made to rotate; and in this case no sliding contact is required, the terminals of the main circuit being attached permanently to the armature." But the statement is misleading, since at least one sliding contact must *always* be used; only when the armature is fixed it is to lead the exciting current into and out of the rotating field-magnets that one, and in some cases two sliding contacts are employed.

Chapters IX., X., and XI., on "The Story of the Telegraph," "Overland Telegraphs," and on "Submarine Telegraphs," are excellent, we may almost say exciting, and they lead the reader on like the pages of a well-written novel. It is not right, however, on p. 112 to say, when speaking of telegraphing with sounders, "The dots are formed by giving a sharp stroke to the key; the dashes by depressing it more slowly," since a dash is formed not by depressing the key slowly, but by holding it down for a time when depressed. Whether a key be depressed slowly or quickly makes no difference in the signal received; what the receiver listens for is the interval between the commencement of the current produced when the key is fully depressed and its termination when the key is caused to begin to rise again. We presume that when the author says, on p. 129, "The cups" of insulators "are made of such a form as to expose the upper portions freely to the cleansing action of the rain while the lower portions are shielded from the rain so as to keep them fairly dry," he means by "upper portions" the *outside* of the cup of the insulator, and by the "lower portions" the *inside*; but if so, he has a curious way of expressing himself. The "speaking galvanometer" used in receiving the message sent through a submarine cable is not, as the author describes it on p. 150, an astatic galvanometer; and even if two magnets were employed so as to form an astatic combination, it would be quite wrong to say "each of them is attached to the back of a small mirror," since, unnecessary as it would be to use two suspended magnets in a speaking galvanometer, it would be still more useless to employ two suspended mirrors. But these are not very serious errors in chapters that are so good.

Chapters XII. and XIII., on "The Telephone" and "The Telephone Exchange System," appear to us to be too much of the newspaper special correspondent order, the descriptions in several cases being very meagre, suggestive rather than descriptive, in consequence of the author having attempted to touch on too many different things. For instance, if the photophone had to be described at all, it required more than one page and a quarter, inclusive of the illustration, to make it intelligible; in fact, unless the framework of the telephones and the gentleman's head which is between them in Fig. 53 are all composed of electrically conducting material, we fail to see how the instrument, as there depicted, works at all. Some very interesting information is given on the subject of telephone exchanges, and we should have liked to have had much more information on this electrical subject; for example, greater details regarding the switches, the reasons of the babble of many conversations that everyone hears who tries to use the telephone in London, &c.; space, if necessary, being economized by the omission of the description of the non-electrical instruments, the graphophone and phonograph.

Chapter XIV., on the "Distribution and Storage of Electrical Energy," is very good and forcible. We fail, however, to see how the use of the three-wire system leads to the result stated on p. 199, that "a variation of 5 per cent. in the E.M.F. in the mains would produce a variation of only 2½ per cent. at the lamp terminals."

The next chapter, XV., on "Electric Lighting," is also very good; "flashing" the filament of an incandescent lamp, however, does not mean sending a current through the filament while the lamp is attached to the Sprengel pump, but sending a current through the filament and making the filament incandescent when in a hydrocarbon atmosphere before it is placed inside the glass bulb of the lamp. Is it a fact that "the Shaftesbury theatre" is "now lighted by incandescent electric lamps?"

The chapter on "Electro-Motors and their Uses" is good considering how much may be said on this subject and how short a space is 14 pages to say it in. By what means, however, Messrs. Immisch have succeeded in making the dogcart for the Sultan of Turkey go "ten miles an hour for about five hours" by means of "twenty-four small accumulators which weigh about seven hundredweight" we are at a loss to conceive, since the weight of accumulators, according to our calculation, must be much greater than this in order that they may have anything like a reasonably long life.

Chapter XVII., on "Electro-Metallurgy," is interesting although very brief, but the descriptions of the electrical circuit-closers for torpedoes in the next chapter, on "Electricity in Warfare," we find too short to be intelligible. A chapter of 5 pages then follows on "Medical Electricity," and another chapter of the same length on "Miscellaneous Applications of Electricity," in which a very interesting account is given of the electrical method employed in America for protecting furnished dwelling-houses that have been left locked up during the absence of the tenants.

On closing this book one certainly cannot deny that one has had one's money's worth, even if the entertainment has been of the "variety order" so characteristic of the amusements of the present day. If a member of the general public will read the book right through, as we have done, he may perhaps feel with exultation that he has mastered the whole subject of electrical engineering; indeed, even a well-trained electrician can learn from it many things that he did not know before, concerning those branches of the subject to which he has not given special attention. But we fear that, if even a general reader were to turn up any particular subject to study in detail, he would probably wish he had been told a good deal more about what was most important, and not so much about everything electrical whether important or not. The best features of "Electricity in Modern Life" are the many interesting scientific narratives, in the writing of which Mr. de Tunzelmann appears to excel; the worst are the mistakes in the science, which more knowledge, or more care, ought to have eliminated.

ON THE TENSION OF RECENTLY FORMED LIQUID SURFACES.[1]

It has long been a mystery why a few liquids, such as solutions of soap and saponine, should stand so far in advance of others in regard to their capability of extension into large and tolerably durable laminæ. The subject was specially considered by Plateau in his valuable researches, but with results which cannot be regarded as wholly satisfactory. In his view the question is one of the ratio between capillary tension and superficial viscosity. Some of the facts adduced certainly favour a connection between the phenomena attributed to the latter property and capability of extension; but the "superficial viscosity" is not clearly defined, and itself stands in need of explanation.

It appears to me that there is much to be said in favour of the suggestion of Marangoni ("Nuovo Cimento," vols. v.–vi., 1871, p. 239), to the effect that both capability of extension and so-called superficial viscosity are due to the presence upon the body of the liquid of a coating or pellicle composed of matter whose inherent capillary force is less than that of the mass. By means of variations in this coating, Marangoni explains the indisputable fact that in vertical soap films the effective tension is different at various levels. Were the tension rigorously constant, as it is sometimes inadvertently stated to be, gravity would inevitably assert itself, and the central parts would fall 16 feet in the first second of time. By a self-acting adjustment the coating will everywhere assume such thickness as to afford the necessary tension, and thus any part of the film, considered without distinction of its various layers, is in equilibrium. There is nothing, however, to prevent the interior layers of a moderately thick film from draining down. But this motion, taking place as it were between two fixed walls, is comparatively slow, being much impeded by ordinary fluid viscosity.

In the case of soap, the formation of the pellicle is attributed by Marangoni to the action of atmospheric carbonic acid, liberating the fatty acid from its combination with alkali. On the other hand, Sondhauss (*Poggendorff's Annalen*, Ergänzungsband viii., 1878, p. 266) found that the properties of the liquid, and the films themselves, are better conserved when the atmosphere is excluded by hydrogen; and I have myself observed a rapid deterioration of very dilute solutions of oleate of soda when exposed to the air. In this case a remedy may be found in the addition of caustic potash. It is to be observed, moreover, that, as has long been known, the capillary forces are themselves quite capable of overcoming weak chemical affinities, and will operate in the direction required.

A strong argument in favour of Marangoni's theory is afforded by his observation,[1] that within very wide limits the superficial tension of soap solutions, as determined by capillary tubes, is almost independent of the strength. My purpose in this note is to put forward some new facts tending strongly to the same conclusion.

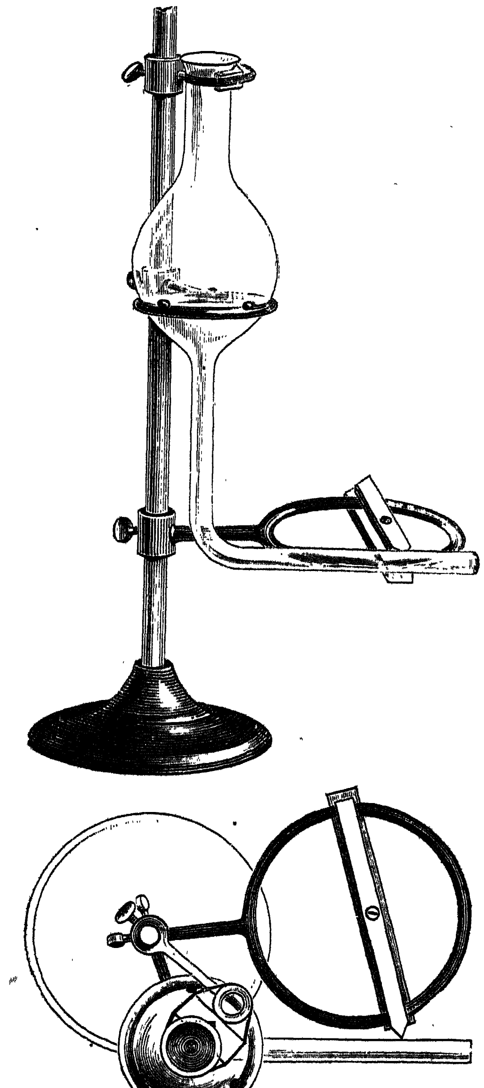
It occurred to me that, if the low tension of soap solutions as compared with pure water was due to a coating, the formation of this coating would be a matter of time, and that a test might be found in the examination of the properties of the liquid surface immediately after its formation. The experimental problem here suggested may seem difficult or impossible; but it was, in fact, solved some years ago in the course of researches upon the capillary phenomena of jets (Roy. Soc. Proc., May 15, 1879). A jet of liquid issuing under moderate pressure from an elongated, *e.g.* elliptical, aperture perforated in a thin plate, assumes a chain-like appearance, the complete period, λ, corresponding to two links of the chain, being the distance travelled over by a given part of the liquid in the time occupied by a complete transverse vibration of the column about its cylindrical configuration of equilibrium. Since the phase of vibration depends upon the time elapsed, it is always the same at the same point in space, and thus the motion is *steady* in the hydrodynamical sense, and the boundary of the jet is a fixed surface. Measurements of λ under a given head, or velocity, determine the time of vibration, and from this, when the density of the liquid and the diameter of the column are known, follows in its turn the value of the capillary tension (T) to which the vibrations are due. *Cæteris paribus*, $T \propto \lambda^{-2}$; and this relation, which is very easily proved, is all that is needed for our purpose. If we wish to see whether a moderate addition of soap alters the capillary tension of water, we have only to compare the wave-lengths λ in the two cases, using the same aperture and head. By this method the liquid surface may be tested before it is $\frac{1}{100}$ second old.

Since it was necessary to be able to work with moderate quantities of liquid, the elliptical aperture had to be rather fine, about 2 mm. by 1 mm. The reservoir was an ordinary flask, 8 cm. in diameter, to which was sealed below as a prolongation a (1 cm.) tube bent at right angles (Figs. 1, 2). The aperture was perforated in thin sheet brass, attached to the tube by cement. It was about 15 cm. below the mark, near the middle of the flask, which defined the position of the free surface at the time of observation.

[1] A Paper read by Lord Rayleigh, Sec. R.S., before the Royal Society, on March 6.

[1] *Poggendorff's Annalen*, vol. cxliii., 1871, p. 342. The original pamphlet dates from 1865.

The arrangement for bringing the apparatus to a fixed position, designed upon the principles laid down by Sir W. Thomson, was simple and effective. The body of the flask rested ont hree protuberances from the ring of a retort stand, while the neck was held by an india-rubber band into a V-groove attached to an upper ring. This provided five contacts. The necessary sixth contact was effected by rotating the apparatus about its vertical axis until the delivery tube bore against a stop situated] near its free end. The flask could thus be removed for cleaning without interfering with the comparability of various experiments.



FIGS. 1 and 2.

The measurements, which usually embrace two complete periods, could be taken pretty accurately by a pair of compasses with the assistance of a magnifying glass. But the double period was somewhat small (16 mm.), and the little latitude admissible in respect to the time of observation was rather embarrassing. It was thus a great improvement to take magnified photographs of the jet, upon which measurements could afterwards be made at leisure. In some preliminary experiments the image upon the ground glass of the camera was utilized without actual photography. Even thus a decided advantage was realized in comparison with the direct measurements.

Sufficient illumination was afforded by a candle flame situated a few inches behind the jet. This was diffused by the interposition of a piece of ground glass. The lens was a rapid portrait lens of large aperture, and the ten seconds needed to produce a suitable impression upon the gelatine plate was not so long as to entail any important change in the condition of the jet. Otherwise, it would have been easy to reduce the exposure by the introduction of a condenser. In all cases the sharpness of the resulting photographs is evidence that the sixth contact was properly made, and thus that the scale of magnification was strictly preserved. Fig. 3 is a reproduction on the original scale of a photograph of a water-jet taken upon November 9. The distance recorded as 2λ is between the points marked A and B, and was of course measured upon the original negative. On each occasion when various liquids were under investigation, the photography of the water-jet was repeated, and the results agreed well.

After these explanations it will suffice to summarise the actual measurements upon oleate of soda in tabular form. The standard solution contained 1 part of oleate in 40 parts of water, and was diluted as occasion required.[1] All lengths are given in millimetres.

	Water.	Oleate 1/40.	Oleate 1/80.	Oleate 1/400.	Oleate 1/4000.
2λ...	40·0	45·5	44·0	39·0	39·0
h...	31·5	11·0	11·0	11·0	23·0

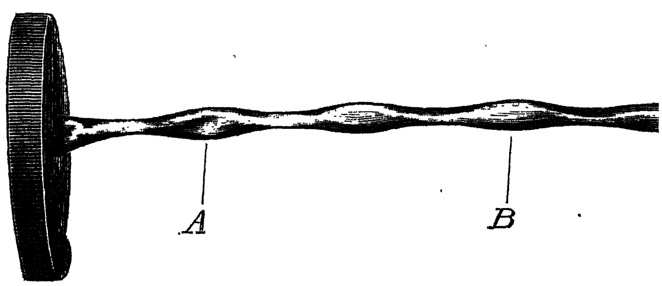


FIG. 3.

In the second row h is the rise of the liquid in a capillary tube, carefully cleaned before each trial with strong sulphuric acid and copious washing. In the last case, relating to oleate solution $\frac{1}{4000}$, the motion was sluggish and the capillary height but ill-defined. It will be seen that even when the capillary height is not much more than one-third of that of water, the wave-lengths differ but little, indicating that, at any rate, the greater part of the lowering of tension due to oleate requires time for its development. According to the law given above, the ratio of tensions of the newly-formed surfaces for water and oleate ($\frac{1}{80}$) would be merely as 6 : 5.[2]

Whether the slight differences still apparent in the case of the stronger solutions are due to the formation of a sensible coating in less than $\frac{1}{100}$ second, cannot be absolutely decided; but the probability appears to lie in the negative. No distinct differences could be detected between the first and second wave-lengths; but this observation is, perhaps, not accurate enough to settle the question. It is possible that a coating may be formed on the surface of the glass and metal, and that this is afterwards carried forward.

[1] Although I can find no note of the fact, I think I am right in saying that large bubbles could be blown with the weakest of the solutions experimented upon.

[2] Curiously enough, I find it already recorded in my note-book of 1879, that λ is not influenced by the addition to water of soap sufficient to render impossible the rebound of colliding jets.

As a check upon the method, I thought it desirable to apply it to the comparison of pure water and dilute alcohol, choosing for the latter a mixture of 10 parts by volume of strong (not methylated) alcohol with 90 parts water. The results were as follows:—

$$2\lambda \text{ (water)} = 38\cdot5, \qquad 2\lambda \text{ (alcohol)} = 46\cdot5,$$
$$h \text{ (water)} = 30\cdot0, \qquad h \text{ (alcohol)} = 22\cdot0;$$

but it may be observed that they are not quite comparable with the preceding for various reasons, such as displacements of apparatus and changes of temperature. It is scarcely worth while to attempt an elaborate reduction of these numbers, taking into account the differences of specific gravity in the two cases; for, as was shown in the former paper, the observed values of λ are complicated by the departure of the vibrations from isochronism, when, as in the present experiments, the deviation from the circular section is moderately great. We have—

$$(46\cdot5/38\cdot5)^2 = 1\cdot46, \qquad 30/22 = 1\cdot36;$$

and these numbers prove, at any rate, that the method of wave-lengths is fully competent to show a change in tension, provided that the change really occurs at the first moment of the formation of the free surface.

In view of the great extensibility of saponine films it seemed important to make experiments upon this material also. The liquid employed was an infusion of horse chestnuts of specific gravity 1·02, and, doubtless, contained other ingredients as well as saponine. It was capable of giving large bubbles, even when considerably diluted (6 times) with water. Photographs taken on November 23 gave the following results:—

$$2\lambda \text{ (water)} = 39\cdot2, \qquad 2\lambda \text{ (saponine)} = 39\cdot5,$$
$$h \text{ (water)} = 30\cdot5, \qquad h \text{ (saponine)} = 20\cdot7.$$

Thus, although the capillary heights differ considerably, the tensions at the first moment are almost equal. In this case then, as in that of soap, there is strong evidence that the lowered tension is the result of the formation of a pellicle.

Though not immediately connected with the principal subject of this communication, it may be well here to record that I find saponine to have no effect inimical to the rebound after mutual collision of jets containing it. The same may be said of gelatine, whose solutions froth strongly. On the other hand, a very little soap or oleate usually renders such rebound impossible, but this effect appears to depend upon *undissolved* greasy matter. At least the drops from a nearly vertical fountain of *clear* solution of soap were found not to scatter (Roy. Soc. Proc., June 15, 1882). The rebound of *jets* is, however, a far more delicate test than that of *drops*. A fountain of strong saponine differs in appearance from one of water; but this effect is due rather to the superficial viscosity, which retards, or altogether prevents, the resolution into drops.

The failure of rebound when jets or drops containing milk or undissolved soap come into collision has not been fully explained; but it is probably connected with the disturbance which must arise when a particle of grease from the interior reaches the surface of one of the liquid masses.

P.S.—I have lately found that the high tension of recently formed surfaces of soapy water was deduced by A. Dupré ("Théorie Mécanique de la Chaleur," Paris, 1869), as long ago as 1869, from some experiments upon the vertical rise of fine jets. Although this method is less direct than that of the present paper, M. Dupré must be considered, I think, to have made out his case. It is remarkable that so interesting an observation should not have attracted more attention.

NOTES.

It is stated that the committee to be appointed to inquire into colour-blindness in seamen, railway guards, and others, will not be exclusively confined to members of the Royal Society. Some gentlemen who, like Dr. Farquharson, M.P., and Mr. Bickerton, of Liverpool, have taken special interest in the question will, it is said, be asked to join the committee. A further question on the subject will, in the course of a few days, be put to the President of the Board of Trade.

We regret to have to record the death of Sir John Henry Lefroy, F.R.S. He died on Friday evening last at his residence, Lewarne, a few miles from Liskeard. He was seventy-three years of age. He entered the Royal Artillery in 1834, and was Director of the Magnetical and Meteorological Observatory at St. Helena from 1840 to 1841, whence he moved to a similar position at Toronto in 1842. During the next year he made a magnetic survey of the interior of North America from Montreal to the Arctic Circle. From 1854 to 1855 he was scientific adviser to the Duke of Newcastle at the War Office on subjects of artillery and inventions, and in 1855 he was sent, as lieutenant-colonel, on a special mission to the seat of war. Afterwards he held several high military appointments. In 1882 he was made a general, and retired. He had been elected a Fellow of the Royal Society in 1848.

Mr. Thomas Johnson, Demonstrator in Botany at the Normal School of Science and Royal School of Mines, has been appointed to succeed the late Prof. McNab, as Professor of Botany at the Royal College of Science, Dublin. Prof. Johnson begins lecturing this term.

An International Medical Congress was opened at Vienna on Tuesday, and will continue its sittings until to-morrow (Friday). Many physicians from the principal Europe an countries are taking part in the proceedings.

At the next meeting of the Anthropological Institute, on Tuesday, April 22, M. Jacques Bertillon will give a lecture, with demonstrations, on the method now practised in France of identifying criminals by comparing their measures with those of convicted persons in the prison registers. The registers contain the measures of many tens of thousands of persons, with their photographs; yet M. Bertillon's method enables the reference to be rapidly effected. It is thought, therefore, that the authorities in England who are concerned with the police, or with the identification of deserters from the army or the navy, may be glad of the opportunity of hearing M. Bertillon's exposition.

The Meteorological Office has adopted a new way of spreading information as to the condition of the weather on our coasts. On Monday it began to exhibit, at 63 Victoria Street, Westminster, outside the building, a series of boards, showing the state of the wind, weather, and sea at Yarmouth, Dover, the Needles, Scilly, Valentia (Ireland), and Holyhead. The information given is for 8 o'clock in the morning and 2 o'clock in the afternoon, and the notices are posted up at about 9.30 a.m. and 3 p.m. respectively. The words are printed in clear type, and can be read by those having ordinarily good sight from the pavement or roadway.

At the meeting of the Institution of Civil Engineers on Tuesday evening, Sir Frederick Bramwell read a paper on the application of electricity to welding, stamping, and other cognate purposes.

There has been some talk lately about a scheme for the construction of a bridge across the Bosphorus. The Turkish newspaper *Hakikat* gives some particulars of the project *à propos* of

an offer by a French syndicate to build a bridge of 800 metres in length and 70 metres high between Roumeli and Anatoli Hissar. The bridge would consist of one span, and this would exceed in length by one-half the longest span of the Forth Bridge. The Anatolian railway, it is thought, will make the construction of such a bridge a necessary and feasible undertaking before many years.

MADAME ROSA KIRSCHBAUM, who has taken the degree of Doctor of Medicine at a Swiss University, has been authorized by a special imperial decree to conduct a hospital for eye diseases at Salzburg. The Vienna Correspondent of the *Times* says this is the first case of a lady physician being admitted to medical practice in Austria.

THE new number of the *Kew Bulletin* begins with a section on canaigre, the root of which seems likely to take an important place as a tanning material. This is followed by sections on pistachio cultivation in Cyprus, Indian sugar, and mites on sugar-cane. The section on Indian sugar consists chiefly of a selection from a file of documents sent to Kew from the India Office, containing much valuable information as to the production of cane sugar in India.

AT the meeting of the Scientific Committee of the Royal Horticultural Society on April 8, Mr. Wilson exhibited a plant of a primrose, a seedling from Scott Wilson, showing a greater advance to a deep blue colour thanh as yet been made. A series of intermediate forms were also shown.

THE Prefect of Savoy has recently prohibited the gathering of the *Cyclamen* in the woods of his department. Notwithstanding its abundance in the locality, this beautiful plant had been threatened with total extinction, from the enormous numbers gathered each year for sale in the markets of Chambéry and Aix-les-Bains.

A SINGULAR fact is related by M. Lagatu in the *Feuille des Jeunes Naturalistes*. In the year 1884 a large number of cattle died after having browsed in a particular pasture in the department of l'Oise. M. E. Prillieux found the cause of death to be poisoning by ergotized *Lolium*; and he attributes it to the fact that the cattle were sent to the pasture about 10 days later than usual. M. Prillieux frequently found ergot on tufts of grass refused by the cattle, which marked the spots where dejecta had been left without being scattered.

DR. G. B. DE TONI has retired from the editorship of the Italian bi-monthly journal *Notarisia*, devoted to cryptogamic botany, which will in future be conducted by Dr. David Levi Morenos.

AT the last meeting of the Natural History Society of Kiel, Major Reinhold read a paper on the botanical condition of the German Ocean. According to researches recently made, the eastern part is almost wholly bare of vegetation. This is believed to be owing to the strong tidal currents, which so disturb the sea bottom as to prevent the germs and spores of marine plants from settling.

A ZOOLOGICAL floating station is now in working order at Isefiord on the Danish coast, under the direction of Dr. Petersen.

THE Proceedings of the International Congress of Zoology, held last August in Paris, were issued a few days ago. Among the contributors are Messrs. Bogdanow, Bowdler Sharpe, D'Arcy Thompson, E. P. Wright, C. V. Riley, V. Wagner, Ray Lankester, A. S. Packard, Trimen, Rütimeyer, Retzius, Hubrecht, de Selys-Longchamps, Agassiz, Blanford, L. Netto, W. A. Conklin, A. Fritsch, and McLachlan. This list of names suffices to show that the meeting was really of an international character.

A SHOCK of earthquake was felt in M U.S.A., on April 11.

REPORTS of an earthquake felt on March 26, between 9.15 and 9.20 p.m., have been received from Innsbruck, the Ziller Valley, Sterzing, Bozen, Meran, the Puster Valley, Salurn, Arco, Ampezzo, and the Weiten Valley. The direction of the shocks was from north to south.

TWO papers on "The Cradle of the Semites," read before the Philadelphia Oriental Club, have just been published. The first is by Dr. Daniel G. Brinton, who contends that the Semitic stock came originally from "those picturesque valleys of the Atlas which look forth toward the Great Ocean and the setting sun." Prof. Jastrow, the author of the second paper, agrees generally as to the probability of a Semitic migration from Africa into Asia, but thinks that Dr. Brinton goes farther than the evidence warrants when he tries to indicate the particular region of Africa from which the migration started.

DURING the summer and autumn of 1888, and the following winter, Mr. Albert Koebele carried on researches in Australia for the purpose of determining whether it would not be possible to introduce into California the most efficient of the Australian natural enemies of the fluted scale (*Icerya purchasi*, Maskell). A report on his investigations has just been issued by the U.S. Department of Agriculture; and from this it seems that the results achieved by him are highly satisfactory. Prof. Riley, who contributes an introduction to the report, says that one of the insects imported, the Cardinal Vedalia (*Vedalia cardinalis*, Mulsant), has multiplied and increased to such an extent as to rid many of the orange-groves of Icerya, and to promise immunity in the near future for the entire State of California.

SOME interesting notes on the archæology and ethnology of Easter Island, by Mr. Walter Hough, appear in the new number of the *American Naturalist*. One of the last acts of the late Prof. Spencer F. Baird was to induce the American Navy Department to send a vessel to explore the island and bring back representative specimens. The U.S.S. *Mohican*, then at Tahiti, was detailed, and the fruits of the successful twelve days' exploration are now to be seen in the north and west halls of the American National Museum. They consist of several stone images, carved stones, painted slabs, and a fine collection of smaller objects obtained by Paymaster W. J. Thomson, U.S.N. In his article Mr. Hough makes good use of the materials thus brought together, and of information placed at the disposal of the National Museum by Mr. Thomson, and by Surgeon G. H. Cooke, U.S.N.

TWO interesting papers on primitive architecture, by Mr. Barr Ferree, have been reprinted together, one from the *American Naturalist*, the other from the *American Anthropologist*. In the first article the author deals with sociological influences, in the second with climatic influences.

FROM the reports, for the past official year, of the Directors of Public Instruction and their subordinates in various Indian districts, on vernacular literature, it appears that, on the whole, but very little scientific work of an original character is being performed by natives of India, and that the taste for scientific literature, original or translated, can scarcely be said to exist. In Bengal, the Director says that, "while physiology keeps in old grooves, medicine seems to be trying to return to them." In Madras scientific works appear to have been confined to the translation of an old Sanskrit work on medicine, unless indeed "a collection of a thousand stanzas in Tamil verse, treating of the Yoga philosophy, can be called scientific." In the North-West Provinces eleven works on medicine were registered during the year, some of them being translations, while others are described as original works of some merit. The great mass of Indian literature appears to be composed of fiction, poetry, and the

drama, and, in Bengal especially, is described as for the most part worthless and immoral.

It is well known that a connection has been observed (in Munich and other towns) between ground-water and typhus; the disease gaining force as the water goes down, and declining as the water rises. (It is thought that certain decompositions are favoured by air taking the place of water in the ground.) While in former years Hamburg has exemplified this effect, the last typhus epidemic there, according to Prof. Brückner, was quite in discordance with the variations of ground-water. From 1838, it is stated, the typhus mortality in Hamburg steadily fell from 19 to 2 or 3 per 1000; but from 1885 it rose again to 9; and whereas before 1885 the epidemic was a summer one, with its maximum in August, it now became a winter one, with maximum in December. The curve of ground-water continued to have the same course as before. Prof. Brückner points out that this epidemic of 1884–87 corresponded in time with certain harbour works being carried out at Hamburg, and he attributes it to the upturning of enormous masses of earth, the abode of numberless bacteria, whose diffusion among the inhabitants was thus facilitated.

The volume of Results of the Magnetical and Meteorological Observations made at the Royal Observatory, Greenwich, in the year 1887, contains an appendix of considerable importance to meteorologists, viz. the hourly reduction of the photographic records of the barometer for 1874–76, and of the dry and wet bulb thermometers for 1869–76. This appendix, which is also published separately, continues the results for the twenty years published in 1878. The tables now given complete the reduction of the photographic records nearly to the present time, commencing with the year 1854 for the barometer, and with the year 1849 for the thermometers. The means for the two periods are given separately, but their value would be further enhanced if the results for the whole period were also given in a combined form.

With the month of January, the Monthly Weather Review of the United States Signal Service entered upon its eighteenth year of publication. The Review is based upon reports from 1934 observers, a large majority of whom belong to the State Weather Services. This number is exclusive of the reports which are usually supplied by the Central Pacific Railway Company, but which could not be forwarded for January, owing to snow blockades and floods. One hundred and twenty miles of the railroad crossing the Sierra Nevada range of mountains was blockaded by snow, being the heaviest blockade ever known there, and it is estimated that fully 50 per cent. of the live stock was lost from exposure and starvation. The paths of twelve depressions that appeared over the North Atlantic Ocean are plotted on a chart. Of the nine depressions that moved eastwards from the American continent, four were traced to the British Isles. Three storms first appeared over the ocean, and two of these were also traced to the British Isles. Among the "Notes and Extracts" is an article on the recent comparison of anemometers, by Prof. Marvin. The results obtained show that of the anemometers exposed to the same wind, those with short arms gave a lower velocity than those with long arms. No experiments were made beyond 32 miles per hour, and although various formulæ were given for the reduction of wind velocities, Prof. Marvin states that they cannot be depended on for velocities beyond the experimental values, so that much more information has yet to be gained, as to the action of anemometers with high velocities, from careful experiments with whirling machines. We take this opportunity of pointing out that a general subject-index to the Monthly Weather Reviews and the Annual Reports of the Chief Signal Officer, to 1887, has been published, and affords easy reference to the valuable information contained in these publications.

A recent writer in the *North China Herald* of Shanghai says that the climate of Asia is becoming colder than it formerly was, and its tropical animals and plants are retreating southwards at a slow rate. This is true of China, and it is also the case in Western Asia. The elephant in a wild state was hunted in the eighth century B.C. by Tiglath Pileser, the King of Assyria, near Carchemish, which lay near the Euphrates in Syria. Four or five centuries before this Thothmes III., King of Egypt, hunted the same animal near Aleppo. In high antiquity the elephant and rhinoceros were known to the Chinese, they had names for them, and their tusks and horns were valued. South China has a very warm climate which melts insensibly into that of Cochin-China, so that the animals of the Indo-Chinese peninsula would, if there were a secular cooling of climate, retreat gradually to the south. This is just what seems to have taken place. In the time of Confucius elephants were in use for the army on the Yangtze River. A hundred and fifty years after this, Mencius speaks of the tiger, the leopard, the rhinoceros, and the elephant, as having been, in many parts of the empire, driven away from the neighbourhood of the Chinese inhabitants by the founders of the Chou dynasty. Tigers and leopards are not yet by any means extinct in China. The elephant and rhinoceros are again spoken of in the first century of our era. If to these particulars regarding elephants be added the retreat from the rivers of South China of the ferocious alligators that formerly infested them, the change in the fauna of China certainly seems to show that the climate is much less favourable for tropical animals than it formerly was. In fact it appears to have become drier and colder. The water buffalo still lives, and is an extremely useful domestic animal, all along the Yangtze and south of it, but is not seen north of the old Yellow River in the province of Kiangsu. The Chinese alligator is still found in the Yangtze, but so rare is its appearance that foreign residents in China knew nothing about it till it was described by M. Fauvel. The flora is also affected by the increasing coldness of the climate in China. The bamboo is still grown in Peking with the aid of good shelter, moisture, and favourable soil, but it is not found naturally growing into forest in North China, as was its habit two thousand years ago. It grows now in that part of the empire as a sort o garden plant only. It is in Szechuan province that the southern flora reaches farthest to the northward.

Some interesting experiments on the physiology of sponges have been recently made by Dr. Lendenfeld, of Innsbruck (*Humboldt*). He operated with eighteen different species, putting carmine, starch, or milk, in the water of the aquarium, and also trying the effect of various poisons—morphine, strychnine, &c. The following are some of his results: Absorption of food does not take place at the outer surface, but in the interior; only foreign substances used for building up the skeleton enter the sponge without passing into the canal-system. Grains of carmine and other matters often adhere to the flat cells of the canals, but true absorption only takes place in the ciliated cylindrical cells of the ciliated chamber. These get quite filled with carmine grains or milk spherules, but starch grains prove too large for them. Remaining in these cells a few days, the carmine cells are then ejected; while milk particles are partly digested, and then passed on to the migratory cells of the intermediate layer. Any carmine particles found in these latter cells have entered accidentally through external lesions. The sponge contracts its pores when poisons are put in the water; and the action is very like that of poisons on muscles of the higher animals. Especially remarkable is the cramp of sponges under strychnine; and the lethargy (to other stimuli) of sponges treated with cocain. As these poisons, in the higher animals, act indirectly on the muscles through the nerves, it seems not without warrant to suppose that sponges also have nerve-cells which cause muscular contraction.

THE additions to the Zoological Society's Gardens during the past week include a Black-eared Marmoset (*Hapale penicillata*) from South-east Brazil, presented by Mr. J. A. Watson, F.Z.S; a Lesser White-nosed Monkey (*Cercopithecus petaurista* ♀) from West Africa, presented by Mr. E. B. Parfitt; a Macaque Monkey (*Macacus cynomolgus* ♀) from India, presented by Mrs. H. F. Batt; a Sambur Deer (*Cervus aristotelis* ♂) from India, presented by Capt. George James; a Common Badger (*Meles taxus*, white variety), British, presented by the Hon. Morton North; a Jackdaw (*Corvus monedula*), British, presented by Mrs. Bowden; a Blessbok (*Alcelaphus albifrons* ♂) from South Africa, four Undulated Grass Parrakeets (*Melopsittacus undulatus* 2 ♂ 2 ♀) from Australia, deposited; an Australian Crane (*Grus australasiana*), two Chestnut-eared Finches (*Amadina castanotis*) from Australia, three European Flamingoes (*Phœnicopterus antiquorum*), four Great Bustards (*Otis tarda*), European, purchased.

OUR ASTRONOMICAL COLUMN.

OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at Greenwich at 10 p.m. on April 17 = 11h. 43m. 55s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G.C. 2841	—	White.	12 13 33	+47 55
(2) 137 Schj.	6	Yellowish-red.	10 54 5	−15 52
(3) ϕ Leonis...	4	Yellowish-white.	11 11 6	− 3 3
(4) β Leonis	2	White.	11 43 30	+15 11
(5) 155*b* Schj.	7	Red.	12 52 6	+66 35
(6) U Virginis	Var.	Reddish.	12 45 31	− 6 9

Remarks.

(1) This large white nebula is situated in the constellation of Ursa Major, and is thus described in the General Catalogue:—"Very bright, very large, suddenly brighter in the middle to a nucleus." According to Smyth, it is oval in shape, the lateral edges being better defined than the ends. Lord Rosse's telescope s' owed it to be much mottled. In 1866 Dr. Huggins described its spectrum as continuous, with "a suspicion of unusual brightness about the middle part." No observations of the spectrum appear to have been made since then, but it is important that it should be re-examined. The spectra of the white nebulæ are usually almost entirely wanting in red light, and it is therefore quite possible that the brightening in the middle is nothing more than the green carbon fluting near λ517. Direct comparisons with the spectrum of a spirit-lamp flame would soon decide this point. In any case, if there be one or more brightenings, some attempt should be made to determine their positions.

(2) The spectrum of this star has not yet been completely described. Secchi stated that it was of the type of α Orionis, and Dunér states that it is most probably a star of Group II., but very feebly developed. As I have previously pointed out, it is these "feebly developed" stars of Group II. which require further examination rather than those which are described as "fully developed," as they are probably transition stages between Groups I. and II., or Groups II. and III.

(3) According to Konkoly, this star has a well-developed spectrum of the solar type. Differential observations as to whether the star belongs to Group III. or to Group V. are required. (For criteria so far determined, see p. 20.)

(4) The spectrum of this star is a very fine one of Group IV. The usual observations are required.

(5) D'Arrest and Dunér both describe the spectrum of this star as a magnificent one of Group VI. According to Dunér, the principal bands are very dark, and the subsidiary bands 4 and 5 are well visible, while the bands 1, 2, 3 are very weak. He also states that the spectrum is rendered unique by the fact that the least refrangible part of the sub-zone in the yellow is considerably weaker than the other. Further observations, as previously suggested for similar stars, should be made.

(6) This star affords another opportunity of searching for bright lines in the spectrum of a variable of Group II. near maximum. Vogel states that the spectrum is a fine one of Group II., but we have as yet no detailed description of the bands present. The period of the variable is about 207 days, and it ranges in magnitude from 7·7–8·1 at maximum to 12·2–12·8 at minimum. The maximum will occur on April 21, but as Mr. Espin has noticed that the bright lines sometimes do not appear until after the maximum, it will be desirable to continue the observations for some days after. The variations of the bright carbon flutings should also receive attention.

A. FOWLER.

COMET BROOKS (*a* 1890).—The following elements have been computed by Dr. Bidschof, of the Imperial Observatory, Vienna, from observations at Cambridge, U.S., March 21; Vienna, March 4 and 28 (*Astr. Nach.*, No. 2962):—

T = 1890 June 3·6399 Berlin mean time.

$$\left.\begin{array}{l} \omega = 71^\circ\ 7'\cdot 5 \\ \Omega = 320\ 44\cdot 9 \\ \iota = 121\ 17\cdot 2 \end{array}\right\} \text{Mean Eq. 1890·0.}$$

$$\log q = 0\cdot 27189$$

Ephemeris for Berlin Midnight.

1890.	R.A.	Decl.	1890.	R.A.	Decl.
	h. m. s.	° ′		h. m. s.	° ′
April 16...	21 9 21...	+19 21·0	April 26...	21 4 5...	+26 15·1
17...	9 0...	19 59·2	27...	3 18...	27 0·9
18...	8 37...	20 38·0	28...	2 27...	27 47·6
19...	8 13...	21 17·5	29...	1 33...	28 35·0
20 ..	7 47...	21 57·7	30...	0 34...	29 33·3
21...	7 18...	22 38·6	May 1...	20 59 31...	30 12·4
22...	6 46...	23 20·3	2...	58 23...	31 2·3
23...	6 10...	24 2·8	3...	57 10...	31 52·9
24...	5 32...	24 46·1	4...	55 51...	32 44·3
25...	4 50...	25 30·2			

Brightness, that at discovery being unity—

18 April = 1·81.	30 April = 2·39.
22 ,, = 1·99.	4 May = 2·62.
26 ,, = 2·18.	

NEW VARIABLE IN CÆLUM.—Prof. Pickering, in a communication to *Astr. Nach.*, No. 2962, notes that an examination of a plate taken by Mr. S. J. Bailey at the Closica station in Peru, shows that the G and *h* lines of hydrogen are bright in the spectrum of a star whose position for 1875 is R.A. 4h. 36·2m., Decl. − 38° 29′. An inspection of photographic chart plates indicates that the star is variable, and its spectrum seems to place it in the same class as *o* Ceti, R Hydrae, R Leonis, and other long-period variables. The date on which the plate was taken is not given, but it is observed that the spectrum is as bright photographically as that of Cordoba Catalogue No. 1077, which is of the magnitude 7½, and since the former is a red star, it was probably much brighter visually. Eye observations at Cambridge, U.S., on February 20 and 21 of this year show that the star was then about magnitude 10·5. It seems, therefore, that the bright lines of hydrogen were photographed in the spectrum of this object when it was near a maximum.

GEOGRAPHICAL NOTES.

THE Council of the Royal Geographical Society met on Monday, and finally decided upon the awards of the honours for the year. One of the Royal Medals has been awarded to Emin Pasha, in recognition of the services rendered by him to geography and the allied sciences by his explorations and researches in the countries east, west, and south of the Upper Nile during his administration of the Equatorial Province of Egypt. The other Royal Medal has been awarded to Lieut. F. E. Younghusband, for his journey across Central Asia in 1886–87, from Manchuria and Pekin *viâ* Hami and Kashgar, and over the Mushtagh to Cashmere and India, a distance of 7000 miles. The Cuthbert Peek grant has been awarded to Mr. E. C. Hare for his observations on the physical geography of Tanganyika made during his many years' residence on that lake. The Murchison grant has been awarded to Signor Vittorio Sella, in consideration of his recent journey in the Caucasus, and the advance made in our knowledge of the physical characteristics and the topography of the chain by means of his series of panoramic photographs taken above the snow level. The Gill

memorial has been given to Mr. C. M. Woodford, for his three expeditions to the Solomon Islands, and the additions made by him to our topographical knowledge and the natural history of the islands. The new honorary corresponding members are Prof. Davidson, of San Francisco; Dr. Junker, the friend of Emin Pasha, and Central African explorer; and Senhor Santa Anna Nery, of Rio Janeiro.

AT the evening meeting of the Royal Geographical Society on Monday, Sir M. E. Grant Duff in the chair, Dr. Hans Meyer read a paper on his journey to the summit of Kilima-Njaro. After giving a short account of his expedition in 1887, and the discouragements to which he had been subjected on two subsequent efforts to carry out his programme, Dr. Meyer proceeded to say that, while the main portion of the caravan encamped in Marangu, he ascended with Herr Purtscheller and eight picked men through the primæval forest to a stream beyond, where he had encamped in the year 1887, at an altitude of 9200 feet. There their large tent was pitched, straw huts were built for the men, and firewood collected. Accompanied by four men they travelled for two more days up the broad, grassy, southern slopes of Kilima-Njaro to the fields of rapilli on the plateau between Kibo and Mawenzi, and found there to the south-east of Kibo, under the protection afforded by some blocks of lava, a spot, at an altitude of 14,270 feet, well suited for the erection of their small tent. As soon as the instruments and apparatus had been placed under cover, three of the men returned to the camp on the edge of the forest, and only one, a Pangani negro, Mwini Amani by name, remained to share, uncomplainingly, their sixteen days' sojourn on the cold and barren heights. With regard to their maintenance, it had been arranged that every third day four men should come up with provisions from the lower camp in Marangu to the central station on the edge of the forest, and that two of the men stationed there should thence convey the necessary food to them in the upper camp, returning immediately afterwards to their respective starting-places. And this accordingly was done. Firewood was supplied by the roots of the low bushes still growing there in a few localities, and their negro fetched a daily supply of water from a spring rising below the camp. In that manner they were enabled, as if from an Alpine Club hut, to carry out a settled programme in the ascent and surveying of the upper heights of Kilima-Njaro. The ice-crowned Kibo towered up steeply another 5000 feet to the west of their camp, itself at an altitude of 14,300 feet. On October 3 they undertook their first ascent. The previous day they had resolved to make the first attempt, not in the direction chosen by him in 1887, but up a large rib of lava which jutted out to the south-east, and formed the southern boundary of the deepest of the eroded ravines on that side of the mountain. Their simple plan of operations, which they succeeded in carrying out, was to climb up this lava-ridge to the snow-line, to begin from its uppermost tongue the scramble over the mantle of ice, and endeavour to reach by the shortest way the peak to the south of the mountain, which appeared to be the highest point. It was not till half-past 7 o'clock that they reached the crown of that rib of lava which had been their goal from the very first, and, panting for breath, they began to pick their way over the boulders and *débris* covering the steep incline of the ridge. Every ten minutes they had to pause for a few moments to give their lungs and beating hearts a short breathing space, for they had now for some time been above the height of Mont Blanc, and the increasing rarefaction of the atmosphere was making itself gradually felt. At an altitude of 17,220 feet they rested for half an hour; apparently they had attained an elevation superior to the highest point of Mawenzi, which the rays of the morning sun were painting a ruddy brown. Below them, like so many mole-heaps, lay the hillocks rising from the middle of the saddle. A few roseate cumulus clouds floated far over the plain, reflecting the reddish-brown laterite soil of the steppe; the lowlands, however, were but dimly visible through the haze of rising vapour. The ice-cap of Kibo was gleaming above their heads, appearing to be almost within reach. Shortly before 10 o'clock they stood at its base, at an elevation of 18,270 feet above sea-level. At that point the face of the ice did not ascend, but almost immediately afterwards it rose at an angle of 35°, so that, without ice-axes, it would have been absolutely impracticable. The toilsome work of cutting steps in the ice began about half-past 10; slowly they progressed by the aid of the Alpine rope, the brittle and slippery ice necessitating every precaution. They made their way across the crevices of one of the glaciers that projected downwards into the valley which they had traversed in the early morning, and took a rest under the shadow of an extremely steep protuberance of the ice-wall at an altitude of 19,000 feet. On recommencing the ascent the difficulty of breathing became so pronounced that every fifty paces they had to halt for a few seconds, bending their bodies forward and gasping for breath. The oxygen of the air amounted there, at an elevation of 19,000 feet, to only 40 per cent., and the humidity to 15 per cent. of what it was at sea level. No wonder that their lungs had such hard work to do. The surface of the ice became increasingly corroded; more and more it took the form which Güssfeldt, speaking of Aconcagua, in Chili, called *nieve penitente*. Honeycombed to a depth of over 6 feet, in the form of rills, teeth, fissures, and pinnacles, the ice-field presented the foot of the mountaineer with difficulties akin to that of a "Karrenfeld." They frequently broke through as far as their breasts, causing their strength to diminish with alarming rapidity. And still the highest ridge of ice appeared to be as distant as ever. At last, about 2 o'clock, after eleven hours' climb, they drew near the summit of the ridge. A few more hasty steps in the most eager anticipation, and then the secret of Kibo lay unveiled before them. Taking in the whole of Upper Kibo, the precipitous walls of a gigantic crater yawned beneath them. The first glance told that the most lofty elevation of Kibo lay to their left, on the southern brim of the crater, and consisted of three pinnacles of rock rising a few feet above the southern slopes of the mantle of ice. They first reached the summit on October 6, after passing the night below the limits of the ice, in a spot sheltered by overhanging rocks, at an altitude of 15,160 feet, an elevation corresponding to that of the summit of Monte Rosa. Wrapped up in their skin bags, they sustained with tolerable comfort even the minimum temperature of 12° F., experienced during the night, and were enabled, about 3 o'clock in the morning of October 6, to start with fresh energy on their difficult enterprise of climbing the summit; and this time Njaro, the spirit of the ice-crowned mountain, was gracious to them—they reached their goal. At a quarter to 9 they were already standing on the upper edge of the crater, at the spot from which they had retraced their steps on October 3. Their further progress, from this point to the southern brim of the crater, although not easy, did not present any extraordinary difficulty. An hour and a half's further ascent brought them to the foot of the three highest pinnacles, which they calmly and systematically climbed one after another. Although the state of the atmosphere and the physical strain of exertion remained the same as on the previous ascent, yet this time they felt far less exhausted, because their condition morally was so much more favourable. The central pinnacle reached a height of about 19,700 feet, overtopping the others by 50 to 60 feet. He was the first to tread, at half-past 10 in the morning, the culminating peak. He planted a small German flag, which he had brought with him in his knapsack, upon the rugged lava summit, and christened that—the loftiest spot in Africa—Kaiser Wilhelm's Peak. After having completed the necessary measurements, they were free to devote their attention to the crater of Kibo, of which an especially fine view was obtainable from Kaiser Wilhelm's Peak. The diameter of the crater measured about 6500 feet, and it sank down some 600 feet in depth. In the southern portion the walls of lava were either of an ash-grey or reddish-brown colour, and were entirely free from ice, descending almost perpendicularly to the base of the crater; and in its northern half the ice sloped downwards from the upper brim of the crater in terraces, forming blue and white galleries of varying steepness. A rounded cone of eruption, composed of brown ashes and lava, rose in the northern portion of the crater to a height of about 500 feet, which was partly covered by the more than usually thick sheet of ice extending from the northern brim of the crater. The large crater opened westwards in a wide cleft, through which the melting water ran off, and the ice lying upon the western part of the crater and the inner walls issued in the form of a glacier. What a wonderful contrast between this icy stream and the former fiery incandescence of its bed! And above all this there reigned the absolute silence of inanimate nature, forming in its majestic simplicity a scene of the most impressive grandeur. An indelible impression was created in the mind of the traveller to whom it had once been granted to gaze upon a scene like that, and all the more when no human eye had previously beheld it. And certainly as they sat that evening in their little tent, which they finally reached at nightfall, after a most arduous return march through the driving mist, and carried their thoughts back to the expeditions of 1887

and 1888, they would indeed have changed places with no one. After giving further details of the expedition, the lecturer said that on October 30 they sorrowfully bade farewell to Kilima-Njaro, the most beautiful and interesting, as well as the grandest, region in the dark continent. At the conclusion of the paper a series of photographs illustrative of some features of the expedition was exhibited by lime-light, and explained by Mr. Ravenstein. A vote of thanks to Dr. Meyer was proposed by Mr. Joseph Thomson, seconded by Mr. Douglas Freshfield, and heartily accorded.

A NEW GREEN VEGETABLE COLOURING MATTER.[1]

THE seeds of the *Trichosanthes palmata* are inclosed in a rounded scarlet fruit and embedded in a green bitter pulp. The bitter principle has been shown by Mr. D. Hooper to be a glucoside differing from colocynthin, and he has named it trichosanthin. The green colouring matter, when freed from the trichosanthin and fatty matter, yields a solution closely resembling a solution of cholorophyll. It is green in thin and red in thick layers, and has a red fluorescence. The spectrum, however, is very different. Taking the thickness and strength yielding the most characteristic spectrum, it may be described thus:—The first band begins (penumbra) at W.L. 654 and ends about W.L. 615; from this there is a small amount of absorption till the second band begins at W.L. 593·4, and continues to W.L. 566·8, with the maximum absorption near the less refrangible end; from this there is no perceptible absorption till the third band, which extends from W.L. 548·4 to 534·8; there is a fourth band, very faint, with its centre about W.L. 510·6, and a fifth extending from about W.L. 485 to W.L. 473·4. Comparing this with the chlorophyll spectrum, it will be seen that the first band has its centre almost midway between the two chief chlorophyll bands, but that bands III., IV., and V. are probably coincident with chlorophyll bands. When the trichosanthes colouring matter is treated with ammonia sulphide the spectrum is completely changed. The first and most prominent band slowly decreases in strength and finally disappears, two new bands appear in the space between bands I. and II. of the original spectrum; band II. is apparently displaced towards the violet end and intensified; and band IV. is greatly widened. Chlorophyll under the same treatment behaves in a totally different manner, and the two spectra become almost complementary. When, however, the trichosanthes colouring matter and chlorophyll are both treated with hydrochloric acid the result is very different, for the two spectra have now three bands in common. The first band in the trichosanthes spectrum has disappeared, and the spectrum is practically reduced to one of three bands corresponding in position with bands II., III., and IV. of the altered chlorophyll spectrum. Band I. of the chlorophyll spectrum has no representative in the trichosanthes spectrum. The conclusions to be derived from a study of these spectra seem to be that we have in the trichosanthes colouring matter a substance in which the "blue chlorophyll" of Sorby or the "green chlorophyll" of Stokes is replaced by some other substance easily decomposed by reducing agents and acids. Farther, if we assume with Schunck that the product obtained by acting on chlorophyll with hydrochloric acid is the same as Frémy's phyllocyanin, this, too, must be a mixture, one constituent of which is obtained by acting on the trichosanthes colouring matter with acid, while the other is, apparently, the unaltered substance yielding band I. in the chlorophyll spectrum.

SOCIETIES AND ACADEMIES

LONDON.

Royal Society, March 13.—"On the Organization of the Fossil Plants of the Coal-measures. Part XVII." By William Crawford Williamson, LL.D., F.R.S., Professor of Botany in the Owens College, Manchester.

In 1873 the author described in the Phil. Trans. an interesting stem of a plant from the Lower Carboniferous beds of Lancashire, under the name of *Lyginodendron Oldhamium*. He also called attention to some petioles of ferns, more fully described in 1874, under the name of *Rachiopteris aspera*. The former of these plants possessed a highly organized, exogenously developed xylem zone, whilst the *Rachiopteris* was only supplied with what looked like closed bundles. Since the dates referred to, a large amount of additional information has been obtained respecting both these plants. Structures, either not seen, or at least ill-preserved, have now been discovered, throwing fresh light on their affinities; but most important of all is the proof that the *Rachiopteris aspera* is now completely identified as the foliar rachis or petiole of the *Lyginodendron*: hence there is no longer room for doubting that, notwithstanding its indisputable possession of an exogenous vascular zone, the bundles of which exhibit both xylem and phloëm elements along with medullary and phloëm rays, it has been a true Fern. Though such exogenous developments have now been long known to exist amongst the Calamitean and Lycopodiaceous stems, as well as in other plants of the Carboniferous strata, we have had no evidence until now that the same mode of growth ever occurred amongst the Ferns. Now, however, this Cryptogamic family is shown to be no longer an exceptional one in this respect. All the three great divisions of the Vascular Cryptogams—the Equisetaceæ, the Lycopodiaceæ, and the Homosporous Filices of the primæval world—exhibited the mode of growth which is confined, at the present day, to the Angiospermous plants. A further interesting feature of the life of this *Lyginodendron* is seen in the history of the development of its conspicuous medulla. In several of his previous memoirs, notably in his Part IV., the author has demonstrated a peculiarity in the origin of the medulla of the Sigillarian and Lepidodendroid plants. Instead of being a conspicuous structure in the youngest state of the stems and branches of these plants, as it is in the recent Ferns, and as in most of the living Angiosperms, few or no traces of it are observable in these fossil Lycopodiaceæ. In them it develops itself in the interior of an apparently solid bundle of tracheæ (within which doubtless some obscure cellular germs must be hidden), but ultimately it becomes a large and conspicuous organ. The author has now ascertained that a similar medulla is developed, in precisely the same way, within a large vascular bundle occupying the centre of the very young twigs of the *Lyginodendron*. But in this latter plant other phenomena associated with this development make its history even yet more clear and indisputable than in the case of the Lycopods. The entire history of these anomalous developments adds a new chapter to our records of the physiology of the vegetable kingdom.

Further light is also thrown upon the structure of the *Heterangium Grievii*, originally described in the author's memoir, Part IV. This plant presents many features in its structure suggesting that it too will ultimately prove to be a Fern. The specimens described in the above memoir, published in 1873, all possessed a more or less developed exogenous xylem zone. But the author has now obtained other, apparently younger examples in which no such zone exists.

He has discovered the stem of a genus of plants (*Bowmanites*), hitherto known only by some fruits, the detailed organization of which was originally described by him in the Transactions of the Literary and Philosophical Society of Manchester, in 1871. The structure of this new stem corresponds closely with what is seen in *Sphenophyllum* and in some forms of *Asterophyllites* (Memoir V., Phil. Trans., 1874, p. 41, *et seq.*). This discovery makes an addition to our knowledge of the great Calamarian family, to which the plant obviously belongs.

Further demonstrations are also given by the author, illustrating some features in the history of the true Calamites. Attention is called to the fact that, whilst the large, longitudinally-grooved and furrowed inorganic casts of the central medullary cavities of these plants are extremely common, we never find similar casts of the smaller branches. The cause of this is demonstrated in the memoir. In these young twigs the centre of the branch is at first occupied by a parenchymatous medulla. The centre of this medulla becomes absorbed at a very early age, leaving the beginnings of a small fistular cavity in its place; but, if any plastic mud or sand entered this cavity when the plant was submerged, the surface of such a cast would exhibit no longitudinal groovings, because there would be nothing in the remaining medullary cells surrounding the cast to produce such an effect. It was only when the further growth of the branch was accompanied by a more complete absorption of the remaining medullary cells, causing the cavity thus produced to

[1] Abstracted from a paper by C. Michie Smith, "On the Absorption Spectra of Certain Vegetable Colouring Matters," read before the Royal Society of Edinburgh, March 17, 1890, and communicated by permission of the Council.

be bounded by the inner wedge-shaped angles of the longitudinal vascular bundles constituting the xylem zone, that such an effect could be produced. After that change any inorganic substance finding its way into the interior of this cavity had its surface so moulded by the wedges as to produce the superficial ridges and furrows so characteristic of these inorganic casts.

March 27.—"The Rupture of Steel by Longitudinal Stress." By Chas. A. Carus-Wilson. Communicated by Prof. G. H. Darwin, F.R.S.

This paper gives an account of experiments made with a view to determining the nature of the resistance that has to be overcome in order to produce rupture in a steel bar by longitudinal stress.

The stress required to produce rupture is in every case computed by dividing the load on the specimen at the moment of breaking by the contracted area at the fracture measured after rupture; this stress is called the "true tensile strength" of the material.

It is well known that any want of uniformity in the distribution of the stress over the ruptured section causes the bar to break at a lower stress than it would if the stress was uniformly distributed. Hence anything that causes want of uniformity is prejudicial; for instance, a groove turned in a cylindrical steel bar will produce want of uniformity, and will consequently be prejudicial, the stress at rupture being lower according as the angle of the groove is more acute. The most favourable condition of test might appear to be that in which a bar of uniform section throughout its length was allowed to draw out freely before breaking, since in this case the stress must be most uniformly distributed.

Experiment, however, shows that the plain bar is not always the strongest. So long as the want of uniformity of stress is considerable, owing to the groove being cut with a very sharp angle, the plain bar is stronger than the grooved bar; but, if the groove be semicircular instead of angular, the grooved bar is considerably stronger than the plain, in spite of the fact that the stress is more uniformly distributed in the latter.

It would seem, then, that we can strengthen a bar over any given section by adding material above and below it, the change in section being gradual; but such an addition of material cannot strengthen the bar if rupture is caused by a certain intensity of tensile stress over the ruptured section; the added material cannot increase the resistance of the ruptured section to direct tensile stress, but it can increase the resistance to the shearing stress.

The resistance of a given section of a steel bar does not, then, depend on its section at right angles to the axis, but on its section at 45° to the axis, for in that direction the shearing stress is a maximum. From this it would seem that the resistance overcome at rupture is the resistance of the steel to shear.

Experiments were made to see whether the resistance of steel to direct shearing bore to its resistance to direct tension the ratio required by the above theory; since the greatest shearing stress is equal to one-half the longitudinal stress, we should expect to find the resistance to direct shearing equal to one-half of the resistance to direct tension.

A series of experiments were made, with the result that the ultimate resistance to direct shearing was within, on the average, 3 per cent. of the half of that to direct tension.

The appearance of the fracture of steel bars is next discussed. It would appear that when the stress is uniformly disturbed in the neighbourhood of the ruptured section, the fracture is at 45° to the axis, the bar having sheared along that plane which is a plane of least resistance to shear. The tendency to rupture along a plane of shear may be masked by a non-uniform distribution of stress.

Two plates of photographs are added, showing examples of steel bars broken by shearing under longitudinal stress.

Physical Society, March 21.—Prof. W. E. Aryton, F.R.S., President, in the chair.—The following communications were read:—The Villari critical points in nickel and iron, by Herbert Tomlinson, F.R.S. Villari has shown that the permeability of iron is increased by longitudinal traction provided the magnetizing force does not exceed a certain limit, but beyond this limit traction produces a decrease of permeability. The value of the force for which traction produces no change in the permeability is known as the Villari critical point. As far as the author is aware, no previous observer has found a similar critical point for nickel, but by confining his attention to *temporary* magnetization he has detected such a point with comparative ease. He has also examined the variation of the Villari critical points in iron and nickel with change of load, and has investigated the influence of permanent strain on these points. The experiments were made by the ballistic method, using wires about 400 diameters long. In each set of observations the permeability was obtained with various loads, the magnetizing force being kept the same, and with each load the circuit was closed and opened until the swings on make and break were equal; this swing was taken as a measure of the induction under the given load. Several diagrams accompany the paper, in which load and percentage change of permeability are plotted, regard being had to sign. The author finds that for annealed unstrained iron the critical value of the force decreases as the load increases, and that the Villari point is much lower for *temporary* than for *total* magnetization. With a load of 4·7 kilos on a 1 mm. wire, the value of the force giving the temporary point was 2·8 C.G.S. units. He also found that for a given magnetizing force there are generally two loads which have no effect on the temporary magnetization. With unstrained nickel the critical value of the force is much greater than in iron, being about 114 C.G.S. units for a load of 10 kilos on a wire 0·8 mm. diameter, and 67 for a load of 6·6 kilos. For a force of 21 units no critical point exists. Experiments on a permanently strained iron wire show that for magnetizing forces ranging from 0·03 to 0·3 there is no critical point, and all the resulting curves are identical. There is, however, considerable difference in the observations taken during loading and those taken on unloading. For greater magnetizing forces the curves cease to be identical, and the maximum increase of permeability becomes less and less until for a certain force the curves begin to cut the load line. As the force increases beyond this value the point of cutting approaches the origin, and the curves begin to cut the load line in two points. Further increase of force to 3 C.G.S. units causes the first point to disappear, and the second point recedes from the origin. Finally, with sufficiently high magnetizing forces the second point cannot be reached before the wire breaks, and the curve lies entirely below the load line. With nickel the curves for very minute forces, like those of iron, are exactly the same for different values of the force, but they lie below the load line, *i.e.* the permeability is diminished by loading; there is no difference, however, in the loading and unloading curves. Beyond a certain value of the force the identity of the curves ceases, and that part of the curve near the origin bulges towards the load line. For a force a little over 21 C.G.S. units the permeability begins to increase with load, and the curve cuts the line in one point, which point recedes from the origin as the force increases. Mr. Shelford Bidwell said that Prof. J. J. Thomson, reasoning from the change of length by magnetization, had predicted a Villari point in cobalt when compressed, and this was verified experimentally. On applying similar reasoning to nickel he, (the speaker) did not expect to find a Villari point, and both Sir William Thomson and Prof. Ewing had searched in vain for one. In some experiments, not yet completed, he had examined the behaviour of nickel, both loaded and unloaded, when subjected to various magnetizing forces. These show that the metal always contracts when magnetized. For no load the contraction at first increased with the magnetizing force, but attains a maximum. With a moderate load the contraction is less for small forces, but for larger forces becomes equal and then exceeds the contraction of the unloaded wire. For greater loads the contraction is less than when unloaded for all values of the force.—On Bertrand's Idiocyclophanous prism by Prof. S. P. Thompson. This hitherto undescribed prism is a total reflection one made of calc-spar, which shows to the naked eye the rings and crosses such as are seen when a slice of spar is examined by convergent light in a polariscope. The spar is cut so that the light after the first reflection passes along the optic axis, and after a second reflection emerges parallel to the incident light. The rings and brushes are present in pairs, but two pairs may be seen by tilting the prism to one side or the other. This was demonstrated before the Society. Prof. Thompson also exhibited a similar prism cut from quartz. Owing to the feeble double-refracting of the substance, no conspicuous rings could be seen, but when examined by the lantern traces of such rings were visible.—On the shape of the movable coils used in electrical measuring instruments, by Mr. T. Mather. The object of this note is to determine the best shape of the horizontal section of swinging coils such as are used in D'Arsonval galvanometers,

electro-dynamometers, wattmeters, &c. Assuming constant period and constant moment of inertia about the axis of rotation, it is shown that for zero instruments, the best shape of the section is two circles tangential to the direction of the deflecting field at the point about which the coil turns. A table accompanies the paper, in which various forms of section are given, together with their relative deflecting moments per unit moment of inertia; the coils being taken of equal lengths and the current density constant. From this table it appears that ordinary D'Arsonval coils only give about 45 per cent. of the maximum deflecting moment, and ordinary Siemens' dynamometers from 40 to 53 per cent. The various assumptions made in the paper are shown to be justifiable in commercial instruments, and the modifications necessary in special cases are pointed out. Mr. C. V. Boys said he had, when working at his radio-micrometer, arrived at a shape similar to that recommended in the paper. He also noticed a peculiar relation true for all shapes where the length parallel to the axis of rotation is great compared with the breadth. Suppose a coil of any dimensions, then another coil of half the breadth and double the length and cross-section will be dynamically, electrically, and magnetically the same as the original; for the moment of inertia, the electric resistance, and the enclosed magnetic field are equal. The above relation is also true when the breadth is not small, if the cross pieces be thickened near the axis so as to make their moment of inertia proportional to their length. He inquired whether the author had considered the subject of grading movable coils; he himself was of opinion that, unlike fixed galvanometer coils, the wire near the axis should be thicker than that further away. The President remarked that in 1881 Prof. Perry and himself exhibited a wattmeter at the Society of Arts, whose movable coil somewhat resembled one of those in the paper, which gave a deflecting moment of 95 per cent. of the maximum. In designing the instrument they had felt that the ordinary method of using a comparatively large swinging coil was not the best, and this led them to the shape adopted.

Entomological Society, April 2.—Mr. Frederick DuCane Godman, F.R.S., Vice-President, in the chair.—Mr. Godman announced the death of Dr. J. S. Baly, of Warwick, the well-known Coleopterist, who had been a member of the Society for the last forty years.—Dr. Sharp exhibited and made remarks on a female specimen of *Temnochila quadricollis*, Reitt., which was the subject of a very unusual malformation of the nature termed "ectromélie" by Lacordaire.—Mr. R. W. Lloyd exhibited three specimens of *Elater pomonæ*, taken at Brockenhurst about the middle of March last.—Colonel Swinhoe exhibited, and read notes on, a number of butterflies of the genus *Euthalia*. He pointed out that the specimens described as a species by the name of *Euthalia sedeva* were only the females of *E. balarama*.—Mr. T. R. Billups exhibited male and female specimens of *Cecidomyia salicis-siliqua*, Walsh, which had just emerged from galls received from Mr. Cockerell, who had collected them on a species of sallow in Colorado. He also exhibited three species of Ichneumonidæ new to Britain, viz. *Ichneumon haglundi*, Holmgr.; *Phygadeuon rufo-niger*, Bridg.; and *Phygadeuon sodalis*, Tasch.—Mr. C. G. Barrett exhibited specimens of *Bryotropha obscurella*, Hein, and *Doryphora elongella*, Hein, two species of Micro-Lepidoptera new to Britain.—Dr. Thallwitz, of Dresden, contributed a paper entitled "Notes on some species of the genus *Hilipus*." These notes had reference to a paper on the genus *Hilipus*, by Mr. F. P. Pascoe, published in the Transactions of the Society for 1889.—Mr. E. Meyrick read a paper entitled "The Classification of the Pyralidina of the European Fauna."—Prof. Westwood communicated a paper entitled "Notes on certain species of Cetoniidæ."—Mynheer P. C. T. Snellen, of Rotterdam, contributed a paper entitled "A Catalogue of the Pyralidæ of Sikkim collected by H. J. Elwes and the late Otto Möller," and Captain Elwes read notes on the foregoing paper as an appendix. Mr. W. L. Distant, Colonel Swinhoe, Mr. McLachlan, and Mr. Jacoby took part in the discussion which ensued.

Zoological Society, April 1.—Dr. A. Günther, F.R.S., Vice-President, in the chair.—The Secretary read a report on the additions that had been made to the Society's Menagerie during the month of March 1890; and called special attention to a fine example of a rare Passerine Bird (*Hypocolius amplinus*) from Karachi, presented to the Society by Mr. W. D. Cumming, Curator of the Museum, Karachi; and to two Mantchurian Cranes (*Grus viridirostris*), presented to the Society by Mr. C. W. Campbell, of H.B.M.'s Consular Service, Corea.—Mr. J. H. Gurney, Jun., exhibited and made remarks on a hybrid between the Tree-Sparrow (*Passer montanus*) and the House-Sparrow (*P. domesticus*), bred in captivity at Norwich.—Mr. W. B. Tegetmeier, exhibited a specimen of a Greek Partridge, shot in the Rhone Valley, and of an abnormal Viper.—Mr. A. Smith-Woodward exhibited and made remarks on a specimen of a Mesozoic Palæoniscid Fish from New South Wales, and pointed out that the structure of its pelvic fins seemed to confirm the recent opinion that the Palæoniscidæ are related to the Acipenseridæ and not to the Lepidosteidæ. The author believed the specimen exhibited to be the only one of the kind in existence.—Mr. C. M. Woodford made some remarks on the fauna of the Solomon Islands; and exhibited a large number of photographs in illustration of his remarks and of his recent explorations in these islands.—A communication was read from Dr. R. W. Shufeldt, entitled "Contributions to the Study of *Heloderma suspectum*," containing a complete account of the osteology and anatomy of this venomous Lizard. A list of the literature on the subject was added.—Dr. A. Günther, F.R.S., read the descriptions of new species of Deep-sea Fish from the Cape (*Lophotes fiski*), based on a specimen sent to the British Museum by the Rev. G. H. R. Fisk.—Mr. Edgar A. Smith, read a report on the Marine Molluscan Fauna of the Island of St. Helena, based principally on a large series of specimens collected by Captain Turton, R.E., and presented to the British Museum.—A second paper by Mr. Edgar A. Smith contained a report on the Marine Mollusca of Ascension Island.

Mathematical Society, April 3.—J. J. Walker, F.R.S., President, in the chair.—The following communications were made:—On the properties of some circles connected with a triangle formed by circular arcs, by Mr. Lachlan.—Some properties of numbers, by Mr. Christie.—The modular equations for $n = 17, 29$, by Mr. R. Russell. Communicated by Prof. Greenhill, F.R.S.

EDINBURGH.

Royal Society, March 17.—Sir W. Thomson, President, in the chair.—The President read a paper on an accidental illustration of the effective ohmic resistance to a transient electric current through an iron bar.—Prof. C. Michie Smith read a paper on the absorption spectra of certain vegetable colouring matters, the most interesting of which was a green colouring matter extracted from the pulp surrounding the seeds *tricosanthes palmata*. This substance is not chlorophyll, but is allied to it.—Prof. Smith also described a method of determining surface tensions by measurement of ripples. Ripples are set up on the surface of the liquid by means of a tuning-fork and the surface is then photographed along with a suitable scale. The lengths of the ripples can thus be obtained by micrometric measurements of the negative. The results obtained for mercury were very concordant, and agreed with the mean value obtained by Quincke. Strong electrification of the surface was found to reduce the value of the surface tension by more than 20 per cent. A few measurements of the surface tension of water also gave very fair results.—The Hon. Lord M'Laren read a paper on the solution of the three-term numerical equation of the nth degree.—The President read a paper, illustrated by a model, on a mechanism for the constitution of ether.

PARIS.

Academy of Sciences, April 8.—M. Duchartre in the chair.—M. Maurice Lévy, in a note on theories of electricity, shows that the formula given in his communication on March 17, representing the action between two moving electric particles, includes all the theories of electricity yet proposed, and that the values of an arbitrary constant required to satisfy each of the known theories are none of them competent to explain the movement of the perihelion of Mercury, whereas the latter is completely in accordance with the formula when another suitable value is chosen for the constant.—M. R. Lépine, in a note on the normal presence in chyle of a ferment destroying sugar, suggests that in the majority of cases of diabetes the disease is probably due to a defect in the production of this necessary body.—Observations of Brooks's comet (a 1890), made with the great equatorial of Bordeaux Observatory, by MM. Rayet, Picart, and Courty. The comet was observed on March 30 and 31, and

April 2 and 3.—Elements and ephemeris of Brooks's comet, by M. E. Viennet. Elements have been computed from observations at Cambridge, U.S., March 21; Kremsmunster, March 26; and Paris, March 31.—Observations of Brooks's comet, made at Paris Observatory, by Mdlle. D. Klumpke.—Fundamental common property of the two kinds of spectra, lines and bands; distinct characteristics of each of the classes; periodic variations to three parameters, by M. H. Deslandres. The facts relating to the periodic recurrence of doubles and triplets in spectra were previously given by M. Rydberg, and reduced to some simple laws (*Comptes rendus*, February 24). It was noted that the lines corresponding to doubles and triplets are represented by a function of whole numbers of the form $N = A - \frac{a}{(m+p)^2}$; where N is the number of waves; A, a, two constants; p a constant less than one, and m a whole number. This function has for a limit the more simple one $N = A - \frac{a}{m^2}$, which, when A and a have proper values, represents exactly, as was shown by Balmer, the unique series of the simple lines of hydrogen. The author states that the distribution of bands is in general more complex, the complete series of groups being represented by a function of three variable parameters, m, n, p — $N = f(n^2p^2) \times m^2 + Bn^2 + \phi(p^2)$; where m, n, and p, are whole numbers; B, a constant; f and ϕ some simple functions the study of which is not completed. N is a function of three parameters, but in certain spectra it is reduced to two or even one. This distribution depending on three parameters is a distinct characteristic of a band spectrum.—On the suppression of halos in photographic plates, by MM. Paul and Prosper Henry. *A propos* of a communication by M. Cornu (*Comptes rendus*, March 17), the authors note that in order to get rid of halos which occur around bright stars on an ordinary photographic plate they cover the backs of plates with collodion containing a small quantity of chrysoidine in solution.—Discharge of the two electricities by the action of ultra-violet light, by M. Edouard Branly. The author has obtained new results by using the induction spark as his source of light in place of the electric arc used by previous observers.—On phosphotrimetatungstic acid and its derived salts, note by M. E. Péchard.—On a nitroso-platinichloride, by M. M. Vèzes. By the action of an excess of hydrochloric acid on a concentrated solution of potassium platinonitrite, a body is obtained of the composition $PtCl_3(NO),2KCl$, analogous to but much less stable than the nitrosoruthenichloride, $RuCl_3(NO),2KCl$, described by M. Joly (*Comptes rendus*, t. cvii. p. 994). It is distinguished from the platinichloride under the microscope by its form and by its action on polarized light.—Glycollic nitrile and the direct synthesis of glycollic acid, by M. Louis Henry. The nitrile is formed by the addition of formic aldehyde to hydrocyanic acid, $HCOH + HCN = CN—CH_2OH$. The glycollic nitrile obtained is a very mobile, odourless, colourless liquid; its density at 12° is 1·100, it boils at 759 mm. pressure at 183° with partial decomposition. By hydrolysis with fuming hydrochloric acid, it yields glycollic acid, which may be separated as the calcium salt. This, in the opinion of the author, is the best method for the preparation of glycollic acid.

STOCKHOLM.

Royal Academy of Sciences, March 13.—On the International Zoological Congress in Paris in 1889, by Prof. F. A. Smitt.—A continuation of the Report of the Ornithological Committee, by Prof. F. A. Smitt.—On the results of the recent winter expedition for hydrographic researches in Skager Rack, by Prof. S. O. Pettersson.—Analytical deduction of the equations of the surfaces and lines which are invariants to the generalized substitution of Poincaré, and some geometrical properties of such invariant surfaces and lines, by F. de Brun.—On a special class of singular surfaces, by T. Fredholm.—On the solution of a system of linear resemblances between an infinite number of unknown quantities, by H. von Koch.—On a paper by H. Weber, entitled "Ein Beitrag zu Poincaré's Theorie der Fuchs'schen Functionen," by G. Cassel.—On the conform representation of a plane on a prism with some correlated problems, by the same.—Researches on mustard-oil-acetic acid and on thiohydantoin, by Prof. Klason.—Derivates of 1 : 3 dichlornaphthalin, by Prof. Cleve.—On the cyclic system of Ribaucour, by Prof. Bäcklund.—Contribution to the knowledge of the Ascomycetæ of Sweden, by C. Starbäck.—Determination of the optical rotation of some resinous derivates, by A. W. Svensson.—Studies on the influence of the irritation of the spinal chord and the nervus splanchnicus on the pressure of the blood with inductions of different frequency and intensity, by J. E. Johansson.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Evolution, Antiquity of Man, Bacteria, &c.: W. Durham (Edinburgh, Black).—Le Premier Etablissement des Néerlandais a Maurice: Prince Roland Bonaparte (Paris).—Le Glacier de l'Aletsch et le Lac de Märjelen: Prince Roland Bonaparte (Paris).—Pocket Meteorological Tables, 4th edition: G. J. Symons (Stanford).—The School Manual of Geology, 5th edition: A. J. Jukes Browne (Edinburgh, Black).—The Two Kinds of Truth: T. E. S. T. (Unwin).—The Art of Paper-making: A. Watt (Lockwood).—Catalogue of Books in the Library of the Indian Museum: R. L. Chapman (Calcutta).—Ueber die Liasischen Brachiopoden des Hierlatz bei Hallstatt: G. Geyer (Wien, Hölder).—Die Liburnische Stufe und deren Grenz-Horizonte. 1 Heft, Erste Abthg.: G. Stache (Wien, Hölder).—Advanced Physiography: J. Thornton (Longmans).—Ferrel's Convectional Theory of Tornadoes; Davis and Curry.—The Root-Knot Disease of the Peach, Orange, and other Plants in Florida (Washington).—The Fossil Butterflies of Florissant: S. H. Scudder (Washington).—The Photographic Quarterly, April (Hazell).—Journal of the Institution of Electrical Engineers, No. 85, vol. xix. (Spon).—Journal of the Chemical Society, April (Gurney and Jackson).—Société d'Encouragement, Paris, Annuaire 1890 (Paris).—Proceedings of the Academy of Natural Sciences, Philadelphia, Part 3, 1889 (Philadelphia).—Insect Life, vol. 2, Nos. 7, 8, 9 (Washington).—Journal of the Bombay Natural History Society, vol. 4, Nos. 3 and 4 (Bombay).—Ergebnisse der meteorologischen Beobachtungen, Jahrg. xi. (Hamburg).—Journal of Anatomy and Physiology, April (Williams and Norgate).—Jahrbuch der k.k. geologischen Reichsanstalt, Jahrg. 1889, 39 Band, 3 und 4 Heft (Wien, Hölder).

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THURSDAY, APRIL 24, 1890.

THE REVISED INSTRUCTIONS TO INSPECTORS.

LAST year it was a matter of considerable complaint against the Education Department that the Draft Code was presented to Parliament unaccompanied by the new instructions to inspectors, without which it could neither be satisfactorily interpreted nor adequately discussed. No such complaint can be made this year. The issue of the new Code, which promises to place elementary schools under what is practically a new system of regulations, has been followed within a few days by a revised edition of the instructions to inspectors, in which the changes are correspondingly large. Indeed, more than half of the document consists of new matter.

On the whole, the approbation which has greeted Mr. Kekewich's Code may be extended to the instructions by which it is explained. So far as we can see, there is no shuffling, no attempt to minimise or to alter the practical effect of the reforms which are conceded on paper in the Code.

The main alterations occur in those parts of the instructions which are to guide the inspector in awarding the Parliamentary grant under the new *régime*. It will be remembered that the system of payment on the results of individual examination disappears almost completely, and is replaced by a grant made up of three parts—a "principal grant" of 12*s.* 6*d.* or 14*s.*, a grant of 1*s.* 6*d.* or 2*s.* for discipline and organization, and a payment as before on results of examination in the so-called "additional subjects." The mode of examination to be adopted in future in the elementary subjects on which the "principal grant" depends is substantially that already in use for "class subjects." That is to say, there will be a collective examination by sample, a certain proportion of children out of each class being chosen at random for examination by the inspector, the teacher being always invited to add a few of his most forward scholars, so that the school may not be injured by any accident in the selection. Several alternative modes of selection are suggested, and the inspector is expressly asked to vary his method from time to time, rather than to adopt any uniform plan. Teachers and managers may hear the oral examination and see the papers, but they are to be warned that "it is not by studying past questions, nor by trying to forecast the kind of questions likely to be set hereafter, but by teaching the subject with good sense and thoroughness that the requirements of the Department will be best fulfilled, and the truest educational success achieved."

The higher "principal grant" is not to be awarded unless a high standard of proficiency is reached in all three elementary subjects. If the scholars do not reach the standard required for the lower "principal grant," the managers are to be warned that next year the grant may be discontinued; and, in all cases where the higher grant is not awarded, the points in which the school is deficient are to be clearly indicated to the managers.

These regulations, if wisely carried out, must be a great improvement on those under which the grant is at present assessed. The old barbarous system of bleeding a bad school to death by diminishing its grant below the minimum required for its efficient maintenance will be discontinued. In place of this a school, so long as it receives anything, will receive enough to enable it to be efficient if the teachers and managers are up to their work. If such a school fails to reach the required standard, though supplied with public aid on as liberal a scale as that on which multitudes of schools do contrive to be efficient, it will simply be removed from the list of grant-earning schools. This is the rational course, if carried out in practice, but very much will depend on the inspector. It is sincerely to be hoped that the instructions will be carried out in such a way as to ensure that the "liberal grant now offered to comparatively humble schools shall serve as an aid and stimulus to improvement, and not as a pretext for remaining content with a low standard of duty."

With the disappearance of payment on individual results in the elementary subjects, the necessity for many of the minute regulations as to the exact meaning of a "pass" in each subject disappears also. But the necessity still remains for the inspector to keep in mind the standard of an individual pass for such purposes as that of the scholar requiring a "labour pass" either for half-time or whole-time exemption.

A few modifications are made in the instructions respecting the three elementary subjects. The justice of the oft-repeated complaints which have been made of the excessive time devoted to English grammar is recognized, not only in the altered regulations for English, but in a great reduction in the "spelling" requirements. As regards reading, it is suggested that a class of older scholars should be set to read a passage to themselves while another class is being examined, and then be questioned as to its matter. Writing will be partly tested by examination of school copy-books, not merely by a piece of writing executed during the anxious and nervous hours of the inspector's visit.

But the most important changes bearing on the school curriculum—indeed, perhaps, on the whole, the most important changes in the whole document—are those passages in which an attempt is made to link the instruction of the school to the life of the home. On the one hand, the co-operation of the parents is to be expressly invited; on the other hand, their special wants are to be more directly consulted. For example, it is pointed out that "in some good schools the aid of the parents has been successfully enlisted, and they have been urged to hear their children read aloud from a newspaper or from a book for a few minutes at home in every day. The amount of oral practice which any one child can obtain in a large class is obviously insufficient; and a little home exercise in reading aloud is often found to have an excellent effect." On the other hand, the elder girls are to be allowed to bring from home garments that want mending, and to repair them in school under the teachers' supervision—an arrangement which will "connect the school-work usefully with the every-day life of the scholars." There are other hints to a similar effect, as in the concluding paragraphs of the instructions, which enumerate the ways in which, besides conforming to the requirements of the Code, a school may seek "to render

service to the children who attend it and to their parents." Taken one by one, the suggestions may seem unimportant; collectively, however, they indicate a policy of taking the parents frankly into confidence, and so, if possible, of establishing a new link of interest between the parent and the school, besides the mere "cash-nexus" of the school pence, which are destined so soon to disappear.

Under the head of "class subjects" an explanation is given of the object of the great changes in Schedule II., which, we learn, have been introduced in order to allow of greater freedom to teachers of different tastes and capacities, and to localities of different industries and requirements. "One good teacher of geography may attach special value to physical facts and phenomena; another who lives in a manufacturing or maritime town prefers to make commercial and industrial geography and the interchange of productions the leading features of his lessons." The same standard is, so far as possible, to be kept in view, in estimating the teaching of all the various alternative courses; but, subject to this one consideration, complete freedom of choice and treatment is to be given to teachers and managers. "In sanctioning any modification of the printed schemes it will be necessary to have regard to the experience and qualifications of the teacher, and to any special opportunities afforded in the town or district for instruction by a skilled demonstrator, who visits several schools in succession, or who gives collective lessons at suitable centres."

The instructions further confirm the view we expressed when commenting on the Code, that the policy of the Department will be to encourage class teaching at the expense of specific subjects. "Those managers and teachers who desire to continue the object-lessons of the infant school in due order through all the lower standards, and so to lead up to the regular study of natural history or physics in the higher, will probably think it better to treat science as a class subject than to postpone specific instruction until the fifth standard."

The recognition of continuity, and the idea of the school course as a connected whole, strikes us as a new and valuable feature in the instructions. From the infant school the child is to be led on through a series of object-lessons to the scientific class-teaching of the upper school, and thence in some cases to specific instruction in the higher standards. But all this is but the beginning. "Teachers should not be satisfied unless the instruction in specific subjects awakens in the scholar a desire for further knowledge, and makes him willing to avail himself of such opportunities as are afforded locally by a Science Class, a Polytechnic Institute, a course of University Extension Lectures, a Free Library, or a Home-Reading Circle." All this is a truism, it may be said; but it is unusual language for an official document, and carries us forward in imagination to the time, which must come sooner or later, when such fragmentary and scattered institutions as are here enumerated will take their proper place as parts of a great scheme of national education.

We fear that the realization of the aims of the Department may be materially impeded if a literal construction is to be placed on the clause providing that the same subject may not be taken both as a "class" and as a "specific" subject. Does this restriction merely mean that no child is to be presented in the same subject under both heads—an obviously reasonable stipulation—or that no children in a school may take as a specific subject any branch of study which is taken as a class subject by any other children in the school? If the latter is the case, we are informed that, in some cases at least, managers will find themselves seriously hampered.

Provision is made for the assistance of experts in the examination of scholars, in cases where the managers choose an "additional" subject with which neither the inspector nor his assistants are fully conversant. But unfortunately this assistance, which will be given by a colleague, on application to the chief inspector, will be confined to the framing of suitable questions, and marking the answers, and hence will be inapplicable to the case of oral examination, in which it is most wanted.

Those interested in manual instruction will turn with interest to the thirty-fifth section, which lays down the duties of inspectors with respect to this newly recognized branch of instruction. It explains that the difficulty which has hitherto prevented the recognition of manual instruction as part of the ordinary course of instruction in a public elementary school has been removed by the alteration in the terms of Art. 12 (*f*), though how such a change in Departmental regulations can alter the sense of an Act of Parliament we are left to conjecture. The instructions suggest such exercises as "modelling, the cutting, fixing, and inventing of paper patterns, the forming of geometrical solids in cardboard, and the use of tools and instruments," which are in use in some foreign schools, and are found to be "not without a useful reflex influence on all the ordinary school studies." The inspector is to report on the working of any system of manual instruction which may be adopted, though "no special grant is made *by this Department*." The words we have italicized clearly tend to confirm our impression as to the intention of the Science and Art Department to include manual instruction in their next Directory.

It is rather strange that under the head of "drawing" no reference is made to the change by which in future drawing will be made compulsory in boys' schools and optional in infant departments. It is true that drawing in ordinary schools will, as now, be paid for by the Science and Art Department, but power is given by the new Code to Her Majesty's Inspector to exempt schools from the necessity of taking the subject where the means of teaching it cannot be procured. We should like to know what standard the inspector will adopt in using this dispensing power. Will the standard be the same in all districts?

This is the question to which we return again and again after examining in detail the various changes in the Code and the instructions. All will depend on the inspectors. What will their action be? We agree on the whole in the praise accorded in the instructions to the "ability, discretion, and fairness with which Her Majesty's Inspectors discharge their arduous duties," but nevertheless, in particular cases, complaints of their action have not been wanting. The inspectors have hitherto been burdened with an amount of routine work which has to some extent hindered them from forming a really intelligent estimate of the value of the school work which they have to assess. This burden is now lightened, more visits may be paid without notice, and thus more intimate knowledge may be acquired of the real work of the school. "It will be

largely owing," we read, "to your influence if all who are concerned with the management of schools habitually regard the officers of this Department not merely as critics and examiners, but as advisers and helpers, in the performance of an important public work." That is the ideal to aim at, though there is a good deal of lee-way to make up before it is realized.

ORANGES IN INDIA.

The Cultivated Oranges and Lemons of India and Ceylon. By E. Bonavia, M.D. Pp. 384, with an Atlas of 259 Plates 7 inches long by 9 inches broad. (London: W. H. Allen, 1890.)

FOR twenty years past Dr. Bonavia has been distinguished in India as a horticulturist. He has been in charge of the Horticultural Gardens at Lucknow, where he has conducted many valuable experiments. Of late years he has tried oranges, and he has also collected information concerning oranges from various parts of India. India, taken as a whole, is very poorly supplied with fruit; really good mangoes and litchis are nearly everywhere dear, and remain in season but a short time. Oranges in several parts of India are cheap and excellent; improvement in their cultivation and extension in their circulation are matters of importance. The book of Dr. Bonavia contains his own experiences and notes, which are valuable. His second-hand information, which he has collected in the fashion of an Indian Secretary to Government or Minister of Agriculture, is of very small value, but is certainly superior to many secretarial compilations about hemp, jute, cotton, &c.

The first ninety pages treat of the various groups of oranges, lemons, limes, citrons, &c., with their sub-varieties; the next fifty pages treat of their cultivation in India; fifteen pages treat of their uses; eleven of the orange trade in India; twenty-one of the morphology of Citrus; forty of the origin of the Citrus and the derivation of its Indian names. Then follow 120 pages of appendix, containing a miscellaneous collection of "cuttings" relating in some way to the subjects in the book, with a translation of the chapters relating to Citrus in Rumphius's "Herbarium Amboinense." The greater part of this appendix appears of small importance; while Dr. Bonavia has by no means exhausted what first-rate authorities have written regarding oranges. The atlas of plates gives hardly anything but outline drawings of oranges and their leaves; a very small selection of these would have served every useful purpose.

Dr. Bonavia has summed up for us the conclusions of his book under seven heads (p. 245):—

(*a*) The pummelo (*Citrus decumana*, Willd.), is not specifically separable from the orange (*C. Aurantium*, Linn.).—This is a point of no possible importance, when naturalists know no line between a well-marked variety and a dubious "species"; but Lowe ("Fl. Madeira," p. 73) agrees with Dr. Bonavia.

(*b*) The sweet orange of Europe (*C. Aurantium*, Linn.) is a distinct race from the Mandarin orange (*C. nobilis*, Lour.).—This is correct, and well brought out by Dr. Bonavia; but it is also done very clearly by Lowe ("Fl. Madeira" [1857], pp. 73, 74).

(*c*) The India name "suntara," for *C. nobilis*, is not a corruption merely of Cintra.

(*d*) The European words "lime," "lemon," are probably derived from Malay words.

(*e*) Huge forms of Citrus fruit may have risen from a fusion of two ovaries [p. 187, "My view would require that the Citrus fruit should have originated in *two* whorls of carpels, the outer or *rind-whorl* and the inner or *pulp-whorl*"].

(*f*) The true lime (*C. acida*, Roxb.) has more probably descended from *C. hystrix*, Kurz, than from *C. medica*, Linn.

(*g*) The juice-vesicles of the Citrus pulp are probably homologous with the oil-cells of the rind and leaves, and perhaps with the ovules.

It will be best to reverently draw a veil over the conclusions (*e*) and (*g*) and over the whole chapter on morphology. And the other five "conclusions," except (*b*), do not conclude anything. The foregoing is Dr. Bonavia's own summary of what he has proved, but he has done more than he claims; his account of his own horticultural observations is of value, and his deductions very generally correct. Of these only a few can be given here.

(1) The *Khatta* or *Karna* orange of Upper India produces two kinds of fruit on the same tree and on the same branch, viz. (1) the regular crop, of smooth oranges, ripe at the end of the dry season, and (2) the after crop, of grossly-warted oranges, ripe at the beginning of the rains.

(2) The European orange (*C. Aurantium*) is only known in India as a cultivated foreign orange, and is not common. It has been probably introduced into India in modern times—possibly from the West.

(3) The *C. nobilis* is the sweet orange of India; it has been in India from ancient times, and is possibly indigenous on the north-east frontier. It has only been brought to Europe in modern times. The Tangerine orange is a small form of it. (This *C. nobilis* is a more slender tree than *C. Aurantium*; its oranges are depressed at the poles; the rind is very full of large oil-glands, and separates easily from the pulp, which lies more or less loosely in the rind as in a bag.)

(4) The pummelo (*i.e.* Pompel-moes) of India and Ceylon is in flavour, structure of carpels, colour of pulp, &c., very distinct from the Syrian shaddock, *i.e.* the shaddock of English fruit-shops.

(5) In the plains of Upper India (Delhi, Lucknow, &c.) the Indian orange (*C. nobilis*) can be successfully cultivated, but requires irrigation (well-water being better than canal-water), budding, trenching, shade, special preparation of the soil by lime or manure, &c.

Every page of Dr. Bonavia's book offers opportunity of comment: the remaining space here available is devoted to the practical subject of the Indian sweet orange, *C. nobilis*, which we shall call the "Mandarin," and, for shortness, state first our own beliefs concerning it.

There are (according to Dr. Bonavia) three great centres of cultivation of the "suntara" in India, viz. (1) Sylhet, *i.e.* South Khasia; (2) Central India; (3) Delhi and Oudh. From Khasia (*fide* Bonavia) about 4000 tons, worth £4 a ton, are exported to Bengal, mainly to Calcutta. From Central India about 800 tons go by rail

to Bombay. The export from Delhi is small. Besides this many stations have a few orange orchards for local consumption—"a mere nothing."

It is evident from this that Khasia is the most important orange centre, and unfortunately Dr. Bonavia has had to treat this part of the subject second-hand. He hardly says anything about the Central Indian cultivation, except the remark (p. 127), "I do not know what the composition is of the black soil of the Central Provinces." This soil, which produces such excellent Mandarins, everybody knows to be disintegrated trap, *i.e.* the same soil which alone produces them in Khasia.

Dr. Bonavia spends much space in attempting to show that the suntara orange is not a Mandarin; he maintains that the suntara and Mandarin are nearly allied, and together form the distinct race (or species) *C. nobilis*. He admits that people in Ceylon and elsewhere *will* call the suntara the Mandarin, but he strongly denies that the Mandarin is a suntara; he may as strongly deny that the greengage is a plum. The best Khasi oranges run very close on the true Mandarin. The *C. nobilis* now grows as if wild from the hills of Southern China, probably to Assam (Khasia); it is also scattered along the outer Himalaya of Sikkim and Nepal. The centre of this area is almost certainly its "origin." Dr. Bonavia speaks of the Butwal (south of Nepal) orange as the sweetest orange in India: he has not tasted from the tree the Khasi orange at the end of January, which is considered *too* sweet by many Europeans. The Khasi orange is in fact larger than the Butwal; and for a *sweet* orange there is no finer in India or elsewhere.

Dr. Bonavia lays stress on the fact that the true Mandarin is when dead ripe a "varnished green," while the suntara is "from orange-yellow to lobster-red"; he found that the green oranges of Ceylon in travelling to Etawah (21 days' journey) had turned or were turning yellow; and he decides triumphantly that "the *green* orange has no *locus standi*." The fact is otherwise: the best Khasi oranges when dead ripe on the tree are an intense "varnished green." Picked somewhat unripe, and carried in a native boat (21–30 days) to Calcutta, they arrive a dull yellow or turning yellow. And perhaps Dr. Bonavia could prove by prolonging the journey that their true colour is black. The withered, unripe-picked, dull yellow, mawkish, Calcutta orange is a very different thing from the orange ripe on the tree above Chela.

The Mandarin grows best in steaming valleys just within the hills (and above all on disintegrated trap) at an elevation of 250–2000 feet: here it grows from seed without any trouble. In the plains, the fruit is worse the farther you recede from the hills, and great pains must be taken with the culture. Dr. Bonavia was unfortunate in having to experiment with the orange at Lucknow; free-trade principles would suggest that the most promising plan would be to improve the communications between the orange districts and the great centres of consumption. It was not the fault, however, of Dr. Bonavia that he had to try to grow oranges where they naturally will not grow. But Dr. Bonavia does not seem, with all the extensive cuttings in his appendix, to have got from the literature the help in his task that he should have got. He hazards, for example, a speculation (p. 116) that "the stock on which the Mandarin is grafted may have some influence"; apparently unaware that the regular practice is to graft the Tangerine on the common orange, as it then becomes a larger tree giving a more certain crop of larger fruit.

Quite apart from the question of oranges, it is well worth while to examine in some detail the method of Dr. Bonavia in obtaining information about the Khasi orange and its results, because it throws a flood of light on Indian reports in general. Dr. Bonavia appears to have tried three sources of information, viz. (α) a description of the orange-groves by Mr. Brownlow, (β) the answers to his questions returned by the Deputy-Commissioner of Sylhet, (γ) similar answers from the Rev. Jerman Jones. Dr. Bonavia does not refer to the "Himalayan Journals" of Sir J. D. Hooker, vol. ii; nor to Medlicott in Mem. Geol. Survey Ind., vol. vii. Art. 3. From these two latter sources, a very fair idea of the circumstances of the orange-groves of Khasia can be gained. Dr. Bonavia appears not to have the wildest notion of the country, climate, or soil.

Turning to Medlicott's map, we see that there are three large valleys (Chela, Umwai, and Sobhar), at the south extremity of the Khasi Hills, which are occupied by the "Sylhet trap." This trap extends in the Chela valley from the debouchement of the river at Chela up to 2800 feet at the head below Mamloo. This trap decomposes into a reddish earth, and there occur soft ashy beds very like forms of the Deccan trap. All three valleys are excessively steep, the undecomposed trap standing in huge masses. The rain-fall varies from 300 to 500 inches per annum. These valleys are thus rough and broken, and full of precipices inaccessible but by ladders and ropes. Intensely hot and steamy, and protected from winds, they exhibited a richer vegetation to Sir Joseph Hooker than he had seen in the Himalaya.

In the Chela valley, at the present time, the Mandarin orange occupies the whole area of the trap. The two other valleys are less completely occupied. There is also an orange-grove on a small trap area a few miles east, behind Jynteapore.

The Khasi cultivation is simple. The pips of the orange are raised without difficulty in a damp seed-bed, often in a nook shaded by a boulder of trap. A piece of the jungle is half cleared (*i.e.* most of the larger trees, some of the smaller); and the young orange-trees, 3–5 feet high, are stuck out promiscuously in the partial shade left; the root of each is pushed if possible under the heel of a block of trap. When the young trees have got hold enough to bear the sun, the *other* half of the jungle is roughly cut. The trees require no further labour. The orange-groves in the cold weather form a monkeys' paradise, and it is necessary to destroy these. Sometimes two or three villages unite, enclose the monkeys, and drive them down to an angle of the main stream, where they are slaughtered pitilessly. The sight of a single monkey is always sufficient to exasperate a Tyrna man to fury.

The crop is enormous; the river at Chela flows sometimes covered apparently with oranges. Before the season is half over, the pigs are so surfeited that their oranges have to be peeled for them. The valley has enormously increased in wealth in the last half-century. It is a Khasi saying that a man here may work for three days and eat for a month.

Now let us see what Dr. Bonavia says. He has the specimen soil collected by Mr. Brownlow analyzed by a trustworthy chemist, who finds no lime in it. Dr. Bonavia argues (p. 94) "that either Mr. Brownlow took his sample from one particular spot, or did not reach the calcareous soil." "Orange wood requires considerable lime. In Chela oranges grow very well; therefore the soil of Chela contains lime. Moreover, it is incredible as the district exports lime that no lime detritus is ever washed down by the floods which flood the orange-groves of Chela to the depth of 6 feet."

Nothing can be wider of the mark. Mr. Brownlow would have had to go very deep into the Sylhet trap, a very hard rock, to get any lime. It is true that there is limestone at Mamloo, and that the water that comes down has some lime in it—but very little. The floods at Chela rise sometimes 60 feet (instead of 6), but they cannot inundate even then much of the orange groves which run up to 2000 feet. Perhaps tbe most extraordinary statement in Mr. Brownlow's description is that (above Chela) "no vacancies are left in the planting of the orange-trees." The trap boulders are as big as cottages all over the valley.

We turn to the second source of information—the Deputy-Commissioner of *Sylhet*. Fifty years ago "Khasia" was attached to Sylhet, and known as North Sylhet; and the oranges are still known as Sylhet oranges. Dr. Bonavia applies, therefore, to the Deputy-Commissioner not of the Khasi Hills, but of Sylhet. The Deputy-Commissioner cannot possibly leave his own Sylhet government and his own station; but, being a very amiable man, he sends Juggaish Babu, Deputy-Magistrate of Chunamgunj, to collect the information for Dr. Bonavia. This gentleman commences his report, "I met with the greatest difficulty in compiling these statistics. The Khasis received my inquiries with suspicion, and tried to mislead me as much as possible." The Khasis would doubtless be most hostile to a Bengali Babu from Sylhet. But a Bengali Babu is not exactly the man to collect scientific information anywhere. Juggaish Babu commences, "The soil must be sandy." "The gardens being situated on river-sides, their soil naturally retains some moisture even in the dry season. Hence, perhaps, artificial irrigation becomes unnecessary." How the idea of the possibility of artificially irrigating the Chela valley can have occurred to the Babu's mind is marvellous; unless his report is in reply to some leading question by Dr. Bonavia.

"The garden is never hoed or harrowed before receiving the orange plants." It would not be possible to harrow such a country at any season. The Babu finally speaks of the land tenure. He does not mention the fact that Chela and its 12 associated villages form a republic under the protection of the English Government; their administrative Government consists of 4 councillors elected for four years by universal manhood suffrage. This constitution was established half a century ago by a Bengal civilian, and is unique.

We now turn to the third source of information to Dr. Bonavia, viz. the Rev. Jerman Jones, a missionary who has been in Khasia more than 25 years, and could have told much. But he appears only to have been consulted about the *names* of oranges in Khasi, and he replied that the name (for the Khasi Mandarin) is *U soh niam-tra*; which Dr. Bonavia writes *Usoh niamtra*; and states (p. 228) that *Usoh* is the generic Khasi name for oranges. [In a footnote, backed up by an appendix, No. 43, Dr. Bonavia carefully and amusingly notes that the word he got from the Deputy-Commissioner of *Sylhet* was *santra*, not *niamtra*. Dr. Bonavia evidently thinks the testimony of a missionary doubtful as against that of a Deputy-Commissioner. But the excellent Deputy-Commissioner in question has an extremely limited knowledge of Khasi, and would certainly not set himself up against Mr. Jerman Jones.]

Dr. Bonavia having got the word *usoh* for orange in Khasi, goes on to connect it with the Amboina words *aussi* and *ussi*. He proceeds (in tracing the origin of the Mandarin), p. 229:—

"We have here, I think, something tangible to go by. The community of the generic name *usoh*, *ussi*, or *usse* to the Khasi Hills and the Malay Archipelago indicates, &c., &c."

In Appendix No. 58, the affinity of *usoh* is pushed further with the aid of Prof. Dr. T. de Lacouperie.

Now we come to the smash of the whole. *Soh* means "fruit" in Khasi, as see Hooker, "Himalayan Journal," vol. ii. p. 268, in note; in which language every noun *must* have the article prefixed, and *soh* being masculine, takes the masculine article U. Throughout Khasia, *usoh* so far from being the generic term for orange, would be understood to be *potatoes*. It is probable that, *at Chela*, if an Englishman pointed at a basket of oranges and said "*usoh*," they would guess which fruit he meant; but it is not Khasi. (Not the least curiosity in this book is that Mr. Jerman Jones should say that he had never found a Khasi who could offer the remotest suggestion as to the derivation or meaning of *niam-tra*. *Some* Khasis have an explanation; it might be worth Dr. Bonavia's while to ask Mr. Stevens of Chela, or Mr. Roberts of Nongsowlia, about it before publishing the corrected edition.)

The sum of the matter is that, if Dr. Bonavia had confined his book to his own observations and his own part of the country, with half a dozen plates showing properly the main types of Indian oranges, it would have been a handy inexpensive book of 200 pages at most. But, unfortunately, in Indian style, Dr. Bonavia's ambition has been to include all India in his book, to put forward his own extremely peculiar views of morphology, and to revel in linguistic and ethnological speculations, some of which are absolutely bad, and many of which can be but of little use. On top of the book thus weighted come the 120 pages of appendix, with the final result that the work bears a painful resemblance to the ordinary Secretarial Report, though it possesses really an amount of original observation and experience which such Reports often entirely want.

In one respect, Dr. Bonavia hardly comes up to the Secretarial Report: he spells, on one page, Shalla, Mhowmloo, Mostock, though those words were correctly spelt Chela, Mamloo, Mousto, as long ago as 1854 by Sir J. D. Hooker; or Dr. Bonavia might have referred to the fine map of the district by Godwin-Austen. Similarly, Dr. Bonavia states (p. 30), "The Bengalis have no *v* in their language." It is true that in vulgar

Bengali the *v* is often degraded into *b*—a linguistic change that runs from Hebrew to Spanish. But Dr. Bonavia might as well maintain there is no *h* in English because a Cockney pine-grower "eats is ouses by ot water."

Turning lastly to the question how far Dr. Bonavia's book assists the cultivation of the orange in India, we may doubt, with every admission of his horticultural skill and assiduity, whether he is on the right tack. The Khasi Mandarin can be grown almost without labour, and of a quality that is not likely to be approached by any horticultural skill and labour on non-volcanic soil in the plains. These oranges are now picked unripe, and occupy a month (often more) in reaching Calcutta in a native boat. A fruit-steamer would take them down in 2 or 3 days from Chattuck to the rail at Goalundo. Bombay would surely take many more oranges from Nagpore if the railway rates were lowered, and the "perishable fruit" accelerated in transit.

Mr. Medlicott made only a hurried march across the Khasi Hills when he laid in his three patches of Sylhet trap, and he only visited a very narrow strip of country. More of this trap certainly exists—perhaps at a low level, suitable for oranges; and the Government Geologist at Shillong might, in the cold weather, possibly discover some more patches. For the present, however, the known area of Sylhet trap is by no means nearly covered with oranges, except in the Chela valley, where the boundary of the orange-groves coincides very closely with the outcrop-line of the trap. C. B. CLARKE.

A NATURALIST AMONG THE HEAD-HUNTERS.

A Naturalist among the Head-hunters. Being an Account of Three Visits to the Solomon Islands in the years 1886, 1887, and 1888. By Charles Morris Woodford, F.R.G.S., &c. (London: George Philip and Son 1890.)

TILL within the last twenty years the Solomon Islands were almost unknown to Europeans, and their inhabitants were considered to be exceptionally uncivilized and treacherous. Whatever they may have been originally, they were not likely to be improved by their first contact with civilization, in the form of chance visits of whalers and vessels engaged in the "labour trade"—which in its early days meant kidnapping and slavery, often leading to murder or to wholesale massacres. With such experiences of the resources of civilization we are not surprised to hear from Mr. Woodford that they are "suspicious of strangers," or that they are "treacherous when they see their opportunity"; yet the fact that he lived among them for several months, often quite alone and unprotected, and that Mr. Lars Nielsen, a trader, lived on good terms with them for ten years, leads us to suppose that, under more favourable circumstances, their character might have been found to compare not unfavourably with that of the Fijians. There is now, however, no chance for them, as they are certainly doomed to speedy extinction. The numerous distinct tribes found on each of the islands live in a state of chronic warfare, incited by the ordinary causes of the quarrels of savages, intensified by a general mania for head-hunting and in some cases by the habit of cannibalism. So long as they fought with native weapons, spears and wooden clubs, the destruction of life was not very great; but the traders have armed them all with Snider rifles and steel tomahawks, the result being that entire villages and tribes are sometimes massacred; and this wholesale destruction, aided by infanticide and other causes, is leading to a steady decrease of the population.

The excellent reproductions of photographs with which the book is illustrated show that the Solomon islanders are typical Papuans, hardly distinguishable physically from those of the western and central portions of New Guinea. Their state of civilization appears to be about the same. They cultivate the ground assiduously, growing chiefly yams, taro, and plantains, and they even terrace whole hill-sides for the taro, a stream of water being admitted at the top, and conducted down from level to level with considerable ingenuity. As domestic animals they keep dogs, pigs, and fowls, and they had all these animals when first visited by the Spaniards in 1568. The dog Mr. Woodford believes to be the dingo of Australia; the pig the *Sus papuensis* of New Guinea; while the fowl was no doubt derived from the Malays. They build excellent canoes, fifty or sixty feet long, of planks hewn out of solid trunks, beautifully fitted together and fastened with rattan. Their houses are fairly built and comfortable; and they construct baskets, shields, wooden bowls, and various weapons and ornaments, with the usual savage ingenuity.

Mr. Woodford's chief occupation in the islands was the collection of specimens of natural history, and his account of the zoology of the group presents several points of interest. It is here we find the eastern limit of the marsupials, which are represented by a species of Phalanger hardly distinguishable from one inhabiting New Guinea. Bats are numerous, seventeen species being described, of which six are peculiar; and there are four species of native rats, one of which is the largest species known. About the two large rats, *Mus imperator* and *Mus rex*, Mr. Oldfield Thomas, who described them, makes the following interesting remarks:—

"It is, however, in their relation to each other that their chief interest lies, for they seem to be clearly the slightly modified descendants of one single species that, once introduced, has been isolated in Guadalcanar for some considerable time, while it has apparently died out elsewhere. Of this original species, some individuals would have adopted a terrestrial and others an arboreal life, and their respective descendants would have been modified accordingly. In this way I would explain the fact that at the present time we have in Guadalcanar two genuine species, agreeing with each other in their essential structure, and yet separated by a considerable number of characters, all having a more or less direct relation to a climbing or non-climbing habit of life. Of these, of course, by far the most striking are the broad foot-pads and the long rasp-like probably semi-prehensile tail of *Mus rex* as compared with the smaller pads and short smooth tail of *Mus imperator*."

This description well illustrates the fact of the importance of insular faunas as showing us how species may be modified under the least complex and therefore most easily understood conditions. On a continent the modification to an arboreal mode of life would have brought the species into competition with a number of other arboreal organisms, and would have exposed it to the attacks of a distinct

set of enemies, requiring numerous modifications of form, structure, and habits, the exact purpose of which we should have found it difficult to interpret. But here, where both competitors and enemies are at a minimum, we are able distinctly to see the few and simple modifications which have adapted the species to its changed mode of life. We have here, too, a case in which the isolation supposed to be essential in the production of new species has been effected solely by a change of habits within the same limited area, and it is evident that this mode of isolation would be equally effective in the case of a continental as of an insular species.

Lizards, snakes, and frogs are tolerably abundant, and the proportion of species peculiar to the islands is in the order in which they are here named; and this also indicates the increasing difficulty of transmission across an ocean barrier. Birds seem to be fairly abundant, parrots and pigeons forming the most conspicuous groups, while birds of paradise appear to be absent. Although insects decrease in number of species as we go eastward from New Guinea, yet two of the grandest of butterflies—*Ornithoptera Urvilleana* and *O. Victoria*—are found in the Solomon Islands, and were among the greatest treasures of Mr. Woodford's collections. The latter species was only known by a female specimen obtained by Macgillivray, the naturalist to the *Herald*, in 1854, till Mr. Woodford again found it in 1886, and discovered also the beautiful green and black male. Many fine Papilios are also found, among them a splendid blue and black species allied to the well-known *P. Ulysses* of the Moluccas. Here, as elsewhere in the tropics, some striking cases of mimicry occur, three species of Euplæa being so closely imitated by three species of Diadema, as to be undistinguishable on the wing; and each pair appeared to be confined to a separate island.

The following is an interesting observation on the habits of pigeons:—

"The small islands on the reefs are much frequented by pigeons. They resort to them during the day, but mostly towards sunset, when, at some islands that I know of, the pigeons may be seen arriving by twos and threes, or in flocks of ten or a dozen each, to roost on the islands, until each tree is crowded with birds. The only reason that I can assign for this habit is, that on these small islands the pigeons are freer from the attacks of the large monitor lizards that abound on all the large islands. I do not consider this at all a satisfactory reason, but it is the only one I am able to suggest. Certain it is that this habit of the pigeons plays an important part in the distribution of seeds from island to island. On any of these small islands the large seeds of the Canarium nut tree may be found, after being disgorged by the pigeons, while young trees in different stages of growth may often be seen."

Mr. Woodford's explanation of the pigeons' roosting on the small islands appears to be a highly probable one, and quite in accordance with other facts relating to this tribe of birds. They are exceptionally abundant in tropical archipelagoes, and most so in those where, as in the Antilles, the Mascarene group, the Moluccas, and the Pacific islands, arboreal carnivorous mammals are very scarce or altogether wanting. An analogous fact to that noted by Mr. Woodford is, that although the beautiful Nicobar pigeon has an enormous range, from the Nicobar Islands to New Guinea, it is almost unknown in the larger islands, especially in the western half of its area where mammals abound, but is more especially confined to the smaller islets and reefs, where it is comparatively free from enemies.[1]

Although the natives of the Solomon Islands are well supplied with Bryant and May's wax vestas in metal boxes—the only kind of matches that can be kept in the damp atmosphere—they still make fire in the native way, by friction, on certain ceremonial occasions, or at other times when matches are not forthcoming; and their method of proceeding is well described by Mr. Woodford. It consists in rubbing a hard piece of wood in a groove formed on a soft dry piece—the method used in the Moluccas and Australia—and he tells us that, though a native will usually produce fire in less than a minute, he has himself rubbed till his elbows and shoulders have ached without ever producing more than smoke.

The following extract gives a fair idea of the author's style:—

"It is amusing to see a mere child paddle alongside in a crazy trough of a canoe, only just capable of supporting its weight. The water splashes into the canoe at every stroke of the paddle, and at intervals the small child kicks it overboard with its foot—a novel kind of baler. Three or four mouldy-looking yams, ostentatiously displayed, are rolling about in the water at the bottom of the canoe. The unsuspecting stranger takes pity on the tender years, and apparent anxiety of the small native to trade, and gives him probably four times the proper price for his rusty yams. The child eagerly seizes the coveted stick of tobacco, and immediately stows it for safety through a hole in his ear, where at least it will be in no danger of getting wet. He next whisks aside a dirty-looking piece of matting that has apparently got accidentally jammed in one end of the canoe, and displays some more yams, of a slightly better quality than the last. For the sake of consistency you cannot well offer him less than you did before, and another stick of tobacco changes hands, and is transferred to the other ear. You think now that he must have finished, as there is no place in the canoe to hide anything else, but with a dexterous jerk that nearly upsets the canoe he produces a single yam that he has been sitting upon. How it managed to escape notice before is a puzzle. For this he demands a pipe, but is not satisfied with the first or second that is shown him. No; he must have a *piala tinoni* or have his yam back. The *piala tinoni* is a pipe with a man's face upon the bowl. But again the young trader is particular, it must also have a knob at the bottom or he will have none of it."

The book is well got up, well illustrated, and very pleasantly written. It is full of information as regards the natives, the scenery, and the natural history of these little-known but very interesting islands, and can therefore be confidently recommended to all who care for books of travel in little-known countries.

A. R. W.

OUR BOOK SHELF.

Recherches sur les Tremblements de Terre. By Jules Girard. (Paris: Ernest Leroux, 1890.)

THE scientific study of earthquake phenomena has of late years made great progress, and we are glad to welcome a book which brings together the new matter

[1] See "The Malay Archipelago," p. 350.

which has hitherto been published only in various Journals and Transactions of Societies. The book commences with a chapter on ancient traditions, giving a chronological table of the more important shocks which have occurred since 79 A.D. The second chapter briefly discusses the connection between earthquakes and volcanoes, a subject of which we have apparently a good deal still to learn. Then follow descriptions and illustrations of various seismometers and seismographs, including the latest forms devised by Profs. Gray and Milne. In this chapter there are given several interesting comparisons of earthquake curves automatically recorded by the instruments, and curves artificially produced by the application of forces of known direction and magnitude. The propagation of shocks through land and water, and their destructive effects, are also considered, the latter being illustrated by sketches of some of the more remarkable fractures and displacements which have been observed. The last chapter summarises the suggestions which have been made as to possible connections between earthquakes and astronomical and meteorological phenomena. In conclusion, M. Girard points out the necessity for continued systematic observations, and enumerates the chief points on which further information is required.

To those who know little or nothing of the subject, M. Girard's little book will form an admirable introduction; and to the initiated it will be a handy book of reference to its latest developments.

La Photographie à la Lumière du Magnésium. By Dr. J. M. Eder. Translated by Henry Gauthier-Villars. (Paris: Gauthier-Villars and Son, 1890.)

THIS is a translation of a very interesting little German work on the employment of magnesium light for the purposes of photography, and will form a useful addition to our photographic literature. The author first gives a brief account of the earlier stages of the subject, taking us back to the time when Bunsen and Roscoe, in the year 1859, indicated the considerable advantages the light of magnesium presented for photo-chemical studies and lighting. He then shows how Crookes afterwards employed the light for photographic purposes.

Amongst the very first attempts of artificial lighting, the wire of magnesium was used. It was burnt in a specially-made lamp, and the light thus produced answered fairly well for interiors, but was useless for portrait work, being too harsh. The next advance was the employment of a mixture consisting of the powder of magnesium, chlorate of potassium, and a sulphide of antimony; the light was produced by igniting the mixture, which flared up instantaneously. The chief drawback to this method was the great precaution that had to be taken during the mixing, as the slightest blow caused an explosion. Saltpetre in place of potassium was sometimes used so as to lessen the chances of explosion.

The methods described in chapters v. and vi. were those which gave the best results. They consisted in blowing powdered magnesium through a tube and allowing this powder to come out at the other extremity into a gas or candle flame; the light thus produced was extremely actinic, and did not present any danger. The lamps of Schirm and Loehr, illustrations of which are given in these chapters, were on this principle, and gave great satisfaction for portraiture, being worked by means of a pneumatic india-rubber ball. Chapter vii. treats of the combustion of magnesium in oxygen, and in it is described Piffard's apparatus for the production of this light, which was found to be enormously increased by the presence of the oxygen. The remaining chapters deal with methods of taking groups by this artificial light; and there is a very interesting illustration of the pupil of the human eye, photographed in a dark room by means of the flash light, the exposure of which was so short that the pupil had no time to contract. The book concludes with some hints on the precaution necessary to insure successful development of the negatives taken by these processes, and with a short appendix by M. Alexandre.

LETTERS TO THE EDITOR.

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Panmixia.

BUT for his statement that I "cannot be sincere," I should not have deemed it necessary again to answer Prof. Lankester; anyone who is read in the literature of Darwinism must already have perceived that a further reply on my part is needless. An accusation of insincerity, however, ought not to pass unnoticed; and therefore I will ask your more general readers to observe the ground on which it has been made.

In my answer to his original criticism I endeavoured to show that Prof. Lankester "fails to distinguish between the cessation and the reversal of selection," or, more particularly, between panmixia and the economy of growth; and this is the point with regard to which insincerity is charged. Yet this is just the point—and the only point—in dispute. I have always represented that the cessation of selection is *per se* a cause of degeneration, whether or not it be associated with the economy of growth. Prof. Lankester, on the other hand, represented that the cessation of selection is not *per se* a cause of degeneration; but merely a "state," which is precedent to, and contemporaneous with, the economy of growth—the latter being the cause, while the former is but a condition to the occurrence of this cause. Such, at any rate, appeared to me the only meaning that could be gathered from his paragraph at the top of p. 488; and it is now over and over again repeated in his last letter. For instance:—"Cessation of selection *must be supplemented by economy of growth* in order to produce the results attributed to 'panmixia.' And inasmuch as economy of growth as a *cause* of degeneration involves the *condition* of cessation of selection, Mr. Darwin *in recognizing the one recognized the other*. . . . It is true that Mr. Darwin did not recognize that such unrestricted variation must lead to a diminution in size of the varying part *without the operation of the principle of 'economy of growth.'* This was no strange oversight: *he would have been in error had he done so*. . . . The term ['panmixia'], like its correlative 'cessation of selection,' *does not indicate a principle, but a natural condition:* it does not involve the inference that a dwindling in the size of the organ must result from inter-breeding; but *simply points to a precedent condition*" (p. 559: italics mine).[1]

Where, then, is the insincerity in saying that Prof. Lankester does not perceive the distinction between the cessation of selection and the economy of growth as two totally different causal "principles"? Or what remains for me but to repeat, with all sincerity, "he confounds the '*idea*' of panmixia with that of the economy of growth," and "fails to perceive the '*essence* of the idea' in the all-important distinction between selection as withdrawn and selection as reversed"?

It is true that at the close of his last letter Prof. Lankester admits, "when we consider shape and structure, and not merely size, it is clear that panmixia without economy of growth would lead to a complete loss of that complex adjustment of parts which many organs exhibit, and consequently to degeneration without loss of bulk." But how was it possible to surmise from his first letter that he had in his mind such reservations as to "shape" and "structure?" Or, indeed, how is it possible to reconcile such reservations with the passages above quoted from his last letter, to the effect that the cessation of selection is "not a principle at all," but merely "a condition which alone cannot produce any important result"? Are we to conclude that in Prof. Lankester's opinion neither "a complete loss of complex

[1] I may remark that the term "cessation of selection" is not the "correlative," but the *synonym* of the term "panmixia." And I may further remark that the term "reversal of selection" is not, as Prof. Lankester supposes, the synonym of the term "economy of growth." Economy of growth, where useless structures are concerned, may determine a reversal of selection; but the reversal of selection may also be determined by many other causes and conditions, which are equally potent—or even very much more potent—in this respect.

adjustment," nor any amount of change as to "shape," deserves to be regarded as "any important result"? Must we not rather conclude that when he first wrote upon "the state of panmixia," he had not sufficiently considered the subject; and, in now endeavouring to trim, ends by contradicting himself?

The only issue being as to whether panmixia is itself a cause, or merely the precedent condition to the occurrence of a totally different cause, nothing more remains to be said. As a result of his further consideration, Prof. Lankester now admits "it is clear" that, "without economy of growth," panmixia *is* a cause of degeneration where "shape" and "structure" are concerned. And, when he considers the matter a little more, he will doubtless perceive the contradiction in saying that, where degeneration as to "size" is concerned, "it is absurd to attribute the result, or any proportion of it, to the panmixia or cessation of selection alone." Variations round an average mean occur in "size" or "bulk," just as they do in "shape" and "structure": therefore, if on this account panmixia is conceded to be a true cause of degeneration as regards the latter, it must likewise be so as regards the former. The fact that in the former case—as I showed in 1874—it must always be more or less associated with the economy of growth, is no proof that it then loses its due "proportion" of causal agency; while, with the now single exception of Prof. Lankester, everyone who has since written upon this "principle" takes the same view as I did—viz. that the phenomena of "dwindling" in our own domesticated animals furnish as good evidence of the operation of panmixia as is furnished by the other forms of degeneration to which he now alludes. Therefore, if he really believes it is in this case "absurd to attribute the result, or any proportion of it, to the panmixia," he becomes opposed, not only to me, but to Galton, to Weismann, to Poulton, and to everybody else who has ever considered the subject. In short, it is now a matter of general recognition that what he calls my "unreal separation between 'cessation of selection' and 'reversal of selection,'" is a separation so fundamentally real, that it is the means—and the only means—of abolishing the evidence of Lamarckian factors where this once appeared to be most conclusive; seeing that "with highly-fed domesticated animals *there seems to be no economy of growth, nor any tendency to the elimination of superfluous details*."[1]

April 19. GEORGE J. ROMANES.

IN NATURE of April 3 (p. 511) Mr. Herbert Spencer suggests an interesting subject for discussion on the effects of use and disuse of organs, asking for an explanation on the theory of panmixia of the well-known tendency of domesticated animals to droop the ears. Many of the ruminants in a wild state have their ears set on horizontally with an inclination to droop; for instance, the gnu, sable, antelope, zebu, gaur (Central India), Cape buffalo, &c. The American bison has completely drooping ears; there is also at the Natural History Museum, South Kensington, in Case 57, a specimen of a smooth-haired sheep from Turkey in Asia, *Ovis aries*, which has dependent ears. Pathologically, though as yet not physiologically proved, the discussion of the transmission of acquired characters possesses a deep interest.

Evolution seems impossible without variation, and until the latter can be explained on other grounds than those of the inheritance of accumulated minute changes in character acquired through ages of slowly varying climate and conditions of life, preserved by natural selection, this transmission would seem a reasonable conclusion so long as the characters acquired were of service to the inheritor in the struggle for existence.

Though Weismann disbelieves most of the evidence Darwin collected on heredity, and doubts the possibility of the communication of external influences by the somatic cells to the germ cell, he suggests no other hypothesis to account for the phenomena of change, beyond the vague expression "predisposition of the germ-plasm." R. HAIG THOMAS.

April 5.

[1] Darwin, "Variation, &c.," ii. p. 289. Seeing the importance of "the idea of panmixia" in this connection, I must still be permitted to regard it as "unfortunate" that it was not present to Mr. Darwin's mind before the publication of his last edition of the "Origin of Species." But this does not mean, as Prof. Lankester "affects to suppose," that I regard the unfortunate nature of such a circumstance as due to the fact that I happened to be the first who perceived it. One can only assign so petty a form of "*badinage*" to the same argumentative level as "pointing out the oversight" that in my first letter I "omitted to credit Mr. Darwin with the recognition of the economy of growth." Prof. Lankester has committed about as grave an oversight in his own letter, by omitting to credit Mr. Darwin with the recognition of natural selection.

The "Rollers" of Ascension and St. Helena.

YOU probably know that the United States Scientific Expedition under Prof. Todd has had occasion to stop here during the past two weeks. I have resided during this time continuously at the signal station on Cross Hill (altitude 870 feet), studying the clouds and winds with many important results. I have had an excellent opportunity to observe the "rollers" for which Ascension and St. Helena are famous, and I have been able to demonstrate convincingly to myself their nature and origin. I should be obliged to anyone who will tell me whether my following views have perhaps been arrived at by previous observers.

The south-east trade blows with very various intensities over different parts of the South Atlantic, and the regions of light trade, no trade, fresh and strong trade, vary from day to day, as shown by comparing the logs of vessels. A limited region of strong south-east trade is a region whence spreads in all directions the corresponding strong south-east swell of the ocean surface—very distant storm winds or very near regions of high south-east winds produce similar results on the ocean swell: the locality of these winds will determine whether any point shall be experiencing a light or heavy swell. What causes the variations in the south-east trades, and in what direction the regions of strong trade move, are questions for further study. My present data would show that these latter regions move against the trade winds, *i.e.* from Ascension towards St. Helena, but there need be no uniformity in this respect.

Now if a south-east swell surrounds such an island as Ascension it is not directly felt on the lee side, but the long rectilinear swells, that advance faster in deep than in shoal water, are seen from my elevated station to assume the new curved shapes that result from the retardations on the shoals. So that finally in typical cases we have off the lee of the island a series of crossing and interfering swells producing at one point a quiet spot, at the next a double swell and great breakers.

The rollers are a magnificent example of deflection by shoals, and of interference and of composition of waves. Their severity at St. Helena and Ascension is apparently due to the proportions of the dimensions of the swell to that of the islands, just as in the interference phenomena of sound and light everything depends on the size of obstacle and length of wave. I have a number of measures that will, I hope, enable me in the future to give more accurate details, but for the present I can only inquire as to the bibliography of the subject. The correct explanation of the rollers, and of the swell on the West African coast, will undoubtedly lead us to further steps in marine meteorology. CLEVELAND ABBE.

U.S.S. *Pensacola*, Ascension, April 2.

Self-Colonization of the Coco-nut Palm.

WITH reference to Mr. Hemsley's note on this subject to NATURE (p. 537), I regret to have to inform him that the two young palms found on Falcon Island were placed there by a Tongan chief of Namuka, who, in 1887, had the curiosity to visit the newly-born island, and took some coco-nuts with him. This information I received from Commander Oldham, who had been much interested at finding these sprouting nuts at some 12 feet above sea-level and well in from the shore of the island, but who found out the unexpected facts in time to save me from making a speculation somewhat similar to Mr. Hemsley's.

W. J. L. WHARTON.

Nessler's Ammonia Test as a Micro-chemical Reagent for Tannin.

IN most cases the presence of tannin is immediately shown by all the ordinary reagents used by the botanist for its discovery. This does not happen sometimes, however; as, for instance, in the tannin-cells found in the epidermis on the dorsal side of the leaves of some plants. As a good typical example the common primrose may be cited. Of all the ordinary tests, including iron salts, potassium bichromate, Moll's test (copper acetate and iron acetate), ammonium molybdate, and osmic acid in 1 per cent. solution, the latter alone acts immediately upon the tannin in the primrose leaf's epidermis. It may hence be worth while recording the discovery of a second reagent capable of acting rapidly and effectively; and one which is easily made and will keep for some time should be especially valuable. Such a reagent is Nessler's test for ammonia.

Nessler's test is made, as all the world knows, by saturating a solution of potassium iodide with mercuric iodide, and adding an excess of caustic potash. Ammonia gives with this a reddish precipitate; tannin a brown, and when in considerable quantity a deep black one; but if little tannin be present, the brown may tend towards purple. It goes without saying that much experiment must be undertaken before one can be sure of the substance giving the brown precipitate being really tannin. To be conclusive, such experiment should be carried out in four different directions:—

(1) The reaction ought to be given in all cases when the ordinary reagents make their presence immediately felt.

(2) Cells which will not immediately give the tannin reaction with ordinary tests, but which will do so with Nessler's test, must also do so under the former conditions if time be allowed.

(3) Tissues which will not yield the reaction with Nessler's test, must not give it with any other reagent even after the lapse of some time.

(4) Solutions of tannin must give a brown precipitate with Nessler's test.

Under the first of these headings may be mentioned growing shoots of the garden rose. On laying a radial longitudinal or a tangential section of this in Nessler's fluid a copious black-brown precipitate is obtained, and the same thing occurs with the beautiful tannin-sacs of *Musa sapientum*. In all other instances where tannin has betrayed its presence by the use of ordinary reagents, the brown colour has been obtained upon treatment with Nessler's test.

The primrose leaf may be again cited as an example of the time sometimes necessary to show up tannin with the usual reagents, of which it must here suffice to particularize ammonium molybdate. On laying in the molybdate a small piece of epidermis torn off the lower side of the leaf, one first sees a cell here and there coloured the characteristic and beautiful yellow given by this test: these coloured cells are usually situated among the elongated more or less rectangular cells overlying the vascular bundles. Re-examination after half an hour or so shows several more of the cells similarly coloured, but it is usually not till after a couple of hours that one can safely declare all the tannin-containing cells to have been stained. With variations in respect of time, and with the sole exception of osmic acid, all the other tests act in precisely the same way; even Möll's, preferred to all others by some of our Continental *confrères*, being as unsatisfactory as the rest. But sooner or later its characteristic colour is imparted to these cells by every reagent, thus proving tannin to be present.

For the negative experiment—the absence of the brown colour from tissues treated with Nessler's fluid, and its absence from the same tissues when acted upon by ordinary tannin reagents—recourse was again had to epidermis. The experiment succeeded in all cases: among these may be cited *Fatsia japonica*, wallflower, box, *Stellaria media*, and *Pelargonium zonale*. In none of these did tannin show up, although twenty-four hours were allowed to elapse before the preparations were destroyed.

Lastly, Nessler's fluid gives a rich brown precipitate with solutions of tannin. Moreover, with gallic acid a grey-green one is thrown down, thus affording an easy means of distinguishing between these bodies.

For these reasons, therefore, viz. the rapidity, certainty, and distinctness of its action; the ease with which it can be made; its permanence when made; and lastly, the difference in its behaviour towards tannin and towards gallic acid—for these reasons I am bold enough to anticipate the time when, to adapt a hackneyed expression, Nessler's fluid will be regarded as a reagent which no botanical laboratory should be without.

SPENCER MOORE.

The Moon in London.

SOME years ago a weekly paper represented a young rustic asking his mother, "Be that the same moon they have up to Lunnon?" to which question the mother evasively replied, "You leave the moon alone and go to bed." The boy was satisfied by retorting, "I baint a touching on it." But his question is this month brought once more to the front by the following passage, which will be found in one of our most important monthly magazines. "But if," says the writer, "there is an abuse of the deductive method of reasoning, there is also an abuse of the inductive method. One who refused to believe that a new moon would in a month become full, and, disregarding observations accumulated throughout the past, insisted on watching the successive phases before he was convinced, would be considered inductive in an irrational degree." We cannot, of course, presume to dictate to or for the moon "up to Lunnon," but here in the country the new moon becomes full in half a month, and we have convinced ourselves by watching the successive phases that a new moon will in a month become a new moon again. Nevertheless we willingly admit that life is far too short and too encumbered to allow of any man's repeating more than a small fraction of the accumulated observations on which his scientific beliefs are founded. Yet, on the other hand, taking things for granted is probably the source of nine-tenths of the errors that fill our minds, while the men of genius seem to be just those who know best what and how to observe for themselves, and how much to trust in the observations of others.

T. R. R. STEBBING.

Tunbridge Wells.

Foreign Substances attached to Crabs.

THERE is, of course, no analogy between whiffing for mackerel with red flannel, and fishing for cod on the bottom with any kind of bait.

If Actinians are offensive to fish, it is a singular fact that, when a cod-line is baited with mussels, herring, sand-eels, and anemones (viz. *T. crassicornis* and *A. mesembryanthemum*), the latter prove by far the most successful baits.

Impalement on a hook by no means kills an anemone, whose powers of offence are, perhaps, little lessened thereby; and under natural conditions the tentacles are not always expanded. Though the full-grown cod does not affect the tidal waters of the coast, yet the "rock" cod, by no means the youngest of its species, ventures close inshore; and the largest cod abound amongst the tidal waters of the Bell Rock.

The cnidæ of an anemone seem very efficient weapons against a soft-skinned Cephalopod, but they are not necessarily so against a tough-skinned fish.

Prof. McIntosh, in the work referred to in a previous letter, records *Tealia* and *Peachia* from the stomach of the cod, and *Edwardsia* (in swarms) from that of the flounder. He also informs me that he has found *Stomphia* in the stomach of the cod. I may add that the practice of baiting here with anemones is much more recent than the work referred to.

Of all British Cœlenterates, *Cyanæa* is, perhaps, the most deadly; yet many trustworthy observers have found young cod sheltering themselves beneath its umbrella—a fact which seems to indicate that they hold its stinging powers in some contempt; and Dr. Collingwood, in "A Naturalist's Rambles in the China Seas" (p. 150), has recorded the discovery of an immense fish-sheltering anemone.

ERNEST W. L. HOLT.

St. Andrews Marine Laboratory.

The Relative Prevalence of North-east and South-west Winds.

IN a note at p. 470 (NATURE, March 20), attention is drawn to the statement by Mr. Prince contained in his meteorological summary of observations taken at Crowborough, Sussex, in 1889, concerning the greater prevalence of north-east as compared with south-west winds which he finds to exist in recent years. The writer of the note mentions that this is not borne out by the Greenwich observations, but some definite statistics as regards Greenwich, and distinct comparison with the Crowborough numbers, may perhaps not be unacceptable to your meteorological readers.

Mr. Prince remarks that in previous years he finds only two years in which north-east winds have been in excess of south-west. In the first, 1864, the days of north-east wind were 104, of south-west wind 89; in the second instance, 1870, the days of north-east wind were 107, of south-west wind 88. The corresponding Greenwich numbers were, in 1864, 43 and 108; and in 1870, 65 and 96.

On the average of the years 1859 to 1883 Mr. Prince gives north-east wind on 63 days, south-west wind on 99 days. The corresponding Greenwich values are 43 and 111 respectively. For the years 1885 to 1889 he gives the average frequency of different winds as follows, to which I have added the values for Greenwich. C. indicates Crowborough, and G. Greenwich.

	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Calm.	
C.	41	102	21	22	38	72	50	17	—	days.
G.	49	52	35	23	37	100	40	19	10	days.

He further gives the averages for 47 years, to which I have added those for Greenwich for 49 years.

	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Calm.	
C. (47 y.)	33	63	29	27	28	91	59	35	—	days.
G. (49 y.)	40	45	27	22	35	106	46	22	22	days.

The Greenwich values are determined from numbers derived from the records of the self-registering Osler anemometer of the Royal Observatory as given in the annual Greenwich volumes. The preponderance of south-west wind over north-east seems to have been, throughout, less at Crowborough than at Greenwich. But it is only in recent years that the difference has become so pronounced, the Crowborough numbers for each year 1885 to 1889 being largely in excess for north-east wind, whilst the Greenwich numbers are greatly in excess for south-west, as in former years. At Greenwich during the first 24 years of the 49 years series, the average number of days of north-east wind was 46, of south-west wind 107; during the last 25 years, of north-east wind 44, of south-west wind 106.

It would be very interesting if a similar comparison could be made with some other station in the south of England.

Greenwich, April 16. WILLIAM ELLIS.

Science at Eton.

IN the *Illustrated London News* for March 29 I find an account (with illustration) of an astronomical lecture at Eton. It appears that the scholars "were allowed" to listen the other day, in the new lecture-room, to a lecture by Major-General A. W. Drayson, R.A., on the second rotation of the earth and its effects.

General Drayson has written some books on this subject which possibly no one has answered, for the simple reason that they answer themselves; but it seems now, that he is permitted, under the auspices of their teachers, to urge his paradoxes on the students of our largest public school.

Is Eton without any science teacher? or is the so-called teacher incapable of preventing absurdities being put forward with authority? Are the lecture-rooms of Eton College open to "Parallax" and the circle-squarers? J. F. TENNANT.

MODIGLIANI'S EXPLORATION OF NIAS ISLAND.

ABOUT two years ago, on his return to Florence, I gave a brief account of Dr. Elio Modigliani's very successful and interesting exploration of Pulo Nias (NATURE, vol. xxxv. p. 342). We have now before us the general results of that exploration, embodied in a portly volume most elegantly got up, rich in maps and illustrations, and, what is better, full of interesting facts, carefully collated notices, and well pondered and carefully drawn deductions; in short, one of the best books of its kind.[1]

Judging from what he has done, Dr. Modigliani is evidently made of the stuff which produces the best explorers. Resolute and persevering, moved by what we in Italy call *il fuoco sacro*, ever ready to put up with privations of all kinds, although accustomed to a very different sort of life, a quick and keen observer, he has indeed done wonders; and considering that he has not had the advantage of any special training in natural science, he has shown himself to be a good geographer and ethnologist, and a clever naturalist.

Dr. Modigliani's choice of the island of Nias as the field of his explorations was a singularly happy one, in which he was guided by no less a man than Odoardo Beccari. Few indeed of the hundreds of islands of that wonderland, the Malayan Archipelago, present such an accumulation of interesting problems as Nias. Lying off the ocean seaboard of Sumatra, and partaking naturally of the characteristic features of its big neighbour, it has a flora and fauna, with a remarkable number of special characteristics, whilst its human inhabitants show strange affinities with people of other races and of distant lands.

I shall now endeavour to give a concise account of Dr. Modigliani's exploration of Nias, and of the results he obtained, as given in his book. Dr. Modigliani left Italy at the end of 1885; he paid a rapid visit to India, crossing overland from Bombay to Calcutta, *viâ* Delhi and Agra, and visiting Darjiling; he touched at Rangoon, and after a short stay at Singapore and a lengthened one in Java, where at Batavia and Buitenzorg he prepared his local equipment, and engaged Javanese hunters and collectors, he reached Siboga, Sumatra, early in spring, 1886. Thence he started for Gunong Sitoli, the only civilized port of Nias, on one of the Dutch Government *Kruis* boats on April 14. Dr. Modigliani spent five months on the island, which he left in the middle of September. On his way back to Italy he completed the tour of Sumatra, touching at Kota Rajah and Olelek (Acheen), visited Singapore again, touched at Colombo, and crossed India a second time from Madras to Calicut, visiting the Todas and some of the hill tribes of Southern India, which had a special interest for him in his researches on the origin and affinities of the people of Nias. Dr. Modigliani brought back with him from Nias extensive and important collections—ethnological, zoological, and botanical—and whilst these were being studied by specialists, he actively set to work arranging and sorting his notes and the material for his book. Undertaking to deal with all the ethnological part himself, he visited the more important ethnographical museums of Europe, and even the minor ones where he knew that specimens from Nias were to be seen. To complete his historical and geographical researches regarding Nias, Dr. Modigliani paid a lengthy visit to Holland, working in the Libraries and Government Archives at the Hague and Leyden. I, who have had many opportunities of observing and admiring his untiring energy and activity, could hardly feel surprised, on reading his book, to find it so full of information and so excellently well done.

Dr. Modigliani has divided his work on Nias into two parts. The first contains three chapters, and is entirely introductory and historical; the second, in twenty-three chapters, with appendices and bibliography, contains the narrative of his sojourn in Nias, and his own personal observations and studies on men and things in that island. I have little to say on the first part of Dr. Modigliani's book except that it embodies the results of much erudition and careful and patient collation. From the earliest semi-fabulous notices of Al-Neyan, El-binan, Neya, Niha, Nia, in ancient Arabic and Persian manuscripts, we are brought to European intercourse with Tano Niha, as the natives call their island, and thence on through the modern vicissitudes of Dutch domination, which to this day is little more than nominal, except at Gunong Sitoli and in the northern portion of the island, where, however, German missionaries appear to have done more to spread the influence of civilization than the colonial authorities.

Part II. occupies by far the greater portion of Modigliani's bulky volume. After telling us how he travelled to Nias from Siboga—an adventurous crossing with a Malayan crew, a bad boat, and dirty weather—Dr. Modigliani devotes a chapter to the geography, meteorology, and geology of Nias. The island is hilly, but can hardly be called mountainous. A notable feature is the frequency of earthquakes, easily explained by the proximity of the volcanic chain of Sumatra. Rivers and watercourses are numerous, but few are of notable size. Geologically, Nias is evidently of recent formation; a collection of rock samples brought together by Dr. Modigliani might have shed much light on this interesting subject, but it was unfortunately lost. Madreporic limestone and clams (*Tridacna*) were noted on the hill-tops; true lignite has, however, been found in various parts. The Dutch colonial authorities deserve much praise for their

[1] Elio Modigliani, "Un Viaggio a Nias." Illustrato da 195 incisioni, 26 tavole tirate a parte e 4 carte geografiche. Pp. xv.–726. (Milano: Fratelli Treves, 1890.)

widely-spread and efficiently organized service of meteorological observations; even in the less important stations these are regularly recorded, and this has been the case for a long series of years at Gunong Sitoli. This is at present the residence of the Dutch civil and military authorities in Nias; the principal magistrate is a *Controleur*, who, with the officer in command of the native garrison, the medical officer, and the missionaries and their wives, form the sum-total of the European residents at Nias. Gunong Sitoli is mostly peopled with Malays, Klings, and Chinamen, the trade of the island being chiefly in the hands of the latter. Here, overcoming not a few serious difficulties, Modigliani made his preparations for visiting the southern parts of Nias, freer from external contact, and therefore more interesting; and for this purpose, a Malay boat—*pencialàng*—was chartered. Whilst these preparations were being completed, Dr. Modigliani visited a large cave near Hili Sabegno, and, besides other interesting animals, collected specimens of a bat (*Emballonura semicaudata*) previously known only from Polynesia. Meanwhile, his hunters were not inactive, and, amongst other interesting specimens, four new species of birds, a singular new earthworm, and several new insects were collected in the neighbourhood of Sitoli; the birds have been recently described by Salvadori as *Gracula robusta*, *Calornis altirostris*, *Miglyptes infuscatus*, and *Syrnium niasense*.

Tobacco is the principal article for barter with the wilder inhabitants of Nias, therefore Modigliani provided himself with a large stock, mostly Sumatra grown, and called *mussi;* Javanese tobacco, called *giau*, has a greater value. He provided himself, besides, with cotton cloth of different colours, and brass wire, also much sought by the Nias people.

At last the *pencialàng* was ready, and Modigliani sailed in her to the south end of the island, and anchored in the Luàha Vára Bay. His first sight of the Nias Southerners was rather forbidding, and seemed to confirm decidedly the many stories he had heard of their indomitable hostility and ferocity. A large number of warriors, armed with lances and rattling their big shields with a peculiar movement of the hand on the forearm, crowded on the beach at his landing, to the no small alarm of his followers. With much pluck and presence of mind, Modigliani overcame the momentary anxious suspense, and in a few minutes he was on his way to the village of Bàwo Lowaláni, surrounded and followed by the excited warriors. Here he soon made friends with Faòsi Aro, the chief, the tallest and most crafty of Southern Niassers, who appeared with two immense earrings resting on his right shoulder. A liberal distribution of tobacco soon made Modigliani popular all round. Bàwo Lowaláni is a good type of a South Nias village, placed on a height and defended by a stout stockade; the incessant wars between village and village render such precautions necessary. Our traveller passed several days here, having taken up his quarters in the house of Faòsi Aro, built as usual on stout piles; he was thus able to gather much information on the ways and manners of the Niassers. His Javanese collectors, although much afraid of the natives, who were constantly armed and on the alert, being then at war with two neighbouring villages, did some good work, and some new and rare insects and a new species of bird (*Cittocincla melanura*, Salvad.) were added to the collections.

At Bàwo Lowaláni, Dr. Modigliani received a special invitation to visit Hili Dgiòno, a village further inland to the west. A deputation awaited him outside Bàwo Lowaláni, not trusting themselves inside; a live fowl packed in a singularly neat manner (see Fig. 1) was presented to him, and the knife of the chief of Hili Dgiòno—the latter to be returned. Faòsi Aro did all in his power to dissuade Modigliani from going, telling him he would certainly be killed, as the Hili Dgiònans were a bad lot; but our traveller decided to keep his promise, and the evening of the next day saw him at Hili Dgiòno, where he met with a most cordial reception, especially from the old chief, Sidúho Ghèo. At this place Modigliani passed pleasant days, was able to take a fine series of photographs, and saw more of the natives and learnt more of their customs than anywhere else. The women alone, as in most parts of Nias, kept aloof, and would not be photographed. Here Modigliani saw palpable proofs of the well-known head-hunting propensities of the Niassers. The big council house, or *osalè*, was adorned

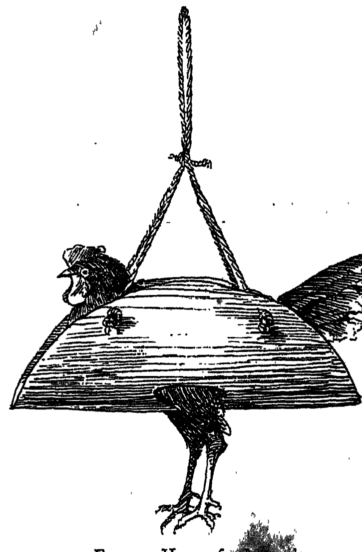


FIG. 1.—How a fowl travels.

with numerous skull trophies, hanging under the low roof. Heads are taken not only in war, but on many other occasions, for reasons amply given in Modigliani's book, most of which are similar to those which send the Dayaks of Borneo on their head-hunting expeditions; neither age nor sex are spared. No youngster in Nias is proclaimed a man and a warrior until he has cut off a head; he then assumes the prized *calabúbo* (Fig. 2), a beautiful collar made of thin circular sections cut out of the double nut of the *Lodoicea seychellarum* (which is often cast by the sea on the island), neatly strung on a brass wire with a circular

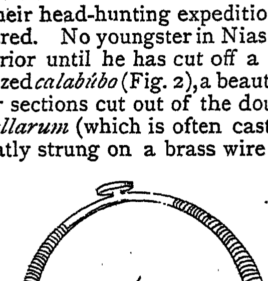


FIG. 2.—A *calabúbo*.

brass disk at the junction. The sections of the nut diminish gradually from about an inch in diameter to less than half at both ends, where the circular collar is closed with the disk; they are polished so as to present a uniform surface. None of the trophy skulls seen by Dr. Modigliani were in any way ornamented, but in his book he gives the drawing of a very singular one with artificial hair, beard, and ears, communicated by the late Baron von Rosenberg, who saw it in a house in Nias; I should fancy that it represents a European (Dutchman), for the beard hardly grows on a Niasser's chin in such luxuriance.

(Fig. 3). When old Sidúho Ghòo heard that Modigliani desired skulls (for his anthropological collection), he of course concluded that he wanted to get fresh ones as trophies, and at once offered to organize an expedition

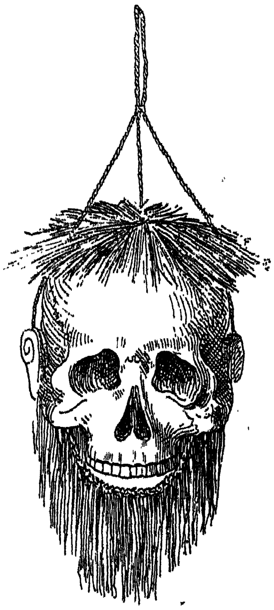


FIG. 3.—Ornamented trophy skull.

with chosen warriors; he would not give away any of those hung under the *osalè*.

At Hili Dgiòno, Modigliani was able to add largely to his ethnological collections, especially weapons. The defensive armour of the Niassers is peculiar. Formerly they made singular helmets of rotang and arenga-fibre, with beard and mustachios; now the chiefs are provided with curious iron helmets, pot-shaped, ornamented with a large plume or palm-leaf cut in a thin iron lamina, usually gilt; they wear, with this, curious iron spur-like mustachios passing under the nose and secured to the ear. The head-dress of the warrior of "old Japan" was a very similar contrivance; to complete the parallel I will add that the ceremonial war-jacket, often a regular cuirass in buffalo-leather, pangolin-skin, and scales or twisted rope tissue of tough *Gnetum* fibres, usually projects widely over each shoulder. It is thus with the war-jacket of some of the Dayak tribes, and was thus with the ceremonial *kamiscimo* of the Nippon *samurai*. The Nias shield, *balúse*, is peculiar, and made in a single board of tough light wood; in the northern parts of the island a heavier one, called *dagne*, more akin to Bornean and Celeban shields, is used. The characteristic weapons of the Niassers are the spear (*tòho*) and sword (*ballátu*), the latter not unlike the Dayak *parang*. The iron spear-heads are generally small and narrow, simple, or more or less provided with barbs; the wood is from the Nibóng palm, and usually ornamented with rings of rotang, brass, or wire, and often with tufts of hair from an enemy's head. The sword is still more characteristic. Its sheath is made with two halves neatly fitted and bound together with plaited rotang; the big sword (*ballátu sebúa*, "number one") is, especially in the south of Nias, the favourite weapon; much trouble is taken in ornamenting it, and the carved handle is often a remarkable specimen of wood-carving. Modigliani was fortunate enough to secure a series of these swords with carved handles, giving a most interesting instance of modification of a figure, in this case a boar's head, in the opposite directions of a simplified and a complicated conventionalism (Fig. 4). Moreover, the *ballátu sebúa* of the Southern Niassers is

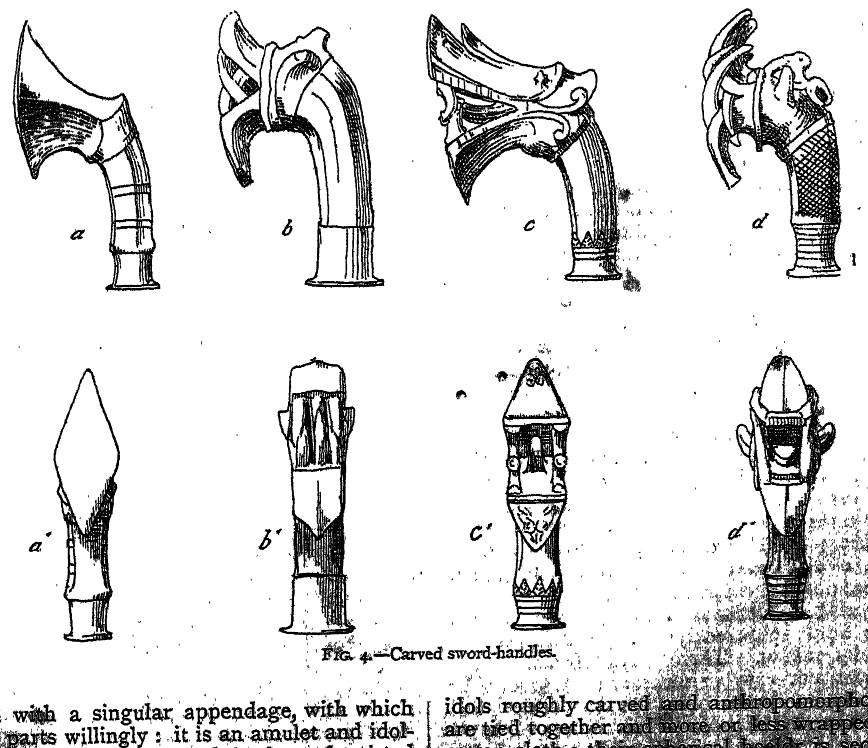


FIG. 4.—Carved sword-handles.

always provided with a singular appendage, with which the owner never parts willingly: it is an amulet and idol-bearer in the shape of a spherical basket of twisted rotang, with various and heterogeneous contents, such as teeth, pieces of stone and bone, &c., always several small idols roughly carved and anthropomorphous. All these are tied together and more or less wrapped up in a bit of cotton-cloth; their spherical hoeldr is securely fastened to the scabbard. Dr. Modigliani has given some highly interesting details on this subject; the *ere*, or "medicine

man," of the Niassers possesses a special talismanic sword with special idols and charms attached to the scabbard. Quite a number of old flint-lock muskets have found their way to Nias, but are fortunately often rendered useless from want of ammunition. The Niassers are able smiths, but they receive the iron and brass they use from Chinese and Malay traders.

On his way back, at Bàwo Lowaláni, Modigliani was able to buy from Faòsi Aro eleven human skulls. He next sailed to Luàha Gúndre Bay, wishing to visit the important village of Hili Sendrecheási, and possibly to proceed thence inland. He was well received by the chief and notabilities, who, however, promised much and did little. Another new bird was obtained here—*Terpsiphone insularis*, Salvad. Meanwhile, the head-man of another neighbouring village, Hili Simaetáno, sent messengers to invite him to go there, promising that he might stay and collect as much he liked. The death of a warrior at Sendrecheási gave Modigliani an opportunity of witnessing the funereal ceremonies of the Niassers, on which subject he gives much important information. He was not able, however, to confirm Piepers's assertion (*Bat. Genoot. v. Kuns. en Wettensch.*, 1887) regarding the horrid and singular custom of putting the body upright in a hollow tree, tapping this below, inserting a bamboo tube, and forcing a slave to drink the putrid liquid which flowed. The unfortunate man's head was afterwards cut off, and hung to the tree as an offering to him whose body was inclosed therein. I may mention that a similar custom is attributed to certain Dayak tribes of Borneo by Perelaer, and that it recalls the ancient Javanese *sétra*. It appears, however, that human lives are still sacrificed at the death of a chief. The author has also brought together highly interesting information as to "animism," belief in a future state, and ancestor-worship amongst the Niassers.

Although lamed, and suffering from a bad foot, he left Luàha Gúndre for Hili Simaetáno on June 1. His reception there was, however, the reverse of what he expected: the people were not only diffident, but evidently hostile, notwithstanding the invitation sent by their chief. Amongst the interesting things seen were two elaborately carved stone thrones of honour, used by the chief on solemn occasions; opposite one, on a pole, was a human skull. These two differed widely, the smaller one in the centre of the village being a sort of arm-chair, the back of which represented the bust of a warrior with a crocodile climbing up behind him. These singular stone seats of honour recall those found in far-off Ecuador. After a couple of days' stay, the hostility of the villagers was so evident that Modigliani decided to leave; and if he was not actually attacked, he owed it not only to his firmness and forbearance, but probably to the fear caused by his repeating-rifle, and to the villagers being short of ammunition. Anyway, he was able to get safely back to his *pencialàng*. Wishing, however, to penetrate into the interior of the island, he sailed to the Nácco Islands off the opposite coast of Nias, where he hoped to get guides and information. Mára Ali, chief of Nácco, received him well, and after much palavering and a liberal distribution of presents, he was able to obtain a guide in the person of Sanabahíli, brother of the local *erè*, and bearers. His intention was to land on the opposite coast of Nias, and penetrate inland to one of the higher mountains, known as Matgiúa, where he hoped to make interesting collections. Having landed, after a narrow escape from shipwreck, at Cape Serombú, he proceeded boldly inland. There were no roads, and his progress was not easy or pleasant; moreover, his guide was hardly up to the office he had undertaken, and conducted him by mistake to the village of Idáno Dôwu. Thence he marched to Mount Buruássi, before reaching which most of his bearers had deserted; small villages were passed, and the sites of bigger ones which had been destroyed during the incessant wars. Halambáva, a strongly fortified village, was next visited; here he found a singular and grotesque idol, *Adú Fangúru*, carved in a cocoa-palm trunk on the occasion of an epidemic which had decimated the village. Crossing next the nearly unknown district of Iraòno-Una, peopled by ferocious head-hunters, he continued on to Hili Lowaláni; here he came to the conclusion that Mount Matgiúa had been purposely missed, or more probably was sadly out of place even in the best maps of Nias, and decided to return to the north. Travelling on by Hili Hôro, he came again to Hili Simaetáno, where he was well received this time, and able to buy some skulls. At the Luàha Gúndre he was rejoined by his *pencialàng*—not until after long waiting, anxious moments, and the risk of starvation, having finished his provisions—and sailed back to Gunong Sitoli. This voyage across the south-west end of Nias was an adventurous one, but hardly equal in results to the trouble it had cost.

After his return to Sitoli, Modigliani decided to spend what time he had left to remain in Nias in some favourable locality in the north, where, amongst quieter people, he might better complete his observations and collections. He selected the village Ombaláta, or rather the neighbouring hill called Hili Zabòbo; here he passed pleasant days and was able to do much. Amongst the interesting species collected I may mention: *Pteropus nicobaricus*, *Chiropodomys gliroides*, a rare and singular rodent lately collected by Fea in Burma; *Macropygia modiglianii*, Salvad., and *Carpophaga consobrina* Salvad., new pigeons; a rare and beautiful lizard, *Gonyocephalus grandis*, and the hitherto unknown *Aphaniotis acutirostris*, Modigl.; and several new species of Coleoptera and ants. It is worth notice that in more than 4000 specimens of Lepidoptera collected by Dr. Modigliani no novelties were found, but he secured some fine specimens of the rare and peculiar *Hebomoia vossi*, Maitl. Dr. Modigliani purposes publishing complete lists of the animals of Nias; meanwhile he has given in an appendix lists of the species he collected, having determined some himself, whilst others have been studied by several specialists. He obtained 15 species of mammals, 62 of birds, 39 of reptiles, 8 of batrachians, 71 of fishes, and lists of over 400 species of insects have already been published. The bulk of these zoological collections are in the Civic Museum of Genoa. Modigliani was not able to do as much in botany as he wished, but he was able to gratify Beccari with some choice specimens of his favourite *Myrmecodia* and *Hydnophytum*, those strange epiphytal ant-harbouring plants first noticed by Jack at Nias.

The last chapters of Dr. Modigliani's book are entirely devoted to the ethnology of Nias, and great and important is the amount of information which he has gathered on this interesting subject. I will merely mention one or two of the principal items. Discussing the origin and affinities of the Niassers, he finds them not only different from the ordinary Malay, but partaking of the characters of the Mongoloids (in a restricted sense) and even of the Arianoid races; and at the same time he notes physical differences between the natives of Northern and Southern Nias. I confess that I cannot quite follow our author in this: the Niassers most evidently belong to the great Malayan family, and perhaps resemble some of the Dayak tribes more than any others. The ancient and constant contact with Chinese may have slightly *mongolized* them, always in the more restricted sense of that term (some of Modigliani's photographs recalled to my mind portraits of Kwei-yings of North Formosa shown to me years ago by my lamented friend Robert Swinhoe). But I fail to see traces of Arianoid features in any of the Niassers photographed by Dr. Modigliani. At the same time, I can quite understand how he found points of resemblance between them and natives of Southern India, who evidently have Malayan blood in their veins.

Modigliani mentions seeing in South-West Nias natives with Arianoid Semitic features and curly or wavy hair, but he himself suspects in such cases the influence of Arabo-Malay immigrants from Acheen.

Amongst the many peculiarities of the inhabitants of Nias, is the custom of the women going about with a long slender stick called *sio;* it is of Nibong palm wood, has a heavy leaden knob, and is more or less ornamented with rings of lead and brass; it is found only in the possession of women. Great is the variety of ornaments worn by the Niassers, male and female. They often denote distinctions of rank and sex. Ear-rings and bracelets are especially varied; singularly beautiful are the bracelets (Fig. 5) carved and polished by a long and tedious process out of a solid block taken from the stony shell of the giant clam (*Tridacna*), more elegant in shape than the equally notable armlets of the same material made by the inhabitants of the Solomon Islands. The Niassers also carve big solid ear-drops out of the *Tridacna* shell. Their principal articles of dress are still made with the beaten and manipulated inner bark of a *Ficus* or *Arctocarpus*, a kind of *tappa* or *masi*, called by them *sambò salówo.*

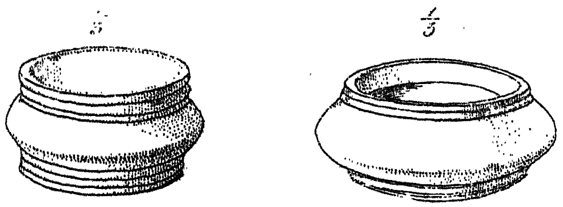


FIG. 5.—Bracelets cut in Tridacna shell.

Dr. Modigliani did not find or hear of stone or shell implements in Nias; possibly the first men who peopled that island were already provided with iron tools. Yet one of the commonest amongst these, the axe, *fáto*, has a singularly archaic form: the iron blade, very similar to the earlier forms of copper and bronze implements of the kind, is let into a slot in a short club-shaped wooden handle (Fig. 6). A yet more singular fact is that the *fáto* of the Niassers is a typical axe, and quite distinct from the adze used right across Malesia from the Nicobar Islands to New Guinea, being, instead, remarkably like the iron axe of some of the wilder tribes of Central Africa.

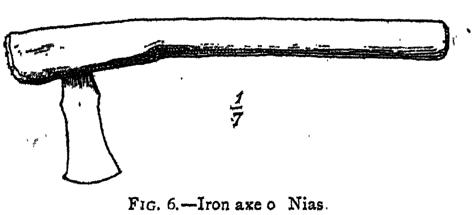


FIG. 6.—Iron axe o Nias.

I may mention here that the rich and important anthropological and ethnological collections made at Nias by Dr. Modigliani have mostly been presented by him to the National Anthropological and Ethnological Museum in Florence.

Dr. Modigliani has collected quite a host of interesting facts relating to the myths and superstitions of the natives of Nias, which all appear to centre in a well-developed form of "ancestor worship." The ancestors more or less remote are spirits good and evil, and as mediators between them and the living are numerous *adú*, or idols (Fig. 7). Amongst the numerous spirits more or less divine venerated by the Niassers is *Sangaròfa*, the sea-god, and Modigliani justly calls attention to the strange similarity in name and attributes to *Tanga-roa*, the sea-god of the Maories and other Polynesians. The principal good spirit is *Lowaláni;* the bad ones are classified in two grades as *Bèchu* and *Bèla*, these being, however, generic terms. The *adú* or idols, whose Nias name, by the way, is singularly like the equivalent Polynesian term *atua*, are very numerous; those which represent dead relations or immediate ancestors are called generically *Adú zatúa.* They appear to have great affinities with similar carved wooden anthropomorphic figures common throughout Papuasia and Melanesia, and known as *karwars* in Western New Guinea.

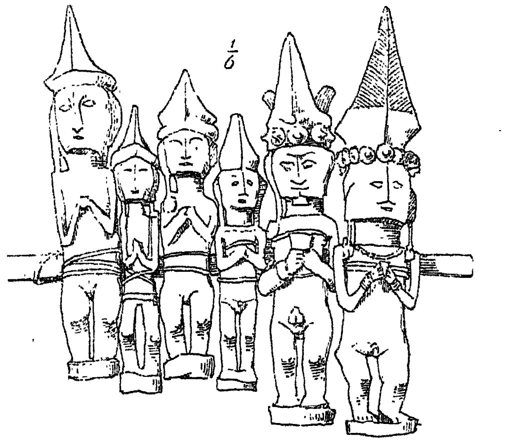


FIG. 7.—Images of ancestors.

In one of the last chapters of his book, Modigliani gives an account of the spoken language of the Niassers, which has many peculiarities; adding an alphabetically arranged collection of words with their Italian equivalents.

But my task, which has been to endeavour to give an idea of the work done by Dr. Modigliani, must now come to an end. His book, containing a very complete monographic study of one of the most interesting islands of the Indian Archipelago and its inhabitants, is, and will long remain, one of the standard works on that beautiful region Malesia.

HENRY H. GIGLIOLI.

NOTES.

THE next general meeting of the Institution of Mechanical Engineers will be held on Thursday evening, May 1, and Friday evening, May 2, at 25 Great George Street, Westminster. The chair will be taken at half-past seven on each evening by the President, Mr. Joseph Tomlinson. On Thursday evening the President will deliver his inaugural address, after which the following paper will be read and discussed, and the discussion will be continued on Friday evening:—Research Committee on Marine-Engine Trials: Report upon Trials of three Steamers, *Fusi Yama, Colchester, Tartar*, by Prof. Alexander B. W. Kennedy, F.R.S., Chairman. The anniversary dinner will take place on Wednesday evening, April 30.

THE first annual meeting of the Museums' Association will be held in Liverpool on June 17, 18, and 19. The business of the meeting will consist of (1) the reading of papers on the management, arrangement, and working of Museums; (2) the discussion of the objects set forth by the meeting of June 20, 1889, with special reference to the following points: the means of interchange of duplicates and surplus specimens; schemes for a general supply of labels, illustrations, &c.; the indexing of the general contents of Museums; concerted action for obtaining Government publications, and also specimens on loan or otherwise; and the issue of a journal devoted to the discussion of practical topics. At this meeting the scheme for the constitution of the Association will be submitted. All engaged or interested

in Museum work are cordially invited to join the Association. The conditions of membership are as follows:—Each Museum contributing not less than one guinea a year becomes a member of the Association, and can send three representatives to the meetings. Individuals interested in scientific work are admitted as Associates on payment of 10*s.* 6*d.* annually. The following are the officers of the Association:—President: Rev. H. H. Higgins; General Secretaries: H. M. Platnauer, Museum, York, T. J. Moore, Museum, Liverpool; Local Secretaries: R. Paden, Museum, Liverpool, H. A. Tobias, Museum, Liverpool.

THE next *conversazione* of the Royal Microscopical Society will be held on Wednesday, the 30th inst., at eight o'clock.

HERR O. JESSE sends us from Steglitz, near Berlin, some very beautiful photographs of luminous night clouds. The photographs of each pair were taken simultaneously at Nanen and Steglitz. Steglitz lies 8 kilometres south-west, Nanen 38 kilometres west-north-west, of the Berlin Observatory. Herr Jesse would add greatly to the value of his work if, the next time he has an opportunity of undertaking it, he would photograph the spectrum.

La Nature (April 12, p. 303) notes the following curious and interesting phenomena:—Two railways, one the Sceaux line and the other the Ceinture, pass within a comparatively short distance of the Montsouris Observatory, Paris, the former line being about 80 metres distant, and the latter but some 60 metres. During the passage of trains on the Ceinture line, which is nearest to the Observatory, the bifilar magnet is found to be disturbed, and its oscillations are registered photographically; indeed the movements are so regular that the curve clearly indicates the exact time of each train passing the Observatory. This phenomenon is due to the fact that as the line crosses the direction of the magnetic meridian the wheel-tires of the carriages become magnetized by induction, and so produce, in consequence of the laws of magnetism, a deviation of the bifilar magnet. The trains on the Sceaux line give rise to a phenomenon not less curious. Whenever the engine-driver blows off steam, the electrometer is partly discharged, the electrical potential of the air falling to about one-half of its original value. These disturbances are brought forward by the Director of the Paris Observatory in order to oppose the scheme which is now proposed of extending the railway from Sceaux to la Place de Médicis.

ON Tuesday evening, M. Jacques Bertillon (head of the Municipal Bureau of Statistics in Paris) delivered a lecture before the Anthropological Institute of Great Britain and Ireland, on the method now practised in France of identifying criminals by comparing their measures with those of convicted persons in the prison registers. Mr. Bertillon, who spoke in French, said that the system which he had come there to explain had for its object the recognition of a person 10, 15, 20, or even 100 years after he had been measured, for by that method it was possible to recognize a person after death, if access could be had to his skeleton. Photography was now used only as an aid to identification established by other means. The basis of the anthropometic system was to obtain measurements of those bony parts of the body which underwent little or no change after maturity, and could be measured with extreme accuracy to within so small a figure as to be practically exact. These parts were the head, the foot, the middle finger, and the extended forearm from the elbow. To clearly illustrate the system, let them suppose 90,000 photographs of men to have been collected. These would be divided into three groups of 30,000, according to the height of the men. There would be short men, men of medium height, and tall men. That these three classes might be approximately equal, it was evident that the limits of the class of men of medium height must be restricted more than those of the other two classes. Each of these primary divisions should again be divided on the same principle, without taking any further notice of the height, into three classes, according to the length of the head of each individual. The three classes of short, medium, and long heads would each again be subdivided into three, according to the width of the heads, and would contain narrow, medium, and wide heads. Experience had proved that with most people the breadth of the head varied independently of the length—that was, given that an individual had a certain length of head, it by no means followed that the breadth of his head could be determined *a priori*. The length of the middle finger gave a fourth and still more precise indication by which to divide again each one of the packets of photographs; and these might be divided again according to the length of the foot, the length of the arms outstretched at right angles to the body, and also according to the colour of the eyes. Thus by these anthropometrical coefficients they would be able to divide their collection of 90,000 photographs into very small groups of about 15 each, which they could easily and rapidly examine. M. Bertillon then proceeded to give a practical demonstration of the way in which the measurements were taken. He laid stress on the importance of the hand and the ear as marks of cogniti on. The hand, because it was the organ in most constant use in almost every calling and in many trades and professions, became modified according to the particular character of the work which it had to do. The ear was the precise opposite to this. It changed very slightly, if at all, except perhaps in the case of prize-fighters, who developed a peculiarity of the ear which it was easy to recognize. The ear, therefore, was an important organ to measure, inasmuch as the results were not likely to be nullified by a change in its conformation.

THE following telegram was sent through Reuter's agency from New York on April 21:—"Despatches from Mexico state that observations show that the height of the active volcano of Popocatepetl has decreased by 3000 feet since the last measurement was taken."

IN the new quarterly statement of the Palestine Exploration Fund, the Committee announce that they have obtained a firman granting permission to excavate at Khürbet 'Ajlân, the Eglon of Joshua. It is understood that all objects, except duplicates, found in the course of the excavations shall be forwarded to the Museum at Constantinople, but that the Committee's agents shall have the right of making squeezes, sketches, models, photographs, and copies of all such objects. The Committee have been so fortunate as to secure the services of Mr. Flinders Petrie, who is now in Syria making arrangements to start the excavations.

THE death of Dr. Gottlob Friederich H. Küchenmeister is announced. He was a great authority on Entozoa.

IN the official outline of the principal arrangements at the Crystal Palace for the summer of 1890, reference is made to the International Exhibition of Mining and Metallurgy which is to be held there from July 2 to September 30. The subjects embraced within the scope of the Exhibition comprise machinery in motion and at rest; gold, silver, diamond, iron stone, and iron ore mining; manufacture of iron and steel; lead mining and manufacture; tin mining and smelting; copper and coal mining; the petroleum and salt industries; mining for precious stones, &c. There is every reason to expect, through the co-operation of colonial and foreign Governments, many valuable exhibits from abroad.

THE *Engineer* and *Engineering* of last week publish long illustrated accounts of the recent disaster to the City of Paris

This accident is without a parallel in the history of steam navigation; the circumstances were so remarkable that many conflicting explanations of the cause have been suggested. The ship is propelled by twin screws, and the engines are placed side by side in separate compartments. When she was off the coast of Ireland, at half-past five on the evening of the 25th ult., the low-pressure cylinder, with the whole of its gear, of the starboard engine, went to pieces, and fell to the bottom of the engine-room in a confused mass, the *débris* of the top cylinder cover being apparently at the bottom of the wreck. The smashing of the condenser allowed an enormous rush of water to flood the starboard engine-room, and the longitudinal bulkhead between the engines, being also damaged, allowed the port engine-room to become flooded, and of course stopped that engine from working. Our contemporaries say that, in the opinion of experts in Liverpool, the accident did not originate in the engine, but in the tail shaft; as follows: the brass liner on the tail shaft burst; then the lignum-vitæ strips were torn out, bringing metal to metal. This, naturally, would allow the steel shaft to grind itself and the bracket away, and the shaft dropped. Then the continual bending of the shaft resulted in its fracture. The engines, being relieved of the resistance of the screw, raced, with the result shown in the engravings. The *Engineer* at present neither accepts nor rejects this theory of the cause of the disaster.

THE Manchester Field Naturalists' Society opened the summer excursion session on the 19th inst., by a visit to the well-known herbaceous garden of Mr. Wm. Brockbank, Withington, near Manchester. The grounds, of about six acres in extent, are laid out in woodland, shrubbery, rockeries, and fernery, with a patch of wilderness, and are entirely devoted to the growth of the native flowers, and the horticulturists' modifications, so far as they will thrive. The special feature, at the time of the visit, was the display of daffodils, over a hundred varieties being included in the gardens, several of them locally raised. Mr. Brockbank explained that the double variety of the daffodil is not obtained by the absorption of the essential organs, as generally supposed; the pistils and stamens remain, and specimens were shown, in vigorous health, obtained from their seeds.

IT has been suggested that the epidemic of influenza was in the last resort due to floods in China. The fertile land in the valley of the Yellow River, it has been said, was covered with a deposit of alluvial mud, and in this mud countless numbers of organic spores were developed from the refuse of a dense population. These germs were carried by merchandise to Russia, whence they spread to Europe generally. Dealing with this theory, the *Shanghai Mercury* points out (1) that there has been no epidemic of influenza in China. (2) There is no valley whatever of the Yellow River, the peculiarity of that stream being that it flows on the surface of the ground, which actually slopes down on both sides from the river bed, so that in case of a breach of either embankment the river is free to flow to the sea almost anywhere between Tientsin in the north, and Shanghai in the south. (3) The plain of the Yellow River is by no means fertile, and is rapidly deteriorating. (4) So far from the deposit left after a breach being alluvial mud, it is unmitigated sand, and for years refuses to grow any crops whatever; and it is only after an exposure of some fifteen or twenty years that the phosphates which enter sparingly into its composition begin to break up, and the land is restored to cultivation. (5) There are no exports of any sort from the plain of the lower Yellow River. Almost the only product exported to Europe from districts anywhere near the river is straw braid, which is shipped not to Russia but to England and the United States; and this not from the plain, but from the highlands of Shantung, far removed from any communication with the river.

THE Ballarat School of Mines, in the University of Melbourne, presented its annual report at a meeting of governors and subscribers on Monday, January 20. The general efficiency and usefulness of the school have been greatly promoted by extensive additions to the buildings and plant, and the numerous improvements effected in connection with the mining and metallurgical departments. That the institution now affords a superior training in scientific and mining subjects is shown by the attendance of a more advanced class of students, and by the better results obtained at the examinations. It attracts to its classes students from all the neighbouring colonies, including Queensland, New South Wales, South Australia, and Tasmania, as well as from distant places within Victoria. The total number of enrolments in the various classes held during the year was 982, and of individual pupils who attended the elementary science lectures delivered in the State schools, 723. The mean average number of students in attendance at the school classes for the whole year was 526, whilst during the same period 286 lectures on elementary chemistry were delivered in nine of the State schools in the city and town, with an average attendance of 53 at each lecture.

MR. A. J. CAMPBELL has returned to Melbourne after a three months' trip in Western Australia. The *Victorian Naturalist* says he has been very successful in his observations and collections. He obtained about 80 different species of eggs, 13 of which it will be necessary to describe as new. The number of eggs obtained altogether was about 400. About 100 skins of birds were collected, though Mr. Campbell made no special effort to secure them. With regard to geographical range of birds he was particularly successful in his observations. No less than 17 species will be recorded as new for Western Australia. Possibly one or two may be deemed new varieties, while others will be restored, having been omitted from a lately issued tabular list. Baron von Mueller has examined the plants, and finds that two ferns, *Asplenium marinum* and *A. trichomanes* (both British species, by the way) are recorded for the first time from the western colony. Of 30 lichens collected, the Rev. F. R. M. Wilson has identified 20 as new for the same colony. Specimens of characteristic lizards and frogs (*e.g.*, *Heleioporus albo-punctatus*) were secured. About three dozen photographs turned out fairly well, those of the remarkable flights of sea-birds being of great interest. Mr. Campbell considers that he brought nearly 1000 natural history specimens back to Melbourne.

IN the latest of his series of instances—printed in the *American Naturalist*—of the effect of musical sounds upon animals, Mr. R. E. C. Stearns mentions the case of a canary "who is particularly fond of music." This interesting bird belongs to the Rev. Mr. James, who writes as follows:—"Immediately I begin to play upon the flute she chirps about as if enjoying the music. If I open the cage-door and leave her, she will come as near to me as possible, but not attempt to fly to the music; but if I put her upon my desk, and lay the flute down, she will perch upon the end, and allow me to raise the instrument and play. I often take her into the church and play there upon the organ, and she will perch upon my fingers, notwithstanding the inconvenience of the motion of the hands, and chirp in evident delight at the sweet sounds."

LAST week Prof. Stricker submitted to the International Medical Congress at Vienna a new electrical lantern which will, it is expected, be of great service to lecturers and medical students. According to the Vienna correspondent of the *Times*, Prof. Stricker, by an ingenious combination of lenses, contrives to project the magnified images of objects on a white screen in their natural colours, so that, for instance, a small pimple on a patient can be shown in its real appearance to an audience of many hundred students.

At the seventh Congress of the American Ornithologists' Union, Dr. R. W. Shufeldt read a report on progress in avian anatomy for the years 1888–89. Towards the end of this report, which has now been reprinted separately, Dr. Shufeldt said he had greatly felt the need of a good hand-book to the muscles of birds. In looking about him, he soon found that there was no such manual in the English language; at least, there was not the kind of work that the thorough dissector required. To meet this want he undertook the preparation of a volume devoted to the subject. A thoroughly cosmopolitan form, or rather a form well representing a cosmopolitan group of birds, the raven, was selected. He carefully dissected out on many specimens every muscle of this type, and figured them in a careful series of drawings. These he supplemented by a series of drawings of the skeleton of the same form, and on the bones indicated the origin and insertion of all the muscles. Full descriptions were written out, and the groups of muscles classified; and finally some comparative work was added. Both the drawings of the muscular system, as well as the skeleton, were life-size, which made the parts very clear and convenient for use. "To my surprise," says Dr. Shufeldt, "when it was all completed, the manuscripts for a small volume were on my hands." The work is now in the press, and will be published shortly by Messrs. Macmillan and Co.

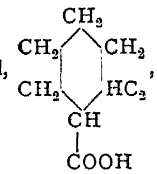
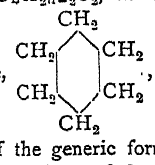
Two volumes of the *Internationales Archiv für Ethnographie* have now been completed. With the current number, just issued, the third volume begins. In a prefatory note, the editor, Dr. Schmeltz, refers with satisfaction to the help he has received from eminent contributors; and he is able to promise that the periodical shall be not less instructive and interesting in the future than it has been in the past. In the present number there are several valuable papers. One of them, by Dr. Franz Boas, deals with the use of masks and head-ornaments on the north-west coast of America. Herr Strebel, of Hamburg, contributes the first of a series of "studies" on a peculiar kind of stone implements found in Mexico and Central America. Hitherto it has been generally supposed that these implements were put on the necks of human victims destined for sacrifice. The author undertakes to show that this view is mistaken.

The Journal of the Anthropological Institute (vol. xix. No. 3) contains an elaborate and most interesting paper, by Prof. A. C. Haddon, on the ethnography of the western tribe of Torres Strait. The other contributors to this number are Dr. Beddoe, who writes on the natural colour of the skin in certain Oriental races; and the Rev. James Macdonald, who has a paper on the manners, customs, superstitions, and religions of South African tribes.

The *Photographic Quarterly*, of which three numbers have been published, meets a need which must often have been felt by those who specially devote themselves to photography. It includes among its contributors many eminent students, and deals freely with all important questions in which photographers are interested. The third number opens with an article on photography of the sky at night, by Captain W. de W. Abney. Among the other contents are papers on the limits and possibilities of art photography, by George Davison; photogravure and heliogravure, by P. G. Hamerton; the optical lantern as an aid in teaching, by C. H. Bothamley; and a phase of naturalistic focussing, by H. Dennis Taylor.

A complete index of the papers printed in the Proceedings of the London Mathematical Society has been issued. It will be of great service to all who have occasion to refer to the series, which now includes twenty volumes.

A catalogue of the books in the library of the Indian Museum has been issued by the trustees. It has been compiled by Mr. R. Leonard Chapman. The number of separate works in the library is about 3500, and every facility is given to students consulting them. In a prefatory note Mr. J. Wood-Mason, superintendent of the Indian Museum, says that most of the books are on zoology and kindred subjects, and he has no doubt that "the gradual spread of scientific education in India will largely extend the field of usefulness of the Museum library in the future."

A new acid, , the first member of a series possessing the generic formula $C_nH_{2n-2}O_2$, derived from the saturated hexa-hydride of benzene, , the so-called naphthene and its homologues of the generic formula C_nH_{2n}, has been isolated by Dr. Ossian Aschan, of the University of Helsingfors, from the natural oil of Baku (*Berichte*, 1890, No. 6, p. 867). The acid may be considered as a saturated hexahydride of benzoic acid; it is a very stable liquid substance of strongly acid properties, readily decomposing calcium chloride with evolution of hydrochloric acid and formation of a calcium salt. The raw mixture of acids obtained by treating the oil with alkali, and subsequent decomposition of the sodium salts by dilute sulphuric acid, was first distilled and the lower boiling portion specially examined. Upon partially saturating this fraction with caustic soda solution, and again decomposing with sulphuric acid, a colourless oil separated. In order to separate the various acids contained in this oil, they were converted into methyl esters by the action of methyl alcohol and strong sulphuric acid. These esters were then submitted to fractional distillation, when a large quantity of an ester boiling constantly at 165°·5–167°·5 C. was eventually isolated, possessing the composition $C_6H_{11}.COOCH_3$. This was, in fact, the methyl ester of the new acid, the first member of the series, of which other higher members have previously been obtained by Markovnikoff and others. The methyl ester is a highly refractive colourless oil of pleasant fruit-like odour. By saponification with alcoholic potash, crystals of the potassium salt of the acid itself were obtained. On acidification of the aqueous solution of these crystals, the free acid separates as an oil, which after rectification boils constantly at 215°–217°. It is a colourless thick liquid of unpleasant and very persistent odour, and does not solidify at − 10°. Its strength as an acid has already been alluded to as evidenced by the turning out of hydrochloric acid from chlorides of the alkaline earths; moreover, the calcium and barium salts are not decomposed by carbonic acid. Strong sulphuric acid readily dissolves it, with decomposition upon heating. Its specific gravity at 18°·4 is 0·95025. This acid is isomeric with the methyl pentamethylenic acid synthesized by Messrs. W. H. Perkin, Jun., and Colman, the latter boiling a little higher, at 219°–219°·5, and possessing a higher specific gravity, 1·02054 at 15°. The potassium salt $C_6H_{11}COOK$ is a soft soap-like substance, which may sometimes be obtained in distinct crystals. It is readily soluble in water and alcohol and is strongly hygroscopic. The sodium salt much resembles its potassium analogue, and may be obtained crystallized in flat prisms from alcohol. It likewise deliquesces very rapidly in the air. The calcium salt dissolves readily in alcohol, but is more difficultly soluble in water. If an aqueous solution is allowed to evaporate over oil of vitriol, the salt, $(C_6H_{11}COO)_2Ca + {}_4H_2O$, is obtained in long needles. If a solution saturated at the ordinary temperature is heated to boiling, it becomes turbid and viscous drops begin to separate; these

again dissolve on cooling. This behaviour is very characteristic of the acid, the barium salt showing the phenomenon also in a striking manner. It is due to the different amounts of water of crystallization in the salts separating at different temperatures. The chloride of the acid radical, the amide, and the anilide of the acid have also been prepared, and found to resemble the corresponding derivatives of the fatty acids.

THE additions to the Zoological Society's Gardens during the past week include two Indranee Owls (*Syrnium indranee*) from Ceylon, presented by Mr. A. R. Lewis; two Lataste's Frogs (*Rana latasti*) from Italy, presented by Mr. G. A. Boulenger, F.Z.S.; a Common Moorhen (*Gallinula chloropus*), British, two Moorish Toads (*Bufo mauritanica*) from North Africa, presented by Mr. Cuthbert Johnson; an Indian White Crane (*Grus leucogeranos*), two Black-gorgeted Jay Thrushes (*Garrulax pectoralis*), an Indian Muntjac (*Cervulus muntjac* ♂) from India, deposited; a Pacific Fruit Pigeon (*Carpophaga pacifica*) from the Solomon Islands, four Madagascar Weaver Birds (*Foudia madagascariensis*, 2 ♂ 2 ♀) from Madagascar, six Common Cormorants (*Phalacrocorax carbo*), European, two Adelaide Parrakeets (*Platycercus adelaidæ*) from South Australia, purchased; a Puma (*Felis concolor*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at Greenwich at 10 p.m. on April 24 = 12h. 11m. 30s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G.C. 2838	—	—	12 13 13	+15 7
(2) G.C. 3035	—	—	12 25 15	+12 59
(3) G.C. 3092	—	—	12 29 50	− 3 11
(4) 3 Canum Venat. ...	6	Yellowish-red.	12 14 23	+49 35
(5) o Virginis	4	Yellow.	11 59 36	+ 9 29
(6) η Virginis	3	White.	12 14 18	− 0 3
(7) B.D. + 1°2694 ...	8	Red.	12 19 7	+ 1 27
(8) S Ursæ Majoris ...	Var.	Strong red-yellow.	12 39 7	+61 42

Remarks.

(1, 2, 3) Although the constellation Virgo is so exceptionally rich in nebulæ, comparatively few of them have been submitted to spectroscopic examination. Smyth remarks that "the situation of the extraordinary conglomerate of nebulæ and compressed spherical clusters which crowd the Virgin's left wing and shoulder is pretty well pointed out to the practised naked eye by ε, δ, γ, η, and β Virginis, forming a semicircle to the east, whilst, due north of the last-mentioned star, β Leonis marks the north-west boundary." As it is not possible to give anything like a complete list, three of the brighter ones which have not yet been spectroscopically observed have been selected. No. 1 is the remarkable spiral nebula 99 M Virginis, and is thus described in the General Catalogue:—"A very remarkable object; bright; large; round; gradually brighter in the middle; three-branched spiral." No. 2 is 87 M Virginis, and is described as "Very bright; very large; round; much brighter in the middle." No. 3 is described as "Very bright; considerably large; pretty much elongated in a direction about 63°; very suddenly much brighter in the middle to a nucleus." It is a remarkable fact that all the nebulæ in Virgo, which have so far been examined, exhibit so-called "continuous" spectra. D'Arrest observed the nebulæ G.C. 2930 (84 M Virginis), 2961 (86 M), 3021 (49 M), and Lieutenant Herschel observed G.C. 3021, 3132, 3227, 3229, and 3397. Some of these may be re-examined for bright maxima in the continuous spectra.

(4) The spectrum of this (Group II.) star is thus described by Dunér:—"The bands 2–8 are well marked by strong lines which terminate them on the violet sides. But, with the exception of 2 and 3, they are rather narrow, and the spectrum approaches to the type of Aldebaran." The star is obviously at a transition stage between Groups II. and III., and a special detailed study of the lines and bands should be made.

(5, 6) The spectra of these two stars have been observed by Vogel, who states that the first has a spectrum of the solar type, whilst the second is one of Group IV. The usual further observations are required in each case.

(7) Notwithstanding the small magnitude of this star, it has, according to Vogel, a magnificent spectrum of Group VI. The star is not included in Dunér's Catalogue, and Vogel gives no particulars as to the number and character of the bands present. Further detailed observations are obviously required. The intensity of the carbon band near λ564, as compared with the other bands, should be particularly noted.

(8) This variable will reach a maximum about April 27. Its period is about 225 days, and it varies from 7·2–8·2 at maximum, to 10·2–12·8 at minimum. According to Dunér, the spectrum is one of Group II., but very feebly developed. As no details of the spectrum are given, it seems probable that the observation was made near minimum, and the present maximum may afford an opportunity of securing further observations. As in similar variables, bright lines may also be looked for.

A. FOWLER.

MATHEMATICAL STUDY OF THE SOLAR CORONA.—The Smithsonian Institution, Washington, has published a paper by Prof. Frank H. Bigelow in which the solar corona is discussed by spherical harmonics. The subject is treated by this theory on the supposition that the phenomenon seen is similar to that of free electricity, the rays being lines of force and the coronal matter being discharged from the body of the sun, or arranged and controlled by these forces. In order to give the solution a general foundation the important parts of the theory of harmonics specially relating to the case are recapitulated, and the corresponding geometrical solution given in a notation adapted to the sun. An analysis of the lines of force demonstrates the applicability of the formulæ of statical electricity to the coronal structure, hence some repulsive force must exist on the surface of the sun which acts upon the corona according to the laws of electric potential. It is shown how the concentration of potential at each pole throws vertical lines of force at the polar region, which gradually bend each side, and finally close on the equator at a certain distance from the centre. Similarly other lines are traced which leave the sphere at various angles to the vertical axis and have diminished potentials; these therefore close on the equator at a less distance from the centre than the high potential vertical lines thrown out at the polar region.

Applying these electrical principles to the solar corona, the author thinks that the straight polar rays of high tension carry the lightest substances, such as hydrogen, meteoritic matter, *débris* of comets and other coronal material away from the sun, and they soon become invisible by dispersion. The strong quadrilateral rays which form the appendages conspicuously seen at periods of great solar activity are produced by four lines of force having potential 0·9, 0·8, 0·7, and 0·6, of the potential at each pole, and the explanation of the long equatorial wings, with absence of well-marked quadrilaterals, seen at periods of minimum, is that they are due to the closing of the lines of force about the equator. The theory is tested by applying it to two photographs taken by Messrs. Barnard and Pickering on January 1, 1889, and Prof. Langley submits it to astronomers and physicists as a possible clue to the explanation of the corona and as suggesting the direction to be taken in future observations and investigations.

SOLAR OBSERVATIONS.—The following is the *résumé* of solar observations made at Rome, by Prof. Tacchini, during the first three months of this year:—

Spots and Faculæ.

1890.	No. of days of observation.	Relative frequency		Relative magnitude		Number of spot-groups per day.
		of spots.	of days without spots.	of spots.	of faculæ.	
Jan. ...	20	1·40	0·55	2·35	33·50	0·60
Feb. ...	23	0·13	0·96	0·09	13·26	0·04
Mar. ...	20	1·00	0·70	2·75	25·75	0·30

Prominences.

1890.	No. of days of observation.	Mean number.	Mean height.	Mean extension.
			″	°
Jan. ...	12	1·92	33·6	1·7
Feb. ...	16	1·69	37·8	0·9
Mar. ...	14	2·21	35·5	1·1

ASTRONOMICAL SOCIETY OF FRANCE.—The following officers have been elected for the session 1890-91:—President, M. H. Faye, Member of the Institute. Vice-Presidents: MM. Bouquet de la Grye, Member of the Institute; Camille Flammarion, Laussedat, and Trouvelot, of Meudon Observatory. Secretaries: MM. Ph. Gérigny, Armelin, and Bertaux.

The Society meets at the Hôtel des Sociétés Savants, 28 Rue Serpente, Paris, and there is an Observatory and a Library open to the members.

D'ARREST'S COMET.—The following ephemeris for the search for this periodic comet on its return this year is given by M. G. Leveau in *Astr. Nach.*, No. 2959:—

Ephemeris for Paris Mean Time.

1890.	R.A. h. m.	N.P.D. ° ′	1890.	R.A. h. m.	N.P.D. ° ′
April 26	16 47·4	84 30	June 1	16 31·1	78 3
30	16 47·3	83 39	5	16 27·7	77 43
May 4	16 46·8	82 48	4	16 24·2	77 31
8	16 45·9	81 58	13	16 20·7	77 27
12	16 44·5	81 10	17	16 17·3	77 33
16	16 42·6	80 24	21	16 14·2	77 49
20	16 40·2	79 41	25	16 11·4	78 14
24	16 37·4	79 3	29	16 9·0	78 48
28	16 34·4	78 30			

INFLUENZA AND WEATHER, WITH SPECIAL REFERENCE TO THE RECENT EPIDEMIC.[1]

IN this inquiry the authors deal only with deaths recorded by the Registrar-General as due to, or caused by, influenza in London between the years 1845–90. The statistics for London are selected because there is there a vast population in a small area, all subject to the same climatic conditions, and because there is also there a weekly record of deaths and their causes for a long period, which they discussed with some fulness of detail some years ago.

After making allowance for certain errors to which such an inquiry is liable, arising chiefly from the methods of registration, it is found that the figures recorded disclose certain phenomena with such emphasis that the lessons taught by the phenomena stand altogether unaffected. Thus, as regards the distribution of deaths over the year, during the 45 years, the results show a strongly marked winter maximum and an equally marked summer minimum; along with which there is also a small secondary maximum in the second half of March and first half of April. Thus, broadly considered, the distribution of deaths from influenza is inversely as the temperature, being at the maximum during the winter months when temperature is lowest, and at the minimum in the summer months when temperature is highest. Hence the curve showing the distribution of deaths from influenza is closely congruent with the curve for diseases of the respiratory organs, with the addition of a slight rise in spring, thus suggesting a connection between influenza and diseases of the brain and the nervous system.

During the last 45 years, 4690 deaths are registered as having occurred from influenza, or 104 per annum. There is no year in which there has not been some deaths recorded as due to influenza; but during the 12 years ending with 1889, the registered deaths have been decidedly fewer than during the preceding 33 years, the mean number for these 12 years being only 6½, falling in some of the years as low as 3. There have been five periods during these years in which the figures point to the prevalence of an epidemic of influenza, the exact periods of which, with the number of deaths registered as due to influenza, are these:—

	Deaths.
December 1847 to April 1848	1631
March to May 1851	258
January to March 1855	130
November 1857 to January 1858	123
January to March 1890	545
Total	2687

Thus the five epidemics yielded 2687 of the 4690 deaths registered, or about 57 per cent. From a discussion of the details of each epidemic and the weather which prevailed duri each of them, it was shown that in each case the rise to th maximum was strikingly rapid after the disease was recognized as existing. It was further concluded that the epidemics of influenza in this country were not, though they occurred during the winter, connected with exceptionally cold weather, especiall at their commencement, but on the contrary rather with e ceptionally warm weather, which manifested itself generai both before and during the epidemic. In no case that has occurred was any exceptionally cold weather intercalated in the period of the epidemic, accompanied with an increase of deaths from influenza, or even with an arresting of the downward course of the curve of mortality, if the cold occurred at the time the epidemic was on the wane. This fact presents influenza under widely different relations to temperature as compared with all diseases of the respiratory organs.

During the first four weeks of 1890, when the mortality from influenza was at the maximum, the total mortality from all causes was 2258 above the average of these weeks, and of this number influenza only accounted for 303, thus leaving 1955 deaths due to other causes; and it is here to be noted that during the time there were no weather conditions, such as excessively low temperature or dense persistent fogs, which could account for this very large increase of the death-rate. It thus became a point of interest to ascertain what the diseases were which h an exceptionally high mortality during the period, and on other hand whether there had been any diseases with a morta for the time much under the average.

The statistics from the various diseases were minutely amined, from which it was shown that those which yielded exceptionally high death-rate during the influenza epidemic w diseases of the respiratory organs, phthisis, diseases of circulatory system, rheumatism, and diseases of the ner system. These diseases, particularly those of the respira organs, produced a very large excess above their averages, spite of the fact that on the whole temperature had been p ticularly high, and dense fogs absent, which, being contrary all rule, plainly indicated that during the period something an exceptional character had been operating to increase t deaths from diseases of the respiratory organs. The stro manifestation of nervous symptoms in the severe headaches a prostration which attended the attacks of influenza, make t increase of deaths from diseases of the nervous system and phthisis deeply interesting, as suggestive of a relation to secondary spring maximum. So, also, the increased nur of deaths from rheumatism is interesting in connection w the muscular pains which were so constant a symptom influenza.

The diseases which yielded a mortality under the aver during the prevalence of the epidemic were diarrhœa a dysentery, liver disease, measles, scarlet fever, typhoid fev and erysipelas. It is, however, necessary to remark that figures refer only to London, and that in other places wh epidemics of measles and scarlet fever prevailed at the ti these epidemics might show a mortality above the average.

On the question of age, the point of interest centred in fact that the death-rate of all persons above the age of rose considerably above the average during the four or f weeks immediately preceding the commencement of the reg tration of deaths due to the epidemic. Thus, though dea from influenza were not registered in November and Deceml there appeared to have been something then present, apart fr weather, which increased the mortality of all persons above t age of 20 much above the mean. At ages under 20 yea the death-rate rose above the mean only in the first three wee of the year.

From a list of twenty-three recorded epidemics of influer since the year 1510, it appeared that spring epidemics were m frequent and better marked than they would be if the figures the past forty-five years were accepted as revealing the w truth; and it also appeared that the epidemic of influenza occurred in early summer and continued to the end of J Facts, however, are too scanty to show whether the incre mortality during this early summer epidemic extended to classes of diseases which have their annual maximum mortali early summer, in a manner similar to the greatly incre mortality from diseases of the respiratory organs or of nervous system according as the epidemic falls during the wi or the spring months.

In conclusion it was remarked that in discussions regardi

[1] Abstract of a Paper, by Sir Arthur Mitchell and Dr. Buchan, read at the half-yearly meeting of the Scottish Meteorological Society, March 31, 1890.

[th]e spread of the germs of diseases from one country to another [b]y the intervention of winds, it had been perhaps universally assumed that it is only the winds blowing over or near the surface of the earth which were concerned in the dissemination of these germs. Generally it has been concluded that, if the surface [wi]nds do not account for the successive appearances of the [ep]idemic at different points, the germs have not been transported [b]y the winds. This, however, is only a mode of looking at the subject which ignores the recent developments of meteorology and its teachings regarding atmospheric circulation through cyclones and anticyclones. As is now virtually proved, the winds in a cyclone are drawn inwards towards its centre, and thence ascend in a vast aërial column to the upper regions of the atmosphere, whence again they flow as an upper current towards any anticyclone or anticyclones that may be in the surrounding region. Thereafter they slowly descend down the centre of the anticyclone to the earth's surface, over which they are carried in every direction. Thus, for example, from a cyclone in Russia, a vast column of air rises from the surface, carrying with it particles of dust, germs, and other light impurities. These are then conveyed by the upper current to the anticyclone that may at the time overspread Western Europe, and thereafter descend to the surface, and are then distributed over Western and Central Europe by winds from all points of the compass. Owing to the [ra]pidity of these aërial movements, two or at most three days are [am]ply sufficient for this distribution.

MATHEMATICAL TEACHING AT THE SORBONNE, 1809–1889.

[T]HE following brief sketch of the illustrious Professors who have during the last eighty years occupied the mathe[ma]tical chairs at the Sorbonne is founded upon an interest[in]g address by the veteran mathematician, M. Ch. Hermite.[1]

The occupants, in 1809, of the respective chairs, were Lacroix [(]Differential and Integral Calculus), Poisson (Mechanics), Biot [(]Astronomy), Francœur (the Higher Algebra), and Hachette [(]Descriptive Geometry). Each, in his respective department, [h]as left traces of his power which are still in evidence. "Nous [é]voquons le souvenir de ces hommes éminents qui ont honoré [la] Faculté des Sciences à son origine; nous voulons rendre [h]ommage qui est dû à leur mémoire, et dans cette circonstance [ra]ppeler leurs titres à la reconnaissance du pays." M. Hermite [the]n proceeds to analyze in turn the work of the above Pro[fes]sors.

(1) Of Lacroix, he says: "La constante préoccupation de [l']auteur a été d'établir entre tant de théories qu'il expose, sur [d]es matières si diverses, une succession naturelle, un enchaîne[m]ent qui en facilite l'étude et contribue à l'intelligence générale [d]e l'analyse." He was well followed by Lefébure de Fourcy.

(2) Francœur occupied his chair down to 1847; he was the [a]uthor of a long list of works. "La concision que s'est imposée [l']auteur pour réunir tant de matières dans un court espace ne [p]orte jamais atteinte à la clarté." A sketch of the "Urano[g]raphie" is furnished by M. Tisserand.

(3) Biot was also a long occupant of his chair, "dont il est [r]esté titulaire jusqu'en 1846." M. Wolf furnishes a note (pp. [3]6–40) which gives a full account of the "Traité Élémentaire [d]'Astronomie physique." "Biot était un érudit et un écrivain," [i]n M. Hermite's judgment.

(4) Poisson is a Colossus:—"Il figure parmi eux à côté de Laplace, de Lagrange, et de Fourier. C'est surtout de l'auteur de la 'Mécanique Céleste' qu'il se rapproche par la nature de ses travaux, son génie analytique, sa puissance pour mettre en œuvre toutes les ressources du calcul. Lagrange, à qui l'on doit la 'Mécanique Analytique,' et de grandes découvertes dans la [t]héorie du son et la mécanique céleste, avait consacré une part [im]portante de ses efforts aux mathématiques abstraites; après [a]voir fondé le calcul des variations, il a laissé la trace de son [g]énie dans l'algèbre et la théorie des nombres. Pour Laplace et [P]oisson, l'analyse pure n'est point le but, mais l'instrument; les [a]pplications aux phénomènes physiques sont leur objet essentiel, [et] Fourier, en annonçant à l'Académie des Sciences les travaux [d]e Jacobi, a exprimé le sentiment qui dominait à son époque, dans ces termes que nous reproduisons: 'Les questions de la philosophie naturelle qui ont pour but l'étude mathématique de tous les grands phénomènes sont aussi un digne et principal objet des méditations des géomètres. On doit désirer que les personnes les plus propres à perfectionner la science du calcul dirigent leur travaux vers ces hautes applications, si nécessaires aux progrès de l'intelligence humaine.' Mais, en ayant un autre but, Poisson et Fourier contribuent au développement de l'analyse, qu'ils enrichissent de méthodes, de résultats nouveaux, de notions fondamentelles. Nous allons essayer de montrer l'importance des découvertes de Poisson dans la domaine de la physique mathématique, en jétant un coup d'œil rapide sur quelques-uns de ses mémoires."

(5) Poisson was succeeded by Sturm, whose reputation is founded upon his well-known theorem in the theory of equations. M. Hermite alludes to Prof. Sylvester's discovery in this branch.

(6) In 1838, a Chair of Mécanique Physique et Expérimentale was founded, of which the first occupant was the illustrious Poncelet. Commencing with an account of the "Traité des Propriétés Projectives des Figures," the writer goes on to describe the other contributions of this eminent mathematician, who was succeeded (7) in 1851 by Delaunay. Here, again, M. Tisserand comes to the help of his colleague with an account of Delaunay's astronomical work.

(8) A short and highly appreciative account follows of Le Verrier. "Il a été donné à l'illustre auteur de ne point laisser son œuvre inachevée; Le Verrier a corrigé sur son lit de mort les dernières feuilles de la théorie de Neptune, léguant à l'astronomie un monument impérissable qui sera l'honneur de son nom et de la science de notre pays."

(9) The various works of Lamé come next under review. "Lamé est un des plus beaux génies mathématiques de notre temps. Des découvertes capitales qui ont ouvert de nouvelles voies dans la théorie de la chaleur, la théorie de l'élasticité, l'analyse générale, le placent au nombre des grands géomètres dont la trace reste à jamais dans la science."

(10) Liouville; (11) Serret; and (12) Duhamel are rapidly examined, the notice of this last being contributed by M. Bertrand.

(13) "Chasles est l'une des plus grandes illustrations de la Faculté; ses découvertes en géométrie, les ouvrages qu'il a publiés sur cette science l'ont placé au premier rang parmi les savants de l'Europe, et rendu son nom à jamais célèbre. De grandes et belles découvertes en mécanique se sont ajoutées à son œuvre principale, ainsi que des recherches d'érudition sur les mathématiques et l'astronomie des Indiens et des Arabes; nous indiquerons succinctement ces travaux qui ont jeté tant d'éclat, et sont présents à toutes les mémoires." The notice closes with the following touching sentence: "il nous reste à dire que ses amis et tous ceux qui ont connu notre cher et vénéré collègue gardent l'inaltérable souvenir de la bonté qui, chez le grand géomètre, était la compagne du génie."

(14) Cauchy is also treated at some length. "La vie du grand géomètre, remplie par des découvertes immortelles qui sont l'honneur de la science française, l'a été aussi par les œuvres de la charité chrétienne et une inépuisable bienfaisance."

(15), (16), and (17). In a few words are summed up the principal results obtained by other colleagues: "Nos collègues Puiseux, Briot, et Bouquet, morts il y a peu d'années, et dont nous gardons si affectueusement le souvenir, se sont inspirés de son génie, et ont consacré des travaux de premier ordre à poursuivre dans le domaine de l'analyse les conséquences de ses découvertes."

The speaker had a grand theme, and perhaps does not exalt too highly the very distinguished mathematicians who have preceded, or been associated with, him in his labours at the Sorbonne. One can pardon an occasional high-flown expression of his admiration for them and for their achievements: to ourselves the perusal of his discourse has furnished much pleasure, and we trust there will be as distinguished a roll of Professors to be celebrated when the work of the new Sorbonne has to be narrated by M. Hermite's successor. We conclude with the closing words of the address:—

"Nous venons d'évoquer le souvenir de nos prédécesseurs, nous avons voulu rendre hommage à leur mémoire, rappeler leurs travaux, leurs découvertes, les grands exemples qu'ils nous ont laissés. Notre mission est de continuer leur œuvre, et d'ajouter à leur glorieux héritage; ce devoir nous est rendu plus sacré par le don magnifique que nous tenons du pays, par sa généreuse assistance pour notre enseignement et nos travaux. Tous, maîtres

[1] "Discours prononcé devant le Président de la République, le 5 Août, à l'inauguration de la nouvelle Sorbonne, par M. Ch. Hermite, Professeur à la Faculté des Sciences, Membre de l'Institut," *Bulletin des Sciences Mathématiques*, January 1890 (pp. 6–36). (Paris: Gauthier-Villars.)

de conférences et professeurs, nous y consacrerons notre dévouement, nos efforts : nous avons la confiance que, pour l'honneur de la Science et de la France, nous saurons fidèlement le remplir."

SCIENTIFIC SERIALS.

The American Journal of Science, April 1890.—On the æolian sandstones of Fernando de Noronha, by John C. Branner. These sandstones lie upon the eastern or south-eastern sides of the island, at an elevation of 70 feet on Ilha do Meio, 90 feet on São José, and about 100 feet on the Ilha Rapta, and at the base of Atalaia Grande. The author has closely investigated the formation, and finds that the material was originally deposited in the form of sand-dunes blown up by winds from the south or south-east. Analyses of several specimens of the rock are given.—A mountain study of the spectrum of aqueous vapour, by Charles S. Cook. The author has devised a means of producing an artificial line whose intensity can be varied at will alongside the line whose intensity is required. The variations in the blackness of the artificial line are effected by the use of a micrometer screw, the readings of which constitute an arbitrary value of intensities. It is found, (1) that the spectroscope studies vapour height primarily, and humidity only secondarily; (2) during stormy weather vapour ascends to altitudes greater than is usually supposed; (3) the great absorption of storm clouds is due to their great thickness, or to extensive strata of damp air associated with them, more than to any peculiar behaviour as clouds.—On the occurrence of basalt dykes in the Upper Palæozoic series in Central Appalachian Virginia, by Nelson H. Darton; with notes on the petrography, by J. S. Diller.—Additional notes on the tryolite from Utah, by W. F. Hillebrand and E. S. Dana. The composition and crystalline form of this mineral are considered.—W. S. Bayley, on the origin of the soda-granite and quartz-keratophyre of Pigeon Point, Minnesota. These rocks have been previously described by the author (*Amer. Journ.*, January 1889). In the present note the reasons are pointed out which lead to the conclusion that the red rock is of contact origin, and produced by the action of the gabbro upon the slate and quartzites.—Frank Waldo, in recent contributions to dynamical meteorology, gives a general idea of the nature of each of fourteen papers on meteorology; most of the papers being by German physicists. The attitude of the writers towards meteorology is also indicated by reference to other work done in the same direction.—Two methods for the direct determination of chlorine in mixtures of alkaline chlorides and iodides, by F. A. Gooch and F. W. Mar.—On the occurrence of polycrase, or of an allied species, in both North and South Carolina, by W. E. Hidden and J. R. Mackintosh. The analyses, so far as they go, show that a mineral previously noticed (*Amer. Journ.*, November 1888) is very closely allied to, if not identical with, the polycrase from Hitteroe, Norway, analyzed by Rammelsberg.—Origin of some topographic features of Central Texas, by Ralph S. Tarr.—On the formation of silver silicate, by J. Dawson Hawkins. A simple method for the preparation of this compound is described. The reaction made use of is $Na_2SiO_3 + 2AgNO_3 = Ag_2SiO_3 + 2NaNO_3$.

SOCIETIES AND ACADEMIES

LONDON.

Royal Society, April 17.—"Preliminary Note on Supplementary Magnetic Surveys of Special Districts in the British Isles." By A. W. Rücker, M.A., F.R.S., and T. E. Thorpe, Ph.D., B.Sc. (Vict.), F.R.S.

During the summer of 1889 we carried out additional magnetic surveys of the Western Isles and the West Coast of Scotland, and of a tract of country in Yorkshire and Lincolnshire.

Both districts were selected with special objects in view. We had found that powerful horizontal disturbing forces acted westwards from the Sound of Islay, from Iona, and from Tiree, and we had deduced a similar direction for the disturbing force at Glenmorven from Mr. Welsh's survey of Scotland in 1857-58. The whole district presents peculiar difficulties, partly from the fact that local disturbance is likely to mask the effects of the regional forces, partly because the normal values of the elements must be especially uncertain at stations on the edge of the area of our survey.

If, then, the general westward tendency of the horizontal disturbing forces was due to some source of error, stations in the extreme south of the Hebrides would in all probability be similarly affected. If the directions of the forces were due to a physical cause, such as a centre of attraction out at sea to the west of Tiree, then the disturbing forces in the Southern Hebrides would almost certainly be directed southwards towards it.

The observations made last summer prove (1) that the direction of the disturbing horizontal force at Bernera, which is the southernmost island of the Hebridean group, is due south; and (2) that, as this point is approached from the north, the downward vertical disturbing attraction on the north pole of the needle regularly increases, which exactly agrees with the supposition that a centre of attraction is being approached.

There is, therefore, now no doubt that there is a centre of attraction on the north pole of the needle to the south of the Hebrides and to the west of Tiree.

(2) In one of the maps communicated to the Society last year we drew two lines, bounding a district about 150 miles long and 40 miles broad, in Yorkshire and Lincolnshire, and gave reasons for the belief that a ridge line or locus of attraction lay between them.

This conclusion has now been tested by means of thirty-five additional stations, with the following results:—

(1) At all stations (with one exception) on or near the two lines, the horizontal disturbing forces tend towards the centre of the district they bound.

(2) The downward vertical disturbing forces are greater in the centre of the district than at its boundaries. In particular, there are two well-marked regions of very high vertical force.

(3) The greatest vertical force disturbances occur at Market Weighton, where the older sedimentary rocks are known to approach the surface, and at Harrogate, which is on the apex of an anticlinal.

(4) The central ridge line runs from the Wash parallel to the line of the Wolds to Brigg. Thence it appears to turn west, and reaches Market Weighton *viâ* Butterwick and Howden. One or two additional stations are, however, required to determine whether this bend is real, or whether the line runs direct from Brigg to Market Weighton. From the latter town it passes to the limestone district of Yorkshire and traverses its centre. It has not yet been traced west of the line of the Midland Railway between Settle and Hawes, but there is ground for believing that it continues to the Lake District.

Although, therefore, one or two points of detail remain for further investigation, the existence of a line of attraction 150 miles long is proved beyond the possibility of doubt, and for about 90 miles its position is known to within 5 miles.

There are, then, even in those parts of England where the superficial strata are not magnetic, regions of high vertical force comparable in size with small counties, and ridge lines or loci of attraction as long and almost as clearly defined as the rivers. Their course is closely connected with the geology of the districts through which they run.

Royal Meterological Society, April 16.—Mr. Baldwin Latham, President, in the chair.—The following papers were read:—The cold period at the beginning of March 1890, by Mr. C. Harding. At the commencement of the month a rather heavy fall of snow was experienced in many parts of England, and very cold weather set in over the midland, eastern, and southern districts, the temperature on the 3rd and 4th falling to a lower point than at any time in the previous winter. The lowest authentic thermometer readings, in approved screens, were 5° at Beddington, 6° at Kenley in Surrey and Hillington in Norfolk, 7° at Chelmsford and Beckenham, 8° at Addiscombe, 9° at Reigate and Brockham, and 10° in many parts of Kent and Surrey. At Greenwich Observatory the thermometer registered 13°, which has only once been equalled in March during the last 100 years, the same reading having occurred on March 14, 1845. During the last half-century the temperature in March has only previously fallen below 20° in three years, whilst during the whole winter so low a temperature has only occurred in eight years.—Note on the whirlwind which occurred at Fulford, near York, March 8, 1890, by Mr. J. E. Clark. A sharp and heavy thunderstorm occurred at York about 2.30 p.m. At the same time, or shortly afterwards, a whirlwind passed a little to the south of the city, from Bishopthorpe to Heslington, a distance of about

iles, its width varying from 3 or 4 to 250 yards. The author de a careful survey of the track of the whirlwind, and de- ibed the damage done by it to trees, buildings, &c.—On the ssibility of forecasting the weather by means of monthly erages, by Mr. A. E. Watson. The author is of opinion that the average values of meteorological phenomena are constant quantities, and that any variation from them is sure to be met by ompensating variation in the opposite direction.

Zoological Society, April 15.—Mr. G. A. Boulenger, in e chair.—Mr. A. Smith-Woodward, read a paper on some new hes from the English Wealden and Purbeck Beds, referable to e genera *Oligopleurus*, *Strobilodus*, and *Mesodon*. Detailed descriptions of several fossils of these genera, now in the British Museum, were given. *Oligopleurus* was stated to be represented by a single species in the Wealden of the Isle of Wight, occurring also in the Purbeck of Dorsetshire; and the latter formation had yielded at least one species both of *Strobilodus* and *Mesodon*. Previous researches had already indicated a close connection between the fish-fauna of the English Purbeck Beds and that of e Upper Jurassic Lithographic Stones of France, Bavaria, and rtemberg; and the new forms now described tended to demon- e that alliance even more clearly.—Mr. G. A. Boulenger r the second of a series of reports on the additions to the Batrachian Collection in the Natural History Museum. Since 1886, when the first report was made on this subject, examples of 71 additional species of Batrachians had been acquired. Amongst these was a remarkable new form allied to the family Engystomatidæ, proposed to be called *Genyophryne thomsoni*, based on a single specimen obtained by Mr. Basil Thomson on Sudest Island, near South-East New Guinea. The form was stated to be unique in having teeth in the lower, but none in the upper jaw.—Mr. Frank E. Beddard read a paper on the structure of *Psophia*, and on its relations to other birds. The author was inclined to consider *Psophia* most nearly allied to *Cariama* and *Chunga*, and more distantly to *Rhinochetus*, but entitled to stand as a distinct family in the group of Cranes and their allies.—Mr. Henry Seebohm gave an account of a collection of birds from the northern part of the province of Fokien, South-Eastern China. Several interesting species were represented in the series, amongst which was a new *Hemixus*, proposed to be called *H. canipennis*.

Linnean Society, April 3.—Mr. Carruthers, F.R.S, Presi- nt, in the chair.—Prof. P. Martin Duncan exhibited a trans- se section of coral, *Caryophyllia clavus*, showing septa and ular theca between them.—Mr. B. D. Jackson exhibited nd seeds of *Mystacidium filicornu*, an epiphytic Orchid for- rded from South Africa by Mr. Henry Hutton, of Kimberly.—A paper by Prof. W. H. Parker, on the morphology of the *Gallinaceæ*, in the unavoidable absence of the author was read by Mr. W. P. Sladen; and a discussion followed, in which Dr. St. George Mivart, Prof. Duncan, and Mr. J. E. Harting took part.

PARIS.

Academy of Sciences, April 14.—M. Hermite, President, he chair.—On the theory of the optical system formed by a escope and a plane mirror movable about an axis, by MM. wy and Puiseux. One of the problems studied is to deter- e the exact co-ordinates of a star with a telescope and a ne mirror placed in front of the object-glass.—On the elements peritoneal serum, by M. L. Ranvier. The humour was ained from the domestic rabbit, the rat (*Mus decumanus*), and the cat. Microscopical examination of the preparations showed the presence of red globules of blood (hæmatics) whatever pre- cautions were taken. It is therefore considered as a normal element, physiological, not accidental, of peritoneal serum. lourless spherical lymphatic cells, having dimensions from 20μ 00μ, are also described; the volume, structure, and re- ons of these cells from the three animals, however, is found ury.—On the artificial production of silk, by M. Emile Blan- d.—*Résumé* of solar observations made at the Royal Obser- y of the College of Rome during the first three months of year 1890, by M. P. Tacchini.—Observations of sun-spots in 1889 at the Lyons Observatory, by M. Em. Marchand. first three months of this year are also included in the list. les are given showing the number of days without spots, the tion and latitude of spots, and their mean total surface (umbra penumbra) expressed in millionths of the sun's visible surface. pproximate rectification of an arc of a curve, by M. A. E. et.—Construction for the radius of curvature of symmetrical triangular curves, of plane anharmonic curves, and of asymptotic lines of Steiner's surface, by M. G. Fouret.—A paper by M. A. Ditte, on the action of nitric acid on aluminium, shows that this acid acts upon aluminium in much the same way as sulphuric acid. The slowness of the reaction is due to the formation of a protecting covering of gas. As in the case of zinc, when weak nitric acid is employed the gases produced consist of nitric oxide and nitrogen, together with some ammonia; with 3 per cent. acid in presence of a little platinum chloride, ammonia is almost the sole product. Just as with the sulphate, the nitrate forms with aluminium in presence of water a basic nitrate with liberation of hydrogen.—On the preparation of hydrobromic acid, by M. A. Recoura. The author passes a stream of H_2S through bromine, and washes the gaseous HBr produced by passing it through a solution of HBr containing a little red phosphorus in suspension. The method admits of the production of gaseous HBr at any desired rate, and without the necessity of the continual watching required by the methods formerly employed.—On the oxidation of hypophosphorous acid by hydrogenized palladium in the absence of oxygen, by M. R. Engel. In the precipitation of palladium by hypophosphorous acid according to the method followed by Wurtz and Graham, the author finds that the product, contrary to the statements of those investigators, contains hydrogen. The spongy palladium produced decomposes an unlimited quantity of phosphorous acid, hydrogen being evolved.—M. P. Cazeneuve contributes a paper on the oxidizing and decolorizing properties of charcoal.—M. E. Jungfleisch, in a note on camphoric acids, shows that the separation of several acids is possible when advantage is taken of their differing solubilities.—A note on the acid malonate, the quadromalonate, and the quadroxalate of potassium, by M. G. Massol, gives the thermal properties of these salts, and an analysis of the quadromalonate.—M. L. Lindet describes a method for the extraction of raffinose from molasses, and for the separation of raffinose from saccharose, the separation depending upon the greater solubility of raffinose in absolute methyl alcohol, and its much inferior solubility in 80 per cent. ethyl alcohol, as compared with the solubility in each medium of saccharose.—On a pseudo-typhoid bacillus found in river water by M. Cassedebat. The author has found in Marseilles drinking-water a bacillus having a great resemblance to that of typhoid fever. The investigations, so far as they have gone, seem to fully establish the identity of the two bacilli.—On the microbes of hæmoglobinuria of the bull, by M. V. Babes. An examination of the character of this organism shows that it has no well-established place in the classification of microbes, and that the conditions of culture are not yet well determined. Nevertheless, its special reactions, its localization in the red globules, and its transmissibility to animals, leave no room for doubt as to its pathological significance.—Nutrition in hysteria, by MM. Gilles de la Tourette and H. Cathelineau. It is noted that in hysteria, notwithstanding nervous pathological manifestations other than permanent affections, nutrition is effected normally.—On operation for strabismus without tenotomy, by M. H. Parinaud.—On the function of air in the physiological mechanism of hatching, sloughing, and metamorphosis among Orthopterous insects of the family Acridides, by M. J. Kunckel d'Herculais.—On a new Lycopodium of the Coal-measures (*Lycopodiopsis Derbyi*), by M. B. Renault.—Pebble impressions, by M. Ch. Contejean. The paper refers to Tertiary pudding-stones found near Montbéliard.

BERLIN.

Physiological Society, March 28.—Prof. du Bois-Reymond, President, in the chair.—Prof. Salkowski spoke on fermentative processes which occur in animal tissues, employing chloroform-water to discriminate between the action of ferments (organized) and enzymes (unorganized). He had thus found that a fermentation (zymolysis) occurs in yeast-cells, by which their cellulose is partly converted into a lævo-rotatory sugar and the nuclein into substances of the xanthin series. He had further isolated from yeast-cells, apart from their cellulose, two other carbohydrates, one belonging to the gum series and one resembling glycogen; either of these might have been the source of the above-mentioned sugar. In a similar way he had studied the fermentative changes which take place in liver and muscle, and found them to yield a series of distinct products which could be determined both qualitatively and quantitatively. He concluded from his researches that fermentative (zymolytic) processes are continually taking place in living tissues, and play a most

important part in the chemistry of their metabolism.—Dr. Rosenberg demonstrated a new reaction of uric acid. When urine is made faintly alkaline, it yields a dark blue colouration on the addition of phosphotungstic acid, which he had satisfied himself was due to the presence of uric acid alone among the other constituents of the excretion.—Dr. Goldscheider gave an account of some experiments which he had made some five years ago, to show that the principle of "specific nerve energy" holds good for the sense of taste. By isolated stimulation of separate taste-papillæ he succeeded in showing that there exist, in all, four kinds or qualities of taste—sour, sweet, bitter, and salt; and that specific end-organs exist for each kind of taste. By electrical stimulation there arises at the anode not only the sensation of sour, but also of bitter and sweet; at the kathode purely sensory impulses are aroused in addition to the gustatory, and to the fusion of these two is due the "alkaline" taste of which some authors speak. It appeared from his researches that the hard palate contained end-organs chiefly for the perception of sweet tastes.—Dr. I. Munk spoke on muscular work and nitrogenous metabolism. He criticized the recent work of Argutinsky, according to which the work done in climbing a mountain, and the heat produced, are the outcome of a breaking down of nitrogenous material. Having recalculated Argutinsky's results, he came to the conclusion that (1) his body was not in nitrogenous equilibrium even during rest; (2) the amount of carbohydrate which he took was insufficient to account for the heat-production during rest. As is well known, both these factors lead to an increased nitrogenous metabolism when extra work is done, the energy required for the excess of work being obtained from the breaking down of proteids; hence no conclusions as to what normally takes place can be drawn from Argutinsky's experiments. He further pointed out that Oppenheim's experiments have shown that dyspnœa leads to increased nitrogenous metabolism, and that hence dyspnœa may very probably have played some part during the exertion of excessive climbing. While not doubting the accuracy of the experiments, he did not feel that the conclusions which Argutinsky had drawn from them were justifiable.

GÖTTINGEN.

Royal Society of Sciences, Oct. 15, 1889.—On the granular pigments occurring in man, by Dr. F. Maas. Two chemically distinct groups of pigments occur: (1) melanin, (2) the granular colouring matters here referred to. The latter are found at all periods of life, but increase in quantity and in the size of the granules with age. They are normal products, not morbid. They are not only transformed but produced by the corpuscle-carrying cells. They are not wholly derived from the blood; the pigment found in the heart is derived from a fatty body. The several pigments can be distinguished by their reactions with hydrochloric and acetic acids, and with caustic potash.—On the analogue of Kummer's surface for $p = 3$, by W. Wirtinger. The author investigates the continuum obtained by taking, as the eight homogeneous point-co-ordinates of a 7-dimension space, eight linearly independent squares of theta-functions of three variables. It appears that this possesses collineations analogous to the system for Kummer's surface, as also the corresponding system of reciprocal transformations into itself.

October 23, 1889.—Determination of the elastic constants of Iceland spar, by W. Voigt. The author uses the refraction observations of G. Baumgarten, and gives elaborate tables of his own measurements. He discusses the property of spar by which the crystal can be forced by shearing into its twin form, and gives diagrams illustrating the changes in the traction and torsion coefficients.—Determination of the elastic constants of certain dense minerals, by W. Voigt and P. Drude. The minerals are dense fluor spar, Solenhofen stone, and dense barytes.

December 3, 1889.—On thermo-electric currents in crystals, by Th. Liebisch. The author confirms some of Backstrom's results, and finds that, in a rectangular parallelepiped of homogeneous conducting crystal of the triclinic system, embedded in homogeneous isotropic "normal" metal, "the thermo-electric force in the direction of the steepest temperature gradient is represented by the squared reciprocal of the parallel radius vector of a certain ellipsoid E."—On contrast-phenomena resulting from suspended attention, by Dr. F. Schumann. Psycho-physical experiments on the estimation of short periods of time, &c.

December 25, 1889.—On the fertilization of the ova of *Agelastica alni*, L., by Dr. H. Henking. In this insect it is observed that in ova taken from the oviducts a number of spermatozoa penetrate deeply among the yolk-masses as far as the level of the female pronucleus. Peculiar karyokinetic appearances are described.—Contribution to the theory of the even Abelian sigma-function of three arguments, by Ernst Pascal. This is a continuation of the author's previous work on the odd sigma-function. The terms of the developments are combinants of a net of quaternary quadratic forms.—On a hyperelliptic multiplication equation, by H. Burkhardt. This equation for hyperelliptic functions ($p = 2$) is the generalisation of Jacobi's equation for elliptic functions.

AMSTERDAM.

Royal Academy of Sciences, March 29.—Prof. van der Waals, Vice-President, in the chair.—M. H. A. Lorentz dealt with the molecular theory of diluted solutions. He showed how the known formula for the vapour-pressure of such solutions may be derived from considerations on molecular motion and attraction, and how a similar theory applies to a conceivable mechanism of osmotic pressure.—M. Baehr gave some observations on the herpolhodie of Poinsot, and explained that this cannot have any points of inflexion, unless the ellipsoid be not a central one.—M. Pekelharing spoke of "the destruction of anthrax spores by rabbits' blood."

STOCKHOLM.

Royal Academy of Sciences, April 9.—On the researches in zoology made at the Zoological Station of the Academy during 1889, by Prof. S. Lovén.—On the possibility of the triangulation of Spitzbergen, by Prof. Rosén.—An analysis of the liquid inclosures in topaz, or the so-called Brewsterlinite, by Otto Nordenskiold.—On the use of invariants and seminvariants for the solution of common algebraic equations of the four lowest degrees, by Dr. A. Bergen.—On the structure of the fruit-wall in the Labiatæ, by Miss A. Olbers.—Some researches on accidental double refraction of gelatinous substances, by Dr. G. Bjerkén.—On the action of iodohydric acid on 1·5 nitronaphthalin-sulphon-acid-amid, by A. Ekbom.

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